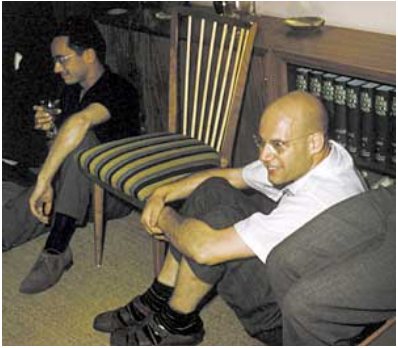
Grothendieck's own idea of "topos theory and étale cohomology."

Born during Serre's talk April 21, 1958.



Serre and Grothendieck, 1958

(Plus "Deligne has proved a beautiful result" and "a little trouble with universes.")

André Weil

A. Utterly unified view of mathematics.

B. Utter disdain for “purity of method.”

C. The architect (not sole founder) of
20th century geometrized arithmetic.

The Weil conjectures 1949

-

The Weil conjectures 1949

N_s counts solutions to a given Diophantine equation over degree s extension of finite field $\mathbf{F}_q$. Generated by a Zeta function:

$$Z(t) \; = \; \exp\,(\sum_{s=1}^{\infty} N_s \frac{t^s}{s}) = \; \frac{P_1(t)P_3(t)\cdots P_{2n-1}(t)}{P_0(t)P_2(t)\cdots P_{2n}(t)}$$

The Weil conjectures 1949

N_s counts solutions to a given Diophantine equation over degree s extension of finite field $\mathbf{F}_q$. Generated by a Zeta function:

$$Z(t) \; = \; \exp\,(\sum_{s=1}^{\infty} N_s \frac{t^s}{s}) = \frac{P_1(t)P_3(t)\cdots P_{2n-1}(t)}{P_0(t)P_2(t)\cdots P_{2n}(t)}$$

Bravura Eulerian analysis, Hilbert-Zariski bi-rational geometry, Betti-Lefschetz topology – to stunning arithmetic effect.

The Weil conjectures 1949

N_s counts solutions to a given Diophantine equation over degree s extension of finite field $\mathbf{F}_q$. Generated by a Zeta function:

$$Z(t) \;=\; \exp\,(\sum_{s=1}^{\infty} N_s \frac{t^s}{s}) = \; \frac{P_1(t)P_3(t)\cdots P_{2n-1}(t)}{P_0(t)P_2(t)\cdots P_{2n}(t)}$$

Bravura Eulerian analysis, Hilbert-Zariski bi-rational geometry, Betti-Lefschetz topology – to stunning arithmetic effect.

Ties deep arithmetic to the Betti numbers of complex manifolds.

The Weil conjectures 1949

N_s counts solutions to a given Diophantine equation over degree s extension of finite field $\mathbf{F}_q$. Generated by a Zeta function:

$$Z(t) \; = \; \exp\,(\sum_{s=1}^{\infty} N_s \frac{t^s}{s}) = \; \frac{P_1(t)P_3(t)\cdots P_{2n-1}(t)}{P_0(t)P_2(t)\cdots P_{2n}(t)}$$

Bravura Eulerian analysis, Hilbert-Zariski bi-rational geometry, Betti-Lefschetz topology – to stunning arithmetic effect.

Ties deep arithmetic to the Betti numbers of complex manifolds.

Suggests proving it by a “fixed point theorem” for Galois actions in cohomology of varieties over finite fields.

Too beautiful not to be true.

Weil knew the conjectures as such were one of his greatest achievements.

Too beautiful not to be true.

Weil knew the conjectures as such were one of his greatest achievements. It is true.

Too beautiful not to be true.

Weil knew the conjectures as such were one of his greatest achievements. It is true.

Proved stunning special cases.

Too beautiful not to be true.

Weil knew the conjectures as such were one of his greatest achievements. It is true.

Proved stunning special cases.

But cohomology of varieties over finite fields made no concrete sense.

Too beautiful not to be true.

Weil knew the conjectures as such were one of his greatest achievements. It is true.

Proved stunning special cases.

But cohomology of varieties over finite fields made no concrete sense.

Lefschetz theorem relies on continuity of $\mathbb{R}$, Impossible for unordered, countable $\overline{\mathbb{F}_q}$.

Too beautiful not to be true.

Weil knew the conjectures as such were one of his greatest achievements. It is true.

Proved stunning special cases.

But cohomology of varieties over finite fields made no concrete sense.

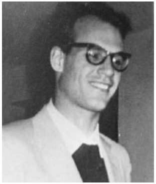
Lefschetz theorem relies on continuity of $\mathbb{R}$, Impossible for unordered, countable $\overline{\mathbb{F}_q}$.

Weil did not believe such cohomology was possible. It was an analogy, not a method.

Grothendieck 1985:

Grothendieck 1985:

> *Serre explained the Weil conjectures to me in cohomological terms around 1955 and only in these terms could they possibly "'hook" me. No one had any idea how to define such a cohomology and I am not sure anyone but Serre and I, not even Weil if that is possible, was deeply convinced such a thing must exist. [R& S, p. 840]*



AG

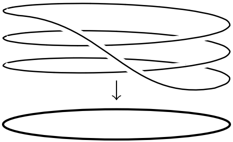
J-P.

Serre and Grothendieck defined cohomology of a topological space X either by *fiber bundles* or by *sheaves* on X.

Serre and Grothendieck defined cohomology of a topological space X either by *fiber bundles* or by *sheaves* on X.

Two kinds of covering space for X, which Grothendieck would redefine.

Sheaf covering S^1.



Monday April 21, 1958 Alexander Grothendieck went to the Séminaire Henri Cartan

Monday April 21, 1958 Alexander Grothendieck went to the Séminaire Henri Cartan

to hear Jean-Pierre Serre's new ground breaking definition of 1-dimensional cohomology groups $\mathrm{H}^1(X, \underline{G})$ on algebraic spaces X (for arbitrary algebraic groups G).

Monday April 21, 1958 Alexander Grothendieck went to the Séminaire Henri Cartan

to hear Jean-Pierre Serre's new ground breaking definition of 1-dimensional cohomology groups $\mathrm{H}^1(X, \underline{G})$ on algebraic spaces X (for arbitrary algebraic groups G).

The buses were on strike.

Serre 2001 recounts: “At the end of the talk, Grothendieck told me ‘this will certainly work for all dimensions!’ I found this very optimistic.”

Serre 2001 recounts: “At the end of the talk, Grothendieck told me ‘this will certainly work for all dimensions!’ I found this very optimistic.”

Why was Grothendieck so sure?

Serre 2001 recounts: “At the end of the talk, Grothendieck told me ‘this will certainly work for all dimensions!’ I found this very optimistic.”

Why was Grothendieck so sure? What persuaded him at that time?

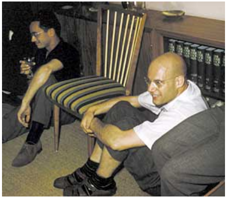
Serre 2001 recounts: “At the end of the talk, Grothendieck told me ‘this will certainly work for all dimensions!’ I found this very optimistic.”

Why was Grothendieck so sure? What persuaded him at that time?

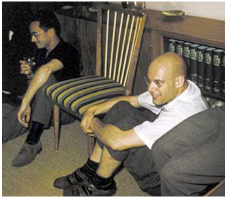
Nearby on that day:

Repairing windows smashed by antisemitic rioters in lead up to the Algerian coup.

Of course, Grothendieck knew Serre's results in the talk.

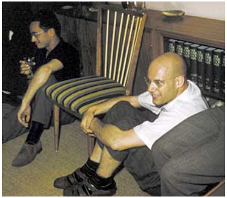


Of course, Grothendieck knew Serre's results in the talk.



But Serre *did not believe* it would work in all dimensions!

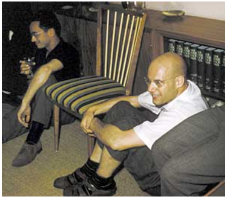
Of course, Grothendieck knew Serre's results in the talk.



But Serre *did not believe* it would work in all dimensions!

> *My reflexes as a topologist told me it would be necessary to deal with higher homotopy groups* $\pi_2, \pi_3, \ldots$ *etc [far too hard].*

Of course, Grothendieck knew Serre's results in the talk.



But Serre *did not believe* it would work in all dimensions!

> *My reflexes as a topologist told me it would be necessary to deal with higher homotopy groups* $\pi_2, \pi_3, \ldots$ *etc [far too hard]. What came later showed Grothendieck was right. (2001)*

Serre was persuaded by the time he finished writing up the talk in September 1958:

> *One may ask if it is possible to define the higher cohomology groups* $\mathrm{H}^q(X, \underline{G})$ ... *in all dimensions (*$q \geq 0$*).*

Serre was persuaded by the time he finished writing up the talk in September 1958:

> *One may ask if it is possible to define the higher cohomology groups* $\mathrm{H}^q(X, \underline{G})$ ... *in all dimensions* ($q \geq 0$). *GROTHENDIECK has shown this is indeed the case (unpublished).*

Serre was persuaded by the time he finished writing up the talk in September 1958:

> *One may ask if it is possible to define the higher cohomology groups* $\mathrm{H}^q(X, \underline{G})$ ... *in all dimensions* ($q \geq 0$)*. GROTHENDIECK has shown this is indeed the case (unpublished). It even seems these new cohomology groups, when G is finite, give the «true cohomology» needed to prove Weil's Conjectures. On this subject see the Introduction to [12].*

Serre was persuaded by the time he finished writing up the talk in September 1958:

> *One may ask if it is possible to define the higher cohomology groups* $\mathrm{H}^q(X, \underline{G})$ … *in all dimensions (*$q \geq 0$*). GROTHENDIECK has shown this is indeed the case (unpublished). It even seems these new cohomology groups, when G is finite, give the «true cohomology» needed to prove Weil's Conjectures. On this subject see the Introduction to [12].*

Serre specifies that Grothendieck's reasons were not published.

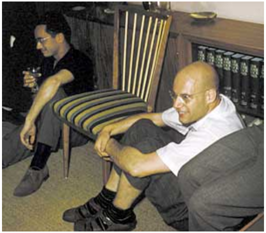
Serre was persuaded by the time he finished writing up the talk in September 1958:

> *One may ask if it is possible to define the higher cohomology groups* $\mathrm{H}^q(X, \underline{G})$ ... *in all dimensions* ($q \geq 0$)*. GROTHENDIECK has shown this is indeed the case (unpublished). It even seems these new cohomology groups, when G is finite, give the «true cohomology» needed to prove Weil's Conjectures. On this subject see the Introduction to [12].*

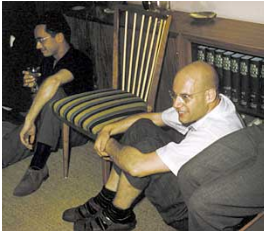
Serre specifies that Grothendieck's reasons were not published.

Only related to Grothendieck's 1958 ICM talk, Edinburgh.

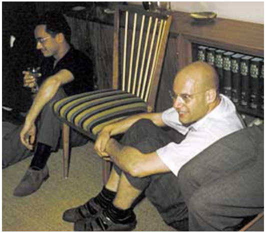
What did Grothendieck see



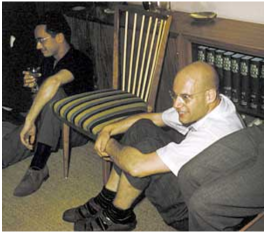
What did Grothendieck see — *during Serre's talk*



What did Grothendieck see — *during Serre's talk* — to persuade him Serre's idea would prove the Weil Conjectures?

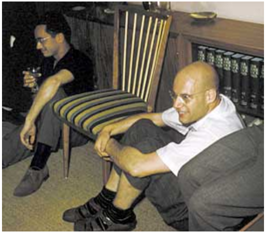


What did Grothendieck see — *during Serre's talk* — to persuade him Serre's idea would prove the Weil Conjectures?



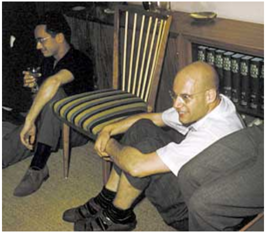
Not some algebra Serre had missed.

What did Grothendieck see — *during Serre's talk* — to persuade him Serre's idea would prove the Weil Conjectures?



Not some algebra Serre had missed. *Certainly not* some arithmetic Serre had missed.

What did Grothendieck see — *during Serre's talk* — to persuade him Serre's idea would prove the Weil Conjectures?



Not some algebra Serre had missed. *Certainly not* some arithmetic Serre had missed.

Grothendieck saw *generalized topology*.

Since 1955 (Kansas) and 1957 (Tōhoku) Grothendieck had his approach to cohomology in topology:

1. Sheaves of sets on a topological space X are étalé spaces, "pasted together" as colimits of open subsets of X.

Since 1955 (Kansas) and 1957 (Tōhoku) Grothendieck had his approach to cohomology in topology:

1. Sheaves of sets on a topological space X are étalé spaces, "pasted together" as colimits of open subsets of X.
2. Sheaves on X have all colimits (unions, quotients), and finite limits (products, equalizers).

Since 1955 (Kansas) and 1957 (Tōhoku) Grothendieck had his approach to cohomology in topology:

1. Sheaves of sets on a topological space X are étalé spaces, "pasted together" as colimits of open subsets of X.
2. Sheaves on X have all colimits (unions, quotients), and finite limits (products, equalizers).
3. Finite limit diagrams define Abelian group sheaves on X, and these define the derived functor cohomology of X.

Since 1955 (Kansas) and 1957 (Tōhoku) Grothendieck had his approach to cohomology in topology:

1. Sheaves of sets on a topological space X are étalé spaces, "pasted together" as colimits of open subsets of X.
2. Sheaves on X have all colimits (unions, quotients), and finite limits (products, equalizers).
3. Finite limit diagrams define Abelian group sheaves on X, and these define the derived functor cohomology of X.

He was entirely certain this was *the* correct view of cohomology.

Since 1955 (Kansas) and 1957 (Tōhoku) Grothendieck had his approach to cohomology in topology:

1. Sheaves of sets on a topological space X are étalé spaces, "pasted together" as colimits of open subsets of X.
2. Sheaves on X have all colimits (unions, quotients), and finite limits (products, equalizers).
3. Finite limit diagrams define Abelian group sheaves on X, and these define the derived functor cohomology of X.

He was entirely certain this was *the* correct view of cohomology.

It was his nature to be entirely certain of things.

During Serre's talk Grothendieck saw:

1. Étalé spaces on a generalized topological space X could be "pasted together" as colimits of Serre's unramified covers of open subsets of X.

During Serre's talk Grothendieck saw:

1. Étalé spaces on a generalized topological space X could be "pasted together" as colimits of Serre's unramified covers of open subsets of X.
2. These sheaves of sets on X will have all colimits, finite limits.

During Serre's talk Grothendieck saw:

1. Étalé spaces on a generalized topological space X could be "pasted together" as colimits of Serre's unramified covers of open subsets of X.
2. These sheaves of sets on X will have all colimits, finite limits.
3. Finite limit diagrams will define Abelian group sheaves on X, and these define the derived functor cohomology of X.

During Serre's talk Grothendieck saw:

1. Étalé spaces on a generalized topological space X could be "pasted together" as colimits of Serre's unramified covers of open subsets of X.
2. These sheaves of sets on X will have all colimits, finite limits.
3. Finite limit diagrams will define Abelian group sheaves on X, and these define the derived functor cohomology of X.

Serre's *unramified covers* = think of unramified algebraic Riemann surfaces.

During Serre's talk Grothendieck saw:

1. Étalé spaces on a generalized topological space X could be "pasted together" as colimits of Serre's unramified covers of open subsets of X.
2. These sheaves of sets on X will have all colimits, finite limits.
3. Finite limit diagrams will define Abelian group sheaves on X, and these define the derived functor cohomology of X.

Serre's *unramified covers* = think of unramified algebraic Riemann surfaces. But of any dimension, and over any alg. closed k.

Serre identified the Weil sheaves.

People credit Grothendieck with defining cohomology by covering maps rather than open covers.

Serre identified the Weil sheaves.

People credit Grothendieck with defining cohomology by covering maps rather than open covers.

That was Serre's idea – as Grothendieck was quick to say.

Serre identified the Weil sheaves.

People credit Grothendieck with defining cohomology by covering maps rather than open covers.

That was Serre's idea – as Grothendieck was quick to say.

Grothendieck's idea was that every "category of sheaves" has intrinsic cohomology in all dimensions, computable by derived functors. And Serre had identified the Weil sheaves.

Serre identified the Weil sheaves.

People credit Grothendieck with defining cohomology by covering maps rather than open covers.

That was Serre's idea – as Grothendieck was quick to say.

Grothendieck's idea was that every "category of sheaves" has intrinsic cohomology in all dimensions, computable by derived functors. And Serre had identified the Weil sheaves.

(Serre used fiber bundles, not sheaves, but the translation was routine for him and Grothendieck.)

Every category of sheaves has cohomology

Deligne (1998, p. 16):

> *In his articles Kansas and Tôhoku, Grothendieck had shown that, for any category of sheaves, there is a notion of cohomology groups.*

That is cohomology groups $\mathrm{H}^q(X, \underline{G})$ for all $q \geq 0$.

Every category of sheaves has cohomology

Deligne (1998, p. 16):

> *In his articles Kansas and Tôhoku, Grothendieck had shown that, for any category of sheaves, there is a notion of cohomology groups.*

That is cohomology groups $\mathrm{H}^q(X, \underline{G})$ for all $q \geq 0$.

Deligne did not mean just categories of sheaves on topological spaces. Tôhoku expressly included module categories for group cohomology.

Every category of sheaves has cohomology

Deligne (1998, p. 16):

> *In his articles Kansas and Tôhoku, Grothendieck had shown that, for any category of sheaves, there is a notion of cohomology groups.*

That is cohomology groups $\mathrm{H}^q(X, \underline{G})$ for all $q \geq 0$.

Deligne did not mean just categories of sheaves on topological spaces. Tôhoku expressly included module categories for group cohomology.

Deligne meant Grothendieck's abstract idea: any AB5 *Abelian category* with a set of generators.

Serre's unramified covers (lightly modified) give "Weil cohomology."

Serre's unramified covers (lightly modified) give "Weil cohomology."

By 1961, replace Galois actions, by actions in any category. This defines a topos, and each topos has an intrinsic cohomology.

Serre's unramified covers (lightly modified) give "Weil cohomology."

By 1961, replace Galois actions, by actions in any category. This defines a topos, and each topos has an intrinsic cohomology.

Grothendieck says, once you think of topology the right way—by functorial cohomology—a topos is essentially the same as an "old-style topological space."

Serre's unramified covers (lightly modified) give "Weil cohomology."

By 1961, replace Galois actions, by actions in any category. This defines a topos, and each topos has an intrinsic cohomology.

Grothendieck says, once you think of topology the right way—by functorial cohomology—a topos is essentially the same as an "old-style topological space."

Important detail: Grothendieck in principle preferred homotopy to cohomology, from the start, as you see in SGA 1. But could not make it work very generally.

Topos lectures, Buffalo 1973

The 1973 lectures stress a two-part idea of *topos* which Grothendieck notes *does not* entirely agree with the collective work *Topos theory and étale cohomology* SGA 4. (Seminar 1963-64, print 1972.)

Topos lectures, Buffalo 1973

The 1973 lectures stress a two-part idea of *topos* which Grothendieck notes *does not* entirely agree with the collective work *Topos theory and étale cohomology* SGA 4. (Seminar 1963-64, print 1972.)

A category theoretic idea and a geometric idea.

Topos lectures, Buffalo 1973

The 1973 lectures stress a two-part idea of *topos* which Grothendieck notes *does not* entirely agree with the collective work *Topos theory and étale cohomology* SGA 4. (Seminar 1963-64, print 1972.)

A category theoretic idea and a geometric idea.

He stresses that he cannot yet make the geometric idea precise.

Topos lectures, Buffalo 1973

The 1973 lectures stress a two-part idea of *topos* which Grothendieck notes *does not* entirely agree with the collective work *Topos theory and étale cohomology* SGA 4. (Seminar 1963-64, print 1972.)

A category theoretic idea and a geometric idea.

He stresses that he cannot yet make the geometric idea precise.

The categorical and the geometric ideas both match Serre's *unramified coverings*, seen through the lens of *Kansas* and *Tôhoku*.

Topos lectures, Buffalo 1973

Currently recovered 33 hours total.

Scheduled for 10 hours, the introductory lectures ran 16. Then 17 hours of smaller group research discussion survive on tape.

Topos lectures, Buffalo 1973

Currently recovered 33 hours total.

Scheduled for 10 hours, the introductory lectures ran 16. Then 17 hours of smaller group research discussion survive on tape.

Also lectured 30 hours on algebraic geometry, and 20 on algebraic groups. Elementary exposition of his viewpoints.

Topos lectures, Buffalo 1973

Currently recovered 33 hours total.

Scheduled for 10 hours, the introductory lectures ran 16. Then 17 hours of smaller group research discussion survive on tape.

Also lectured 30 hours on algebraic geometry, and 20 on algebraic groups. Elementary exposition of his viewpoints.

He said the only mathematics he was thinking about was topos.

Topos: The categorical idea, Buffalo 1973.

A generalized topological space.

> *The essential property of the category of sheaves [of sets] on a topological space, which I have tried to convey, is that it shares essentially all exactness properties of the category of sets—at least those expressed by direct limits with arbitrary indexing diagrams and by finite inverse limits.*

Topos: The categorical idea, Buffalo 1973.

A generalized topological space.

> *The essential property of the category of sheaves [of sets] on a topological space, which I have tried to convey, is that it shares essentially all exactness properties of the category of sets—at least those expressed by direct limits with arbitrary indexing diagrams and by finite inverse limits.*
>
> *So the notion of a topos E should be that E shares the exactness properties of the category of sets, insofar as direct limits and finite inverse limits go. Moreover for technical reasons one has to assume that in E has a small subset, not as big as the whole universe we are working in, which is generating.*

Topos: The categorical idea, Buffalo 1973.

The yoga one finally gets to is essentially the following: the category of sheaves on a topological space is just as good, with a grain of salt, as far as exactness properties are concerned, as the category of sets.

Topos: The categorical idea, Buffalo 1973.

The yoga one finally gets to is essentially the following: the category of sheaves on a topological space is just as good, with a grain of salt, as far as exactness properties are concerned, as the category of sets.

The grain of salt is that this is true for all commutation relations, exactness properties, involving arbitrary direct limits (which may be infinite) and also inverse limits provided we take only finite inverse limits.

Topos: the categorical idea Buffalo 1973.

Grothendieck eventually gives Giraud axioms, and sites: *So here is the notion of a topos, which is slightly technical.*

Topos: the categorical idea Buffalo 1973.

Grothendieck eventually gives Giraud axioms, and sites: *So here is the notion of a topos, which is slightly technical*.

Well I think it is kind of intuitive though, to take the vague notion which intuitively makes more sense: all direct limits, finite inverse limits.

I have a tendency to forget which properties Giraud uses.

Topos: the categorical idea Buffalo 1973.

Grothendieck eventually gives Giraud axioms, and sites: *So here is the notion of a topos, which is slightly technical*.

Well I think it is kind of intuitive though, to take the vague notion which intuitively makes more sense: all direct limits, finite inverse limits.

I have a tendency to forget which properties Giraud uses.

- Locally small category with small generating set.

Topos: the categorical idea Buffalo 1973.

Grothendieck eventually gives Giraud axioms, and sites: *So here is the notion of a topos, which is slightly technical*.

Well I think it is kind of intuitive though, to take the vague notion which intuitively makes more sense: all direct limits, finite inverse limits.

I have a tendency to forget which properties Giraud uses.

- Locally small category with small generating set.
- All finite limits.

Topos: the categorical idea Buffalo 1973.

Grothendieck eventually gives Giraud axioms, and sites: *So here is the notion of a topos, which is slightly technical.*

Well I think it is kind of intuitive though, to take the vague notion which intuitively makes more sense: all direct limits, finite inverse limits.

I have a tendency to forget which properties Giraud uses.

- Locally small category with small generating set.
- All finite limits.
- All small coproducts, disjoint, and stable.

Topos: the categorical idea Buffalo 1973.

Grothendieck eventually gives Giraud axioms, and sites: *So here is the notion of a topos, which is slightly technical.*

Well I think it is kind of intuitive though, to take the vague notion which intuitively makes more sense: all direct limits, finite inverse limits.

I have a tendency to forget which properties Giraud uses.

- Locally small category with small generating set.
- All finite limits.
- All small coproducts, disjoint, and stable.
- Stably effective equivalence relations.

Topos: the whole idea Buffalo 1973.

The intuition is the following: viewing objects of a topos as being something like étalé spaces over the final object of the topos, and the induced topos over an object as just the object itself. That is I think the way one should handle the situation.

Topos: the whole idea Buffalo 1973.

The intuition is the following: viewing objects of a topos as being something like étalé spaces over the final object of the topos, and the induced topos over an object as just the object itself. That is I think the way one should handle the situation.

It's a funny situation because in strict terms, you see, the language which I want to push through doesn't make sense. But of course there are a number of mathematical statements which substantiate it.

Topos: the whole idea Buffalo 1973.

> *The intuition is the following: viewing objects of a topos as being something like étalé spaces over the final object of the topos, and the induced topos over an object as just the object itself. That is I think the way one should handle the situation.*
>
> *It's a funny situation because in strict terms, you see, the language which I want to push through doesn't make sense. But of course there are a number of mathematical statements which substantiate it.*

Don't make sense: 1) all objects are étalé spaces, 2) objects are induced topoi.

Objects of a topos are étalé spaces over the final object

So every $f : Y \to X$ in a topos shows Y as étalé space over X.

Objects of a topos are étalé spaces over the final object

So every $f : Y \to X$ in a topos shows Y as étalé space over X.

AG intended a major generalization of topological "étalé space."

Objects of a topos are étalé spaces over the final object

So every $f : Y \to X$ in a topos shows Y as étalé space over X.

AG intended a major generalization of topological "étalé space."

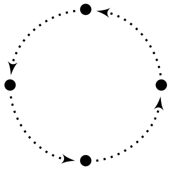
His favorite example is the topos of G-sets for a group, say $G = \mathbb{Z}/4$.

Objects of a topos are étalé spaces over the final object

So every $f : Y \to X$ in a topos shows Y as étalé space over X.

AG intended a major generalization of topological "étalé space."

His favorite example is the topos of G-sets for a group, say $G = \mathbb{Z}/4$.

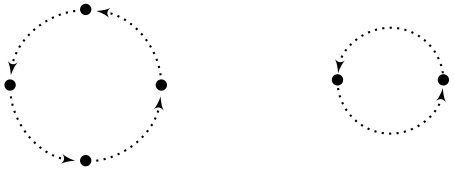


Objects of a topos are étalé spaces over the final object

So every $f : Y \to X$ in a topos shows Y as étalé space over X.

AG intended a major generalization of topological "étalé space."

His favorite example is the topos of G-sets for a group, say $G = \mathbb{Z}/4$.

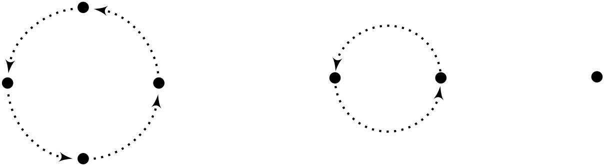


Objects of a topos are étalé spaces over the final object

So every $f : Y \to X$ in a topos shows Y as étalé space over X.

AG intended a major generalization of topological "étalé space."

His favorite example is the topos of G-sets for a group, say $G = \mathbb{Z}/4$.

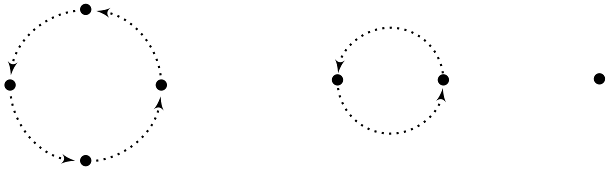


Objects of a topos are étalé spaces over the final object

So every $f : Y \to X$ in a topos shows Y as étalé space over X.

AG intended a major generalization of topological "étalé space."

His favorite example is the topos of G-sets for a group, say $G = \mathbb{Z}/4$.



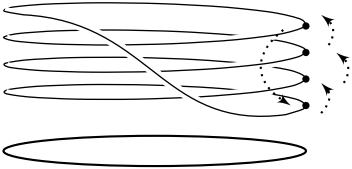
Each orbit is an étalé space over every quotient of it. This is *Not* standard terminology today.

Objects of a topos are étalé spaces over the final object

Étale sheaves put Galois orbits over points of a scheme.

Objects of a topos are étalé spaces over the final object

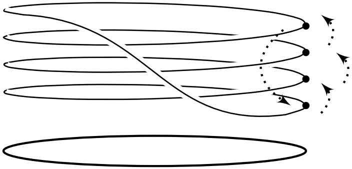
Étale sheaves put Galois orbits over points of a scheme.



Étalé space for a fourth root. Arrows show Galois action on a fiber.

Objects of a topos are étalé spaces over the final object

Étale sheaves put Galois orbits over points of a scheme.



Étalé space for a fourth root. Arrows show Galois action on a fiber.

Thus the idea of topos as a generalized topological space was born, between 4 and 6 pm Monday, April 21, 1958.

Milne *Étale Cohomology* p. 156:

> *Any sheaf F on X_{et} can be represented by an espace étalé, $\widetilde{F}$ provided $\widetilde{F}$ is allowed to be an algebraic space [in the sense of Knutson] rather than a scheme.*

Milne *Étale Cohomology* p. 156:

> *Any sheaf* F *on* X_{et} *can be represented by an espace étalé,* $\widetilde{F}$ *provided* $\widetilde{F}$ *is allowed to be an algebraic space [in the sense of Knutson] rather than a scheme.*

Leads quickly to Artin stacks, moduli spaces, large problems, so that I do not see where it has been pursued in Grothendieck's geometric direction.

Milne *Étale Cohomology* p. 156:

> *Any sheaf F on X_{et} can be represented by an espace étalé, $\widetilde{F}$ provided $\widetilde{F}$ is allowed to be an algebraic space [in the sense of Knutson] rather than a scheme.*

Leads quickly to Artin stacks, moduli spaces, large problems, so that I do not see where it has been pursued in Grothendieck's geometric direction.

Étale sites were the origin of this idea, but by 1973 Grothendieck meant if more generally.

Back to Buffalo 1973: Geometry and algebra.

Grothendieck says of every topos:

> *When we speak about a topos there are always two intuitions. We think of the topos as something like a generalized topological space, embodied through the category of sheaves E. But in fact we think of the topos as being something still different from E, the space which is 'underneath' so to say.*

I think more and more, by the way, that in the language of topoi one should really distinguish between the category and the geometrical object which one has in mind. One has to make this abuse of language because otherwise one will always be a little torn. Never mind.

There is a little trouble with universes

July 12, 1973, the last day of the Buffalo workshop, Grothendieck said:

There is a little trouble with universes

July 12, 1973, the last day of the Buffalo workshop, Grothendieck said:

> *There is a little trouble with universes because one has to add such a strong axiom to set theory. Once one adds the axiom one is in a way happy because one has a lot of leeway to do category theory. For example Deligne has just proved a beautiful theorem on Weil's conjectures and I guess he has used large cardinals.*

There is a little trouble with universes

July 12, 1973, the last day of the Buffalo workshop, Grothendieck said:

> *There is a little trouble with universes because one has to add such a strong axiom to set theory. Once one adds the axiom one is in a way happy because one has a lot of leeway to do category theory. For example Deligne has just proved a beautiful theorem on Weil's conjectures and I guess he has used large cardinals.*
>
> *You have all these topoi, all this general nonsense, and you have to use universes.*

Do Weil's conjectures depend on this axiom?

So therefore the question arises [he and Duskin laugh] do Weil's conjectures depend on this axiom? Everybody would be convinced of course they don't. But Samuel thought about introducing galaxies smaller than universes. . . .

Do Weil's conjectures depend on this axiom?

So therefore the question arises [he and Duskin laugh] do Weil's conjectures depend on this axiom? Everybody would be convinced of course they don't. But Samuel thought about introducing galaxies smaller than universes. . . .

One has to be careful with injectives and such homological algebra, to see if you can construct them in this context.

Do Weil's conjectures depend on this axiom?

So therefore the question arises [he and Duskin laugh] do Weil's conjectures depend on this axiom? Everybody would be convinced of course they don't. But Samuel thought about introducing galaxies smaller than universes. . . .

One has to be careful with injectives and such homological algebra, to see if you can construct them in this context.

I think there is mathematics behind all of this.

–30–

SGA VI: IV.Le lecteur qui ignorerait le langage des sites et topos pourra remplacer partout lesdits objets par des espaces topologiques ordinaires, les objets du topos étant alors remplacés par des ouverts de ces espaces; mais nous lui conseillons néanmoins, de préférence, de s'assimiler le langage des topos, qui fournit un principe d'unification extrêment commode

L'ensemble des deux séminaires consécutifs SGA 4 et SGA 5 (qui pour moi sont comme un seul "séminaire") développe à partir du néant, à la fois le puissant instrument de synthèse et de découverte que représente le langage des topos, et l'outil parfaitement au point, d'une efficacité parfaite, qu'est la cohomologie étale.

Cet ensemble représente la contribution la plus profonde et la plus novatrice que j'aie apportée en mathématiques, au niveau d'un travail entièrement mené à terme.

(RetS 373 note 88)

Deligne, P. (1998). Quelques idées maîtresses de l'œuvre de A. Grothendieck. In *Matériaux pour l'Histoire des Mathématiques au XX*e *Siècle (Nice, 1996)*, pages 11–19. Soc. Math. France.