UNIVALENT FOUNDATIONS

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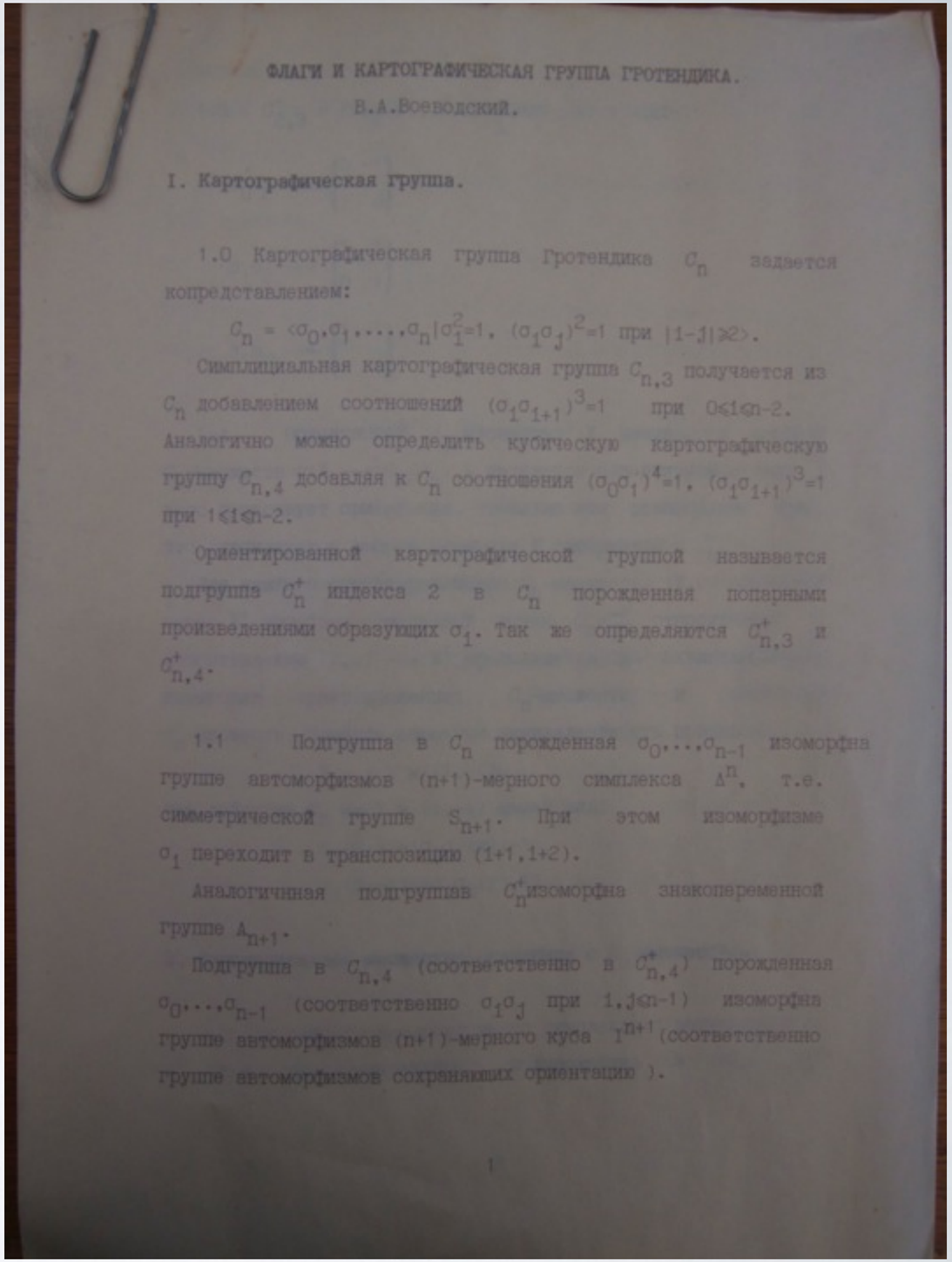
In January, 1984, Alexander Grothendieck submitted to CNRS his proposal "Esquisse d'un Programme". Soon copies of this text started circulating among mathematicians.

A few months later, as a first year undergraduate in Moscow University, I was given a copy of it by George Shabat, my first scientific advisor.

After learning some French with the sole purpose of being able to read this text, I started to work on some of the ideas outlined there.

In 1988 or 1989, I met Michael Kapranov who was, just as I, fascinated by the perspectives of developing mathematics of new "higher dimensional" objects inspired by the theory of categories and 2-categories.



ФЛАГИ И КАРТОГРАФИЧЕСКАЯ ГРУППА ГРОТЕНДИКА.

В.А.Воеводский.

I. Картографическая группа.

1.0 Картографическая группа Гротендика C_n задается копредставлением:

$C_n = \langle \sigma_0, \sigma_1, \ldots, \sigma_n | \sigma_i^2 = 1,\ (\sigma_i\sigma_j)^2 = 1$ при $|i-j| \geq 2 \rangle$.

Симплициальная картографическая группа $C_{n,3}$ получается из C_n добавлением соотношений $(\sigma_i\sigma_{i+1})^3 = 1$ при $0 \leq i \leq n-2$. Аналогично можно определить кубическую картографическую группу $C_{n,4}$ добавляя к C_n соотношения $(\sigma_0\sigma_i)^4 = 1$, $(\sigma_i\sigma_{i+1})^3 = 1$ при $1 \leq i \leq n-2$.

Ориентированной картографической группой называется подгруппа C_n^+ индекса 2 в C_n порожденная попарными произведениями образующих σ_i. Так же определяются $C_{n,3}^+$ и $C_{n,4}^+$.

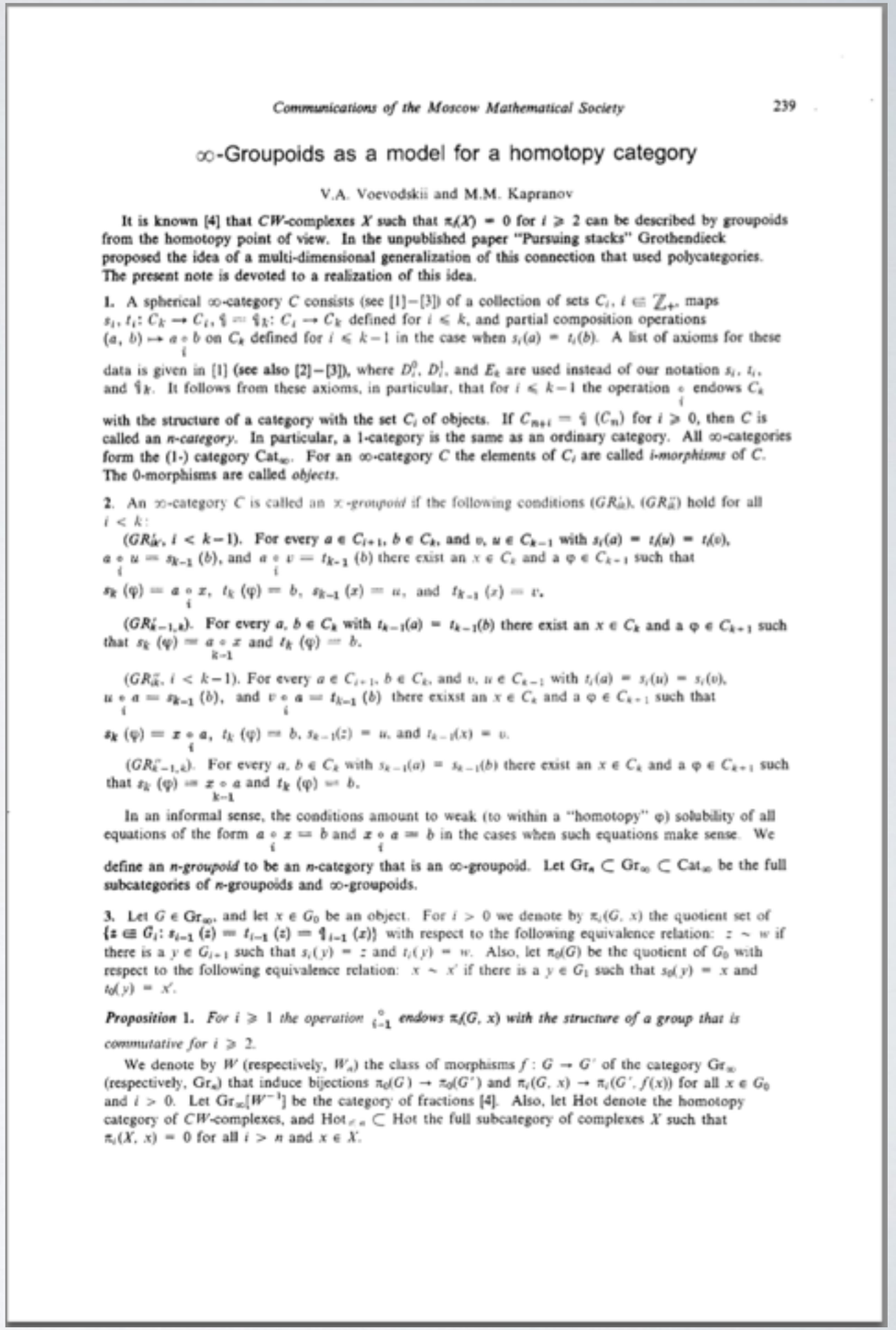
1.1 Подгруппа в C_n порожденная $\sigma_0, \ldots \sigma_{n-1}$ изоморфна группе автоморфизмов (n+1)-мерного симплекса Δ^n, т.е. симметрической группе S_{n+1}. При этом изоморфизме σ_i переходит в транспозицию (i+1, i+2).

Аналогичная подгруппа в C_n^+ изоморфна знакопеременной группе A_{n+1}.

Подгруппа в $C_{n,4}$ (соответственно в $C_{n,4}^+$) порожденная $\sigma_0, \ldots \sigma_{n-1}$ (соответственно $\sigma_i\sigma_j$ при $i,j \leq n-1$) изоморфна группе автоморфизмов (n+1)-мерного куба I^{n+1} (соответственно группе автоморфизмов сохраняющих ориентацию).

The first paper that we published together was called ***"∞-groupoids as a model for a homotopy category"***. In it we claimed to provide a rigorous mathematical formulation and a proof of Grothendieck's idea connecting two classes of mathematical objects: ***∞-groupoids*** and ***homotopy types***.

Later we decided that we could apply similar ideas to another top mathematical problem of that time: to construct ***motivic cohomology***, conjectured to exist in a 1987 paper by A. Beilinson, R. MacPherson and V. Schechtman.



Communications of the Moscow Mathematical Society 239

∞-Groupoids as a model for a homotopy category

V.A. Voevodskii and M.M. Kapranov

It is known [4] that *CW*-complexes X such that $\pi_i(X) = 0$ for $i \geqslant 2$ can be described by groupoids from the homotopy point of view. In the unpublished paper "Pursuing stacks" Grothendieck proposed the idea of a multi-dimensional generalization of this connection that used polycategories. The present note is devoted to a realization of this idea.

1. A spherical ∞-category C consists (see [1]–[3]) of a collection of sets C_i, $i \in \mathbb{Z}_+$, maps $s_i, t_i: C_k \to C_i$, $\S = \S_k: C_i \to C_k$ defined for $i \leqslant k$, and partial composition operations $(a, b) \mapsto a \underset{i}{\circ} b$ on C_k defined for $i \leqslant k-1$ in the case when $s_i(a) = t_i(b)$. A list of axioms for these data is given in [1] (see also [2]–[3]), where D_i^0, D_i^1, and E_k are used instead of our notation s_i, t_i, and $\S_k$. It follows from these axioms, in particular, that for $i \leqslant k-1$ the operation $\underset{i}{\circ}$ endows C_k with the structure of a category with the set C_i of objects. If $C_{n+i} = \S\,(C_n)$ for $i \geqslant 0$, then C is called an *n-category*. In particular, a 1-category is the same as an ordinary category. All ∞-categories form the (1-) category Cat_∞. For an ∞-category C the elements of C_i are called *i-morphisms* of C. The 0-morphisms are called *objects*.

2. An ∞-category C is called an *∞-groupoid* if the following conditions (GR_{ik}'), (GR_{ik}'') hold for all $i < k$:

$(GR_{ik}'$, $i < k-1)$. For every $a \in C_{i+1}$, $b \in C_k$, and $v, u \in C_{k-1}$ with $s_i(a) = t_i(u) = t_i(v)$, $a \underset{i}{\circ} u = s_{k-1}(b)$, and $a \underset{i}{\circ} v = t_{k-1}(b)$ there exist an $x \in C_k$ and a $\varphi \in C_{k-1}$ such that

$$s_k(\varphi) = a \underset{i}{\circ} x,\ t_k(\varphi) = b,\ s_{k-1}(x) = u,\ \text{and}\ t_{k-1}(x) = v.$$

$(GR_{k-1,k}'')$. For every $a, b \in C_k$ with $t_{k-1}(a) = t_{k-1}(b)$ there exist an $x \in C_k$ and a $\varphi \in C_{k+1}$ such that $s_k(\varphi) = a \underset{k-1}{\circ} x$ and $t_k(\varphi) = b$.

$(GR_{ik}''$, $i < k-1)$. For every $a \in C_{i+1}$, $b \in C_k$, and $v, u \in C_{k-1}$ with $t_i(a) = s_i(u) = s_i(v)$, $u \underset{i}{\circ} a = s_{k-1}(b)$, and $v \underset{i}{\circ} a = t_{k-1}(b)$ there exixst an $x \in C_k$ and a $\varphi \in C_{k+1}$ such that

$$s_k(\varphi) = x \underset{i}{\circ} a,\ t_k(\varphi) = b,\ s_{k-1}(z) = u,\ \text{and}\ t_{k-1}(x) = v.$$

$(GR_{k-1,k}'')$. For every $a, b \in C_k$ with $s_{k-1}(a) = s_{k-1}(b)$ there exist an $x \in C_k$ and a $\varphi \in C_{k+1}$ such that $s_k(\varphi) = x \underset{k-1}{\circ} a$ and $t_k(\varphi) = b$.

In an informal sense, the conditions amount to weak (to within a "homotopy" φ) solubility of all equations of the form $a \underset{i}{\circ} x = b$ and $x \underset{i}{\circ} a = b$ in the cases when such equations make sense. We define an *n-groupoid* to be an *n*-category that is an ∞-groupoid. Let $\mathrm{Gr}_n \subset \mathrm{Gr}_\infty \subset \mathrm{Cat}_\infty$ be the full subcategories of *n*-groupoids and ∞-groupoids.

3. Let $G \in \mathrm{Gr}_\infty$, and let $x \in G_0$ be an object. For $i > 0$ we denote by $\pi_i(G, x)$ the quotient set of $\{z \in G_i: s_{i-1}(z) = t_{i-1}(z) = \S_{i-1}(x)\}$ with respect to the following equivalence relation: $z \sim w$ if there is a $y \in G_{i+1}$ such that $s_i(y) = z$ and $t_i(y) = w$. Also, let $\pi_0(G)$ be the quotient of G_0 with respect to the following equivalence relation: $x \sim x'$ if there is a $y \in G_1$ such that $s_0(y) = x$ and $t_0(y) = x'$.

Proposition 1. *For $i \geqslant 1$ the operation $\underset{i-1}{\circ}$ endows $\pi_i(G, x)$ with the structure of a group that is commutative for $i \geqslant 2$.*

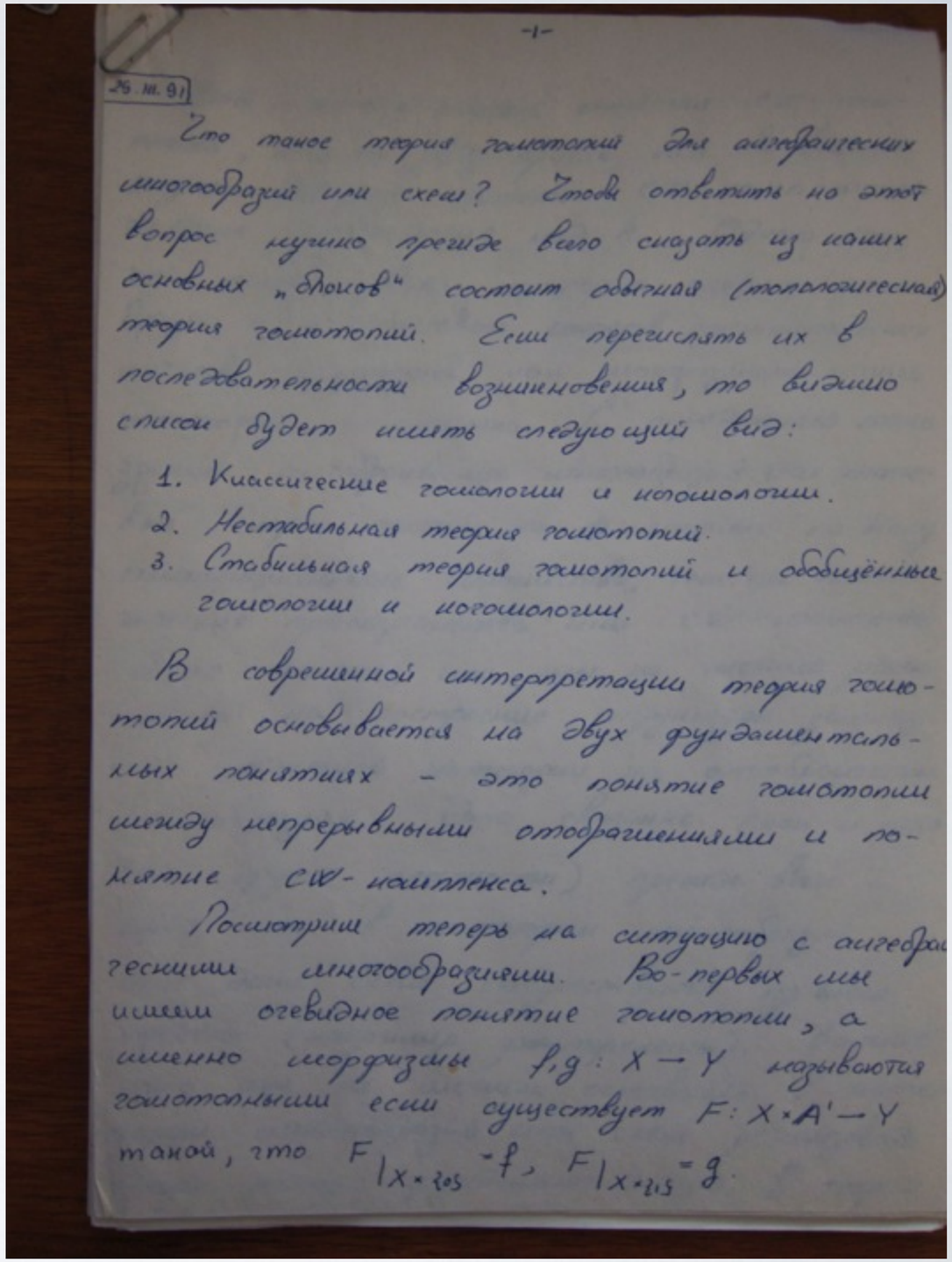
We denote by W (respectively, W_n) the class of morphisms $f: G \to G'$ of the category Gr_∞ (respectively, Gr_n) that induce bijections $\pi_0(G) \to \pi_0(G')$ and $\pi_i(G, x) \to \pi_i(G', f(x))$ for all $x \in G_0$ and $i > 0$. Let $\mathrm{Gr}_\infty[W^{-1}]$ be the category of fractions [4]. Also, let Hot denote the homotopy category of *CW*-complexes, and $\mathrm{Hot}_{\leqslant n} \subset \mathrm{Hot}$ the full subcategory of complexes X such that $\pi_i(X, x) = 0$ for all $i > n$ and $x \in X$.

In the summer of 1990, Kapranov arranged for me to be accepted to graduate school at Harvard without applying.

After a few months, while he was at Cornell and I was at Harvard, our mathematical paths diverged.

I concentrated my efforts on motivic cohomology and later on motivic homotopy theory.

My notes on the right are dated Mar 29, 1991, and start with the question ***"What is a homotopy theory for algebraic varieties or schemes?"***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Что такое теория гомотопий для алгебраических многообразий или схем? Чтобы ответить на этот вопрос нужно прежде всего сказать из каких основных „блоков" состоит обычная (топологическая) теория гомотопий. Если перечислять их в последовательности возникновения, то видимо список будет иметь следующий вид:

1. Классические гомологии и когомологии.
2. Нестабильная теория гомотопий.
3. Стабильная теория гомотопий и обобщённые гомологии и когомологии.

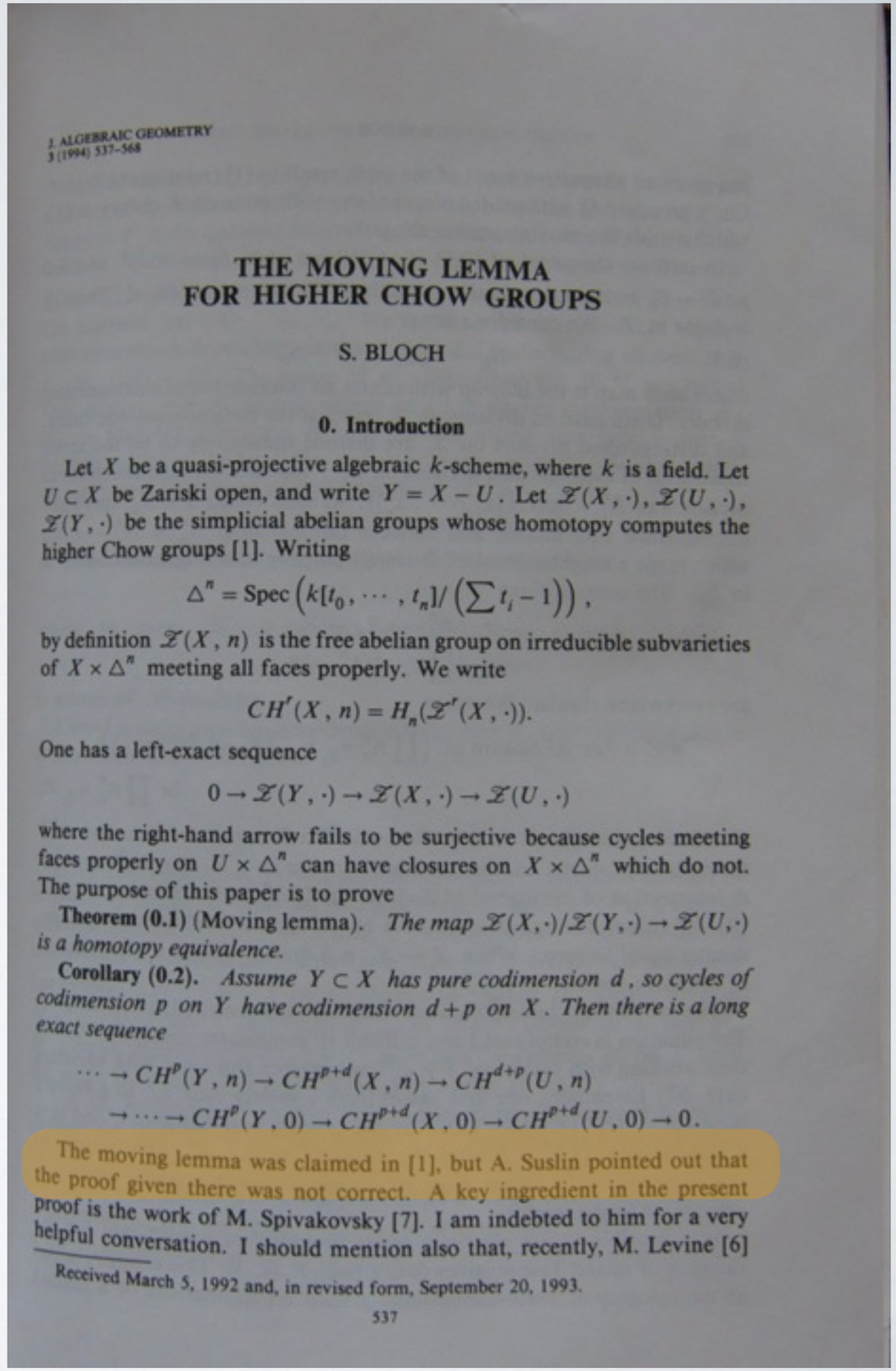
В современной интерпретации теория гомотопий основывается на двух фундаментальных понятиях – это понятие гомотопии между непрерывными отображениями и понятие CW-комплекса.

Посмотрим теперь на ситуацию с алгебраическими многообразиями. Во-первых мы имеем очевидное понятие гомотопии, а именно морфизмы $f, g : X \to Y$ называются гомотопными если существует $F: X \times A^1 \to Y$ такой, что $F|_{X \times 0} = f$, $F|_{X \times 1} = g$.

The field of motivic cohomology was considered at that time to be highly speculative and lacking firm foundation.

The groundbreaking 1986 paper "***Algebraic Cycles and Higher K-theory***" by Spencer Bloch was soon after publication found by Andrej Suslin to contain a mistake in the proof of Lemma 1.1.

The proof could not be fixed, and almost all of the claims of the paper were left unsubstantiated.



J. ALGEBRAIC GEOMETRY
3 (1994) 537–568

THE MOVING LEMMA FOR HIGHER CHOW GROUPS

S. BLOCH

0. Introduction

Let X be a quasi-projective algebraic k-scheme, where k is a field. Let $U \subset X$ be Zariski open, and write $Y = X - U$. Let $\mathcal{Z}(X, \cdot)$, $\mathcal{Z}(U, \cdot)$, $\mathcal{Z}(Y, \cdot)$ be the simplicial abelian groups whose homotopy computes the higher Chow groups [1]. Writing

$$\Delta^n = \mathrm{Spec}\left(k[t_0, \cdots, t_n]/\left(\sum t_i - 1\right)\right),$$

by definition $\mathcal{Z}(X, n)$ is the free abelian group on irreducible subvarieties of $X \times \Delta^n$ meeting all faces properly. We write

$$CH^r(X, n) = H_n(\mathcal{Z}^r(X, \cdot)).$$

One has a left-exact sequence

$$0 \to \mathcal{Z}(Y, \cdot) \to \mathcal{Z}(X, \cdot) \to \mathcal{Z}(U, \cdot)$$

where the right-hand arrow fails to be surjective because cycles meeting faces properly on $U \times \Delta^n$ can have closures on $X \times \Delta^n$ which do not. The purpose of this paper is to prove

Theorem (0.1) (Moving lemma). *The map $\mathcal{Z}(X, \cdot)/\mathcal{Z}(Y, \cdot) \to \mathcal{Z}(U, \cdot)$ is a homotopy equivalence.*

Corollary (0.2). *Assume $Y \subset X$ has pure codimension d, so cycles of codimension p on Y have codimension $d+p$ on X. Then there is a long exact sequence*

$$\cdots \to CH^p(Y, n) \to CH^{p+d}(X, n) \to CH^{d+p}(U, n)$$
$$\to \cdots \to CH^p(Y, 0) \to CH^{p+d}(X, 0) \to CH^{p+d}(U, 0) \to 0.$$

The moving lemma was claimed in [1], but A. Suslin pointed out that the proof given there was not correct. A key ingredient in the present proof is the work of M. Spivakovsky [7]. I am indebted to him for a very helpful conversation. I should mention also that, recently, M. Levine [6]

Received March 5, 1992 and, in revised form, September 20, 1993.

A new proof, which replaced one paragraph from the original paper by 30 pages of complex arguments, was not made public until 1993, and it took many more years for it to be accepted as correct.

Interestingly, this new proof was based on an older result of Mark Spivakovsky, who, at about the same time, announced a proof of the resolution of singularities conjecture. Spivakovsky's proof of resolution of singularities was believed to be correct for several years before being found to contain a mistake. The conjecture remains open.

The approach to motivic cohomology developed by Andrej Suslin, Eric Friedlander and me circumvented Bloch's moving lemma by relying instead on my paper "***Cohomological Theory of Presheaves with Transfers,***" which was written when I was a member at the IAS in 1992/93.

In 1999/2000, again at the IAS, I was giving a series of lectures, and Pierre Deligne was taking notes and checking every step of my arguments. Only then did I discover that the proof of a key lemma in "***Cohomological Theory***" contained a mistake and that the lemma, as stated, could not be salvaged.

Fortunately, I was able to prove a weaker and more complicated lemma which turned out to be sufficient for all applications. A corrected sequence of arguments was published in 2006.

LEMMA 22.10. *Suppose that F is a homotopy invariant presheaf with transfers. Then for any open covering $S = U_0 \cup V$ there is an open $U \subset U_0$ such that $S = U \cup V$ and the sequence $F(MV(Q))$ is exact, where $Q = Q(S,U,V)$:*

$$0 \to F(S) \to F(U) \oplus F(V) \to F(U \cap V) \to 0.$$

PROOF. We may assume that S is connected, since we can work separately with each component. By assumption, there are open $\tilde{U}_0$, $\tilde{V}$ in X such that $U_0 = S \cap \tilde{U}_0$, $V = S \cap \tilde{V}$. Since $\tilde{U}_0$ is open in X, there is an affine open $\tilde{U}$ contained in $\tilde{U}_0$ which contains the finite set of closed points of U_0. Setting $U = S \cap \tilde{U}$, we have $S = U \cup V$. We will show that $F(MV(Q))$ is exact for the square $Q = Q(S,U,V)$.

We first suppose that k is an infinite field. For each α, set $U_\alpha = X_\alpha \cap \tilde{U}$ and $V_\alpha = X_\alpha \cap \tilde{V}$. The canonical map from Q to the square $Q_\alpha = Q(X_\alpha, U_\alpha, V_\alpha)$ induces a morphism of Mayer-Vietoris sequences, $F(MV(Q_\alpha)) \to F(MV(Q))$. It suffices to show that these morphisms are chain homotopic to zero, because $F(MV(Q))$ is the direct limit of the $F(MV(Q_\alpha))$.

Let $Z \subset X$ denote the union of $X - (\tilde{U} \cap \tilde{V})$ and the closed points of S. For each X_α, we know by 11.17 that there is an affine neighborhood X'_α of S in X_α and a standard triple $T_\alpha = (\bar{X}_\alpha, X_{\infty,\alpha}, Z_\alpha)$ with $X'_\alpha \cong \bar{X}_\alpha - X_{\infty,\alpha}$ and $Z_\alpha = X_\alpha \cap Z$. Set $U'_\alpha = X'_\alpha \cap \tilde{U}$ and $V'_\alpha = X'_\alpha \cap \tilde{V}$. Since $\bar{X}_\alpha - (U'_\alpha \cap V'_\alpha)$ lies in $X_{\infty,\alpha} \cup Z_\alpha$, it lies in an affine open subset of $\bar{X}_\alpha$ (by definition 11.5). By 21.2, the Zariski square $Q'_\alpha = Q(X'_\alpha, U'_\alpha, V'_\alpha)$ comes from a covering morphism of triples $T'_\alpha \to T_\alpha$.

By 11.14, the triple T_α is split over an affine neighborhood X''_α of S in X'_α. Set $U''_\alpha = X''_\alpha \cap \tilde{U}$ and $V''_\alpha = X''_\alpha \cap \tilde{V}$, and form the square $Q''_\alpha = Q(X''_\alpha, U''_\alpha, V''_\alpha)$. Since

X''_α and $\tilde{U}$ are affine, so is U''_α. By theorem 21.6, the morphism $F(MV(Q'_\alpha)) \to F(MV(Q''_\alpha))$ is chain homotopic to zero. Since $F(MV(Q_\alpha)) \to F(MV(Q))$ factors through this morphism, it too is chain homotopic to zero.

$$\begin{array}{ccccccccc}
0 & \longrightarrow & F(X_\alpha) & \longrightarrow & F(U_\alpha) \oplus F(V_\alpha) & \longrightarrow & F(U_\alpha \cap V_\alpha) & \longrightarrow & 0 \\
 & & \downarrow & & \downarrow & & \downarrow & & \\
0 & \longrightarrow & F(X'_\alpha) & \longrightarrow & F(U'_\alpha) \oplus F(V'_\alpha) & \longrightarrow & F(U'_\alpha \cap V'_\alpha) & \longrightarrow & 0 \\
 & & \downarrow & & \downarrow & & \downarrow & & \\
0 & \longrightarrow & F(X''_\alpha) & \longrightarrow & F(U''_\alpha) \oplus F(V''_\alpha) & \longrightarrow & F(U''_\alpha \cap V''_\alpha) & \longrightarrow & 0 \\
 & & \downarrow & & \downarrow & & \downarrow & & \\
0 & \longrightarrow & F(S) & \longrightarrow & F(U) \oplus F(V) & \longrightarrow & F(U \cap V) & \longrightarrow & 0
\end{array}$$

If k is finite, exactness follows by a transfer argument. Any element a in the homology of $F(MV(Q))$ must vanish when we pass to $Q \otimes_k k'$ for any infinite algebraic extension k' of k. Since a must vanish for some finite subextension k'_0, a has exponent $[k'_0 : k]$. Since $[k'_0 : k]$ can be chosen to be a power of any prime, we conclude that $a = 0$. □

Lemma 22.10 corrects [**CohTh**, 4.23], which omitted the passage from U_0 to U.

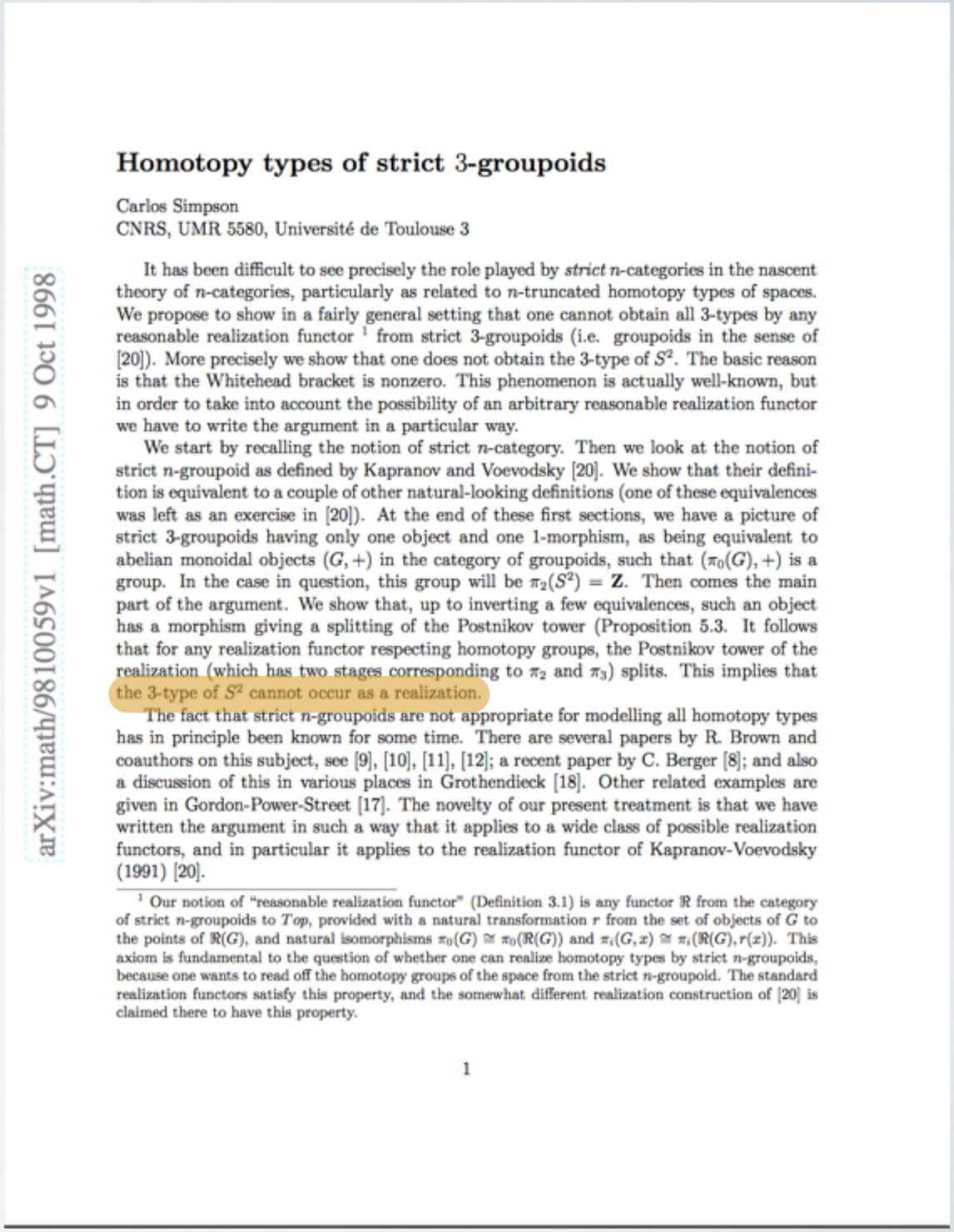
This story got me scared. Starting from 1993 multiple groups of mathematicians studied the "***Cohomological Theory***" paper at seminars and used it in their work and none of them noticed the mistake.

And it clearly was not an accident. ***A technical argument by a trusted author, which is hard to check and looks similar to arguments known to be correct, is hardly ever checked in detail.***

But this is not the only problem that makes mistakes in mathematical texts persist.

In October, 1998, Carlos Simpson submitted to the arXiv preprint server a paper called "***Homotopy types of strict 3-groupoids***". It claimed to provide an argument that implied that the main result of the "∞-groupoids" paper, which M. Kapranov and I had published in 1989, can not be true.

However, Kapranov and I had considered a similar critique ourselves and had convinced each other that it did not apply. I was sure that we were right until the Fall of 2013 (!!).



Homotopy types of strict 3-groupoids

Carlos Simpson
CNRS, UMR 5580, Université de Toulouse 3

It has been difficult to see precisely the role played by *strict* n-categories in the nascent theory of n-categories, particularly as related to n-truncated homotopy types of spaces. We propose to show in a fairly general setting that one cannot obtain all 3-types by any reasonable realization functor [1] from strict 3-groupoids (i.e. groupoids in the sense of [20]). More precisely we show that one does not obtain the 3-type of S^2. The basic reason is that the Whitehead bracket is nonzero. This phenomenon is actually well-known, but in order to take into account the possibility of an arbitrary reasonable realization functor we have to write the argument in a particular way.

We start by recalling the notion of strict n-category. Then we look at the notion of strict n-groupoid as defined by Kapranov and Voevodsky [20]. We show that their definition is equivalent to a couple of other natural-looking definitions (one of these equivalences was left as an exercise in [20]). At the end of these first sections, we have a picture of strict 3-groupoids having only one object and one 1-morphism, as being equivalent to abelian monoidal objects $(G, +)$ in the category of groupoids, such that $(\pi_0(G), +)$ is a group. In the case in question, this group will be $\pi_2(S^2) = \mathbf{Z}$. Then comes the main part of the argument. We show that, up to inverting a few equivalences, such an object has a morphism giving a splitting of the Postnikov tower (Proposition 5.3. It follows that for any realization functor respecting homotopy groups, the Postnikov tower of the realization (which has two stages corresponding to π_2 and π_3) splits. This implies that the 3-type of S^2 cannot occur as a realization.

The fact that strict n-groupoids are not appropriate for modelling all homotopy types has in principle been known for some time. There are several papers by R. Brown and coauthors on this subject, see [9], [10], [11], [12]; a recent paper by C. Berger [8]; and also a discussion of this in various places in Grothendieck [18]. Other related examples are given in Gordon-Power-Street [17]. The novelty of our present treatment is that we have written the argument in such a way that it applies to a wide class of possible realization functors, and in particular it applies to the realization functor of Kapranov-Voevodsky (1991) [20].

[1] Our notion of "reasonable realization functor" (Definition 3.1) is any functor $\Re$ from the category of strict n-groupoids to Top, provided with a natural transformation r from the set of objects of G to the points of $\Re(G)$, and natural isomorphisms $\pi_0(G) \cong \pi_0(\Re(G))$ and $\pi_i(G, x) \cong \pi_i(\Re(G), r(x))$. This axiom is fundamental to the question of whether one can realize homotopy types by strict n-groupoids, because one wants to read off the homotopy groups of the space from the strict n-groupoid. The standard realization functors satisfy this property, and the somewhat different realization construction of [20] is claimed there to have this property.

1

I can see two factors that contributed to this outrageous situation:

- Simpson claimed to have constructed a counterexample, but he was not able to show where in our paper the mistake was. Because of this, it was not clear whether we made a mistake somewhere in our paper or he made a mistake somewhere in his counterexample.

- Mathematical research currently relies on a complex system of mutual trust based on reputations. By the time Simpson's paper appeared, both Kapranov and I had strong reputations. Simpson's paper created doubts in our result, which led to it being unused by other researchers, but ***no one came forward and challenged us on it.***

At about the same time as I discovered the mistake in my motivic paper I was working on a new development, which I called ***2-theories***. The 3-dimensional diagram on the right is an example of the kind of "formulas" that I would have to use to support my arguments about 2-theories.

satisfy the adjunction axiom.

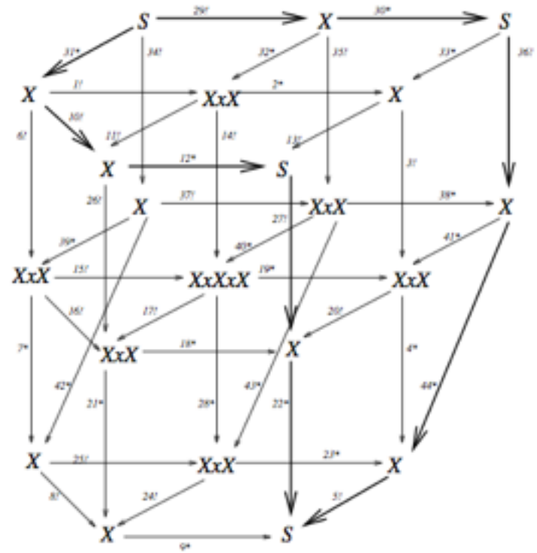
Proof: We have to verify that the compositions

$$\Omega_{(X,x)} \to \Omega_{(X,x)}\Sigma_{(X,x)}\Omega_{(X,x)} \to \Omega_{(X,x)}$$

and

$$\Sigma_{(X,x)} \to \Sigma_{(X,x)}\Omega_{(X,x)}\Sigma_{(X,x)} \to \Sigma_{(X,x)}$$

coincide with the corresponding identity 2-morphisms. One can easily see that these two compositions are dual in the sense of 1.2.3 and therefore it is sufficient to show that the first one equals identity. The main marked diagram for the proof looks as follows:



For the convenience of further reference we numbered all the arrows. The right vertical face of the diagram is the diagram (2) defining the 2-morphism $Id \to \Omega\Sigma$ and the upper horizontal face is the diagram (1) defining the 2-morphism $\Sigma\Omega \to Id$. The whole diagram is the union of the front part which

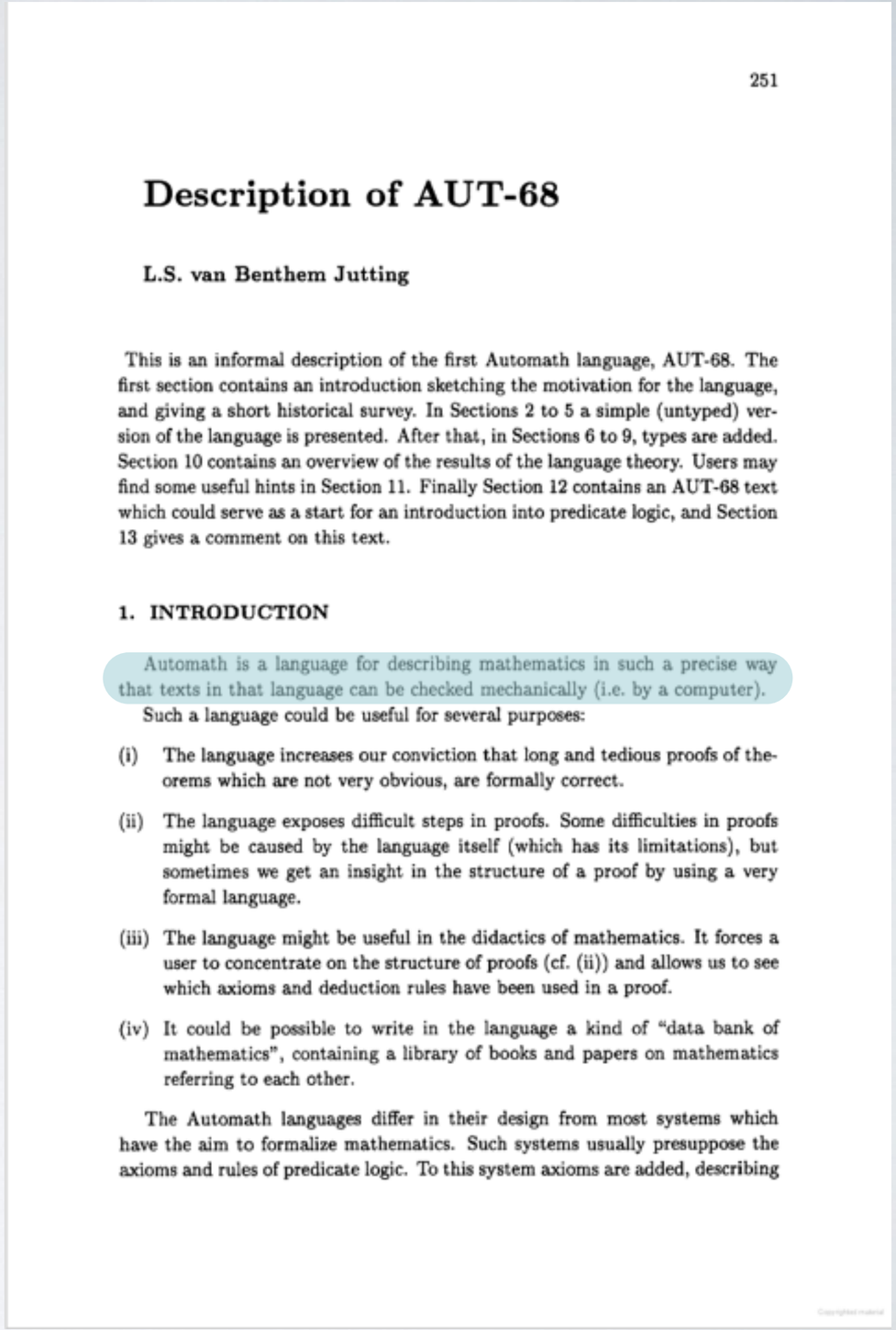
As I was working on these ideas I was getting more and more uncertain about how to proceed. The mathematics of 2-theories is an example of precisely that kind of higher-dimensional mathematics that Kapranov and I had dreamed about in 1989. And I really enjoyed discovering new structures there that were not direct extensions of structures in lower "dimensions".

But to do the work at the level of rigor and precision I felt was necessary would take an enormous amount of effort and would produce a text that would be very difficult to read. And who would ensure that I did not forget something and did not make a mistake, if even the mistakes in much more simple arguments take years to uncover?

I think it was at this moment that I largely stopped doing what is called "curiosity driven research" and started to think seriously about the future.

It soon became clear that the only real long-term solution to the problems that I encountered is to start using computers in the verification of mathematical reasoning.

The software for doing this has been in development since the sixties. The page on the right is from a very interesting book called "Selected Papers on Automath". The number 68 in the title refers to 1968, the year when Automath was created.



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Description of AUT-68

L.S. van Benthem Jutting

This is an informal description of the first Automath language, AUT-68. The first section contains an introduction sketching the motivation for the language, and giving a short historical survey. In Sections 2 to 5 a simple (untyped) version of the language is presented. After that, in Sections 6 to 9, types are added. Section 10 contains an overview of the results of the language theory. Users may find some useful hints in Section 11. Finally Section 12 contains an AUT-68 text which could serve as a start for an introduction into predicate logic, and Section 13 gives a comment on this text.

1. INTRODUCTION

Automath is a language for describing mathematics in such a precise way that texts in that language can be checked mechanically (i.e. by a computer).

Such a language could be useful for several purposes:

(i) The language increases our conviction that long and tedious proofs of theorems which are not very obvious, are formally correct.

(ii) The language exposes difficult steps in proofs. Some difficulties in proofs might be caused by the language itself (which has its limitations), but sometimes we get an insight in the structure of a proof by using a very formal language.

(iii) The language might be useful in the didactics of mathematics. It forces a user to concentrate on the structure of proofs (cf. (ii)) and allows us to see which axioms and deduction rules have been used in a proof.

(iv) It could be possible to write in the language a kind of "data bank of mathematics", containing a library of books and papers on mathematics referring to each other.

The Automath languages differ in their design from most systems which have the aim to formalize mathematics. Such systems usually presuppose the axioms and rules of predicate logic. To this system axioms are added, describing

At the time when I started to look for a practical proof assistant around 2000, I could not find any.

Among mathematicians computer proof verification was almost a forbidden subject. A conversation started about the need for computer proof assistants would invariably drift to the Goedel Incompleteness Theorem (which has nothing to do with the actual problem) or to one or two cases of verification of already existing proofs, which were used only to demonstrate how impractical the whole idea was.

Some of the very few mathematicians who persisted in trying to advance the field of computer verification in mathematics during this time were Tom Hales and Carlos Simpson.

Today, only a few years later, computer verification of proofs and of mathematical reasoning in general looks completely practical to many people who work on Univalent Foundation and Homotopy Type Theory.

The roadblock that prevented generations of interested mathematicians and computer scientists from solving the problem of computer verification of mathematical reasoning was the unpreparedness of foundations of mathematics for the requirements of this task.

Formulating mathematical reasoning in a language precise enough for a computer to follow meant using a foundational system of mathematics not as a standard of consistency applied only to establish a few fundamental theorems, but as a tool that can be employed in everyday mathematical work.

There were two main problems with the existing foundational systems which made them inadequate.

Firstly, existing foundations of mathematics were based on the languages of Predicate Logic and languages of this class are too limited.

Secondly, existing foundations could not be used to directly express statements about such objects as, for example, the ones that my work on 2-theories was about.

It is extremely difficult to accept that mathematics is in need of a completely new foundation. Even many of the people who are directly connected with the advances in Homotopy Type Theory are struggling with this idea.

There is a good reason it is difficult: the existing foundation of mathematics - ZFC, and its main contender for a new foundation - category theory, have been very successful.

It was overcoming the appeal of category theory as a candidate for new foundation of mathematics that was for me personally most difficult.

The story starts with ZFC: the Zermelo-Fraenkel theory with the Axiom of Choice.

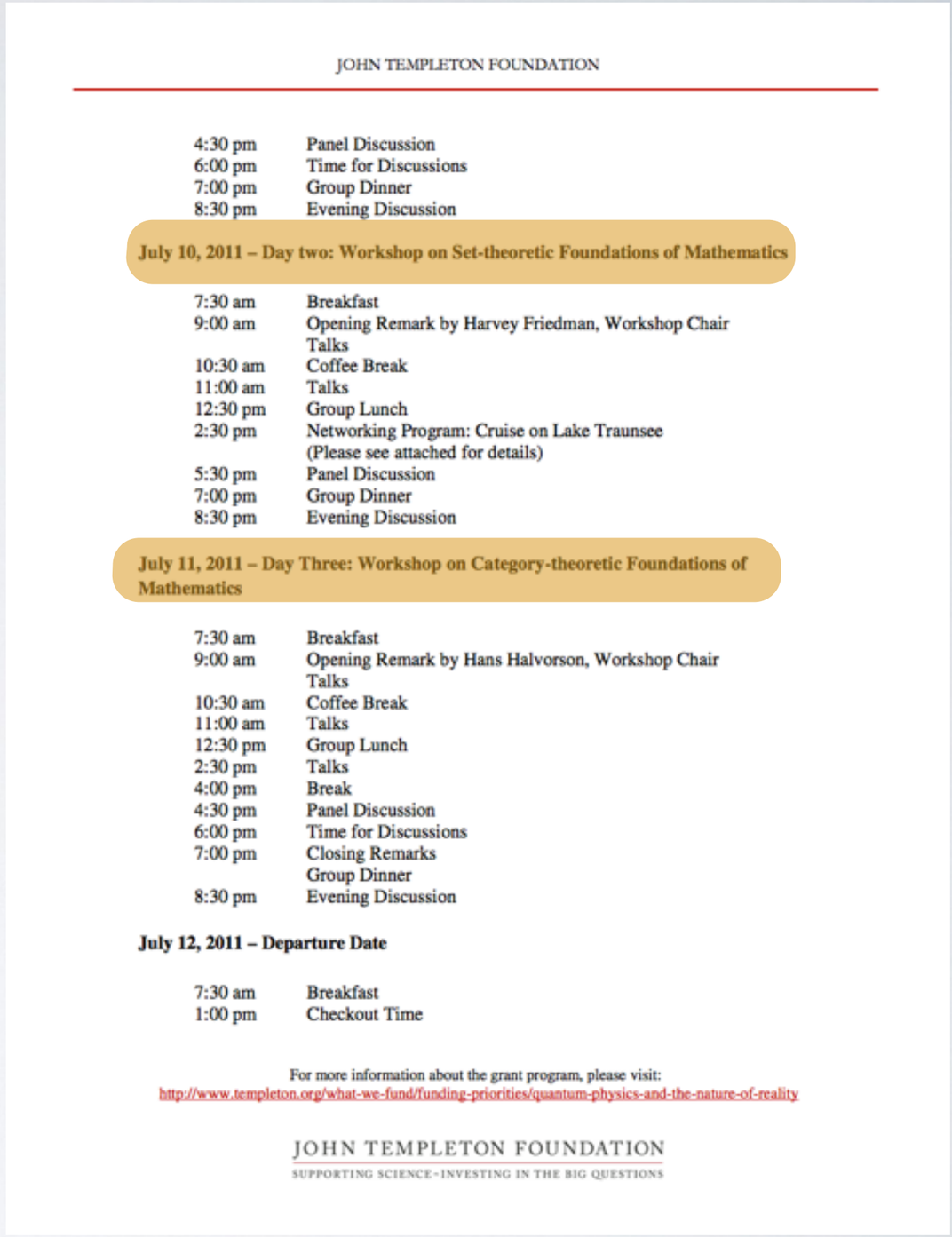
Since the first half of the 20th century mathematics has been presented as a science based on ZFC and ZFC was introduced as a particular theory in Predicate Logic.

Therefore someone who wanted to get to the bottom of things in mathematics had a simple road to follow - learn what Predicate Logic is, then learn a particular theory called ZFC, then learn how to translate propositions about a few basic mathematical concepts into formulas of ZFC, and then learn to believe, through examples, that the rest of mathematics can be reduced to these few basic concepts.

This state of affairs was extremely beneficial for mathematics and it is rightly credited for the great successes of abstract mathematics in the 20th century.

Historically the first problems with ZFC could be seen in the decline of the great enterprise of early Bourbaki, which occurred because the main organizational ideas of mathematics of the second half of 20th century were based on category theory, and category theory could not be well presented in terms of ZFC.

The successes of category theory inspired the idea that categories are "sets in the next dimension" and that the foundation of mathematics should be based on category theory or on its higher dimensional analogs.



JOHN TEMPLETON FOUNDATION

4:30 pm	Panel Discussion
6:00 pm	Time for Discussions
7:00 pm	Group Dinner
8:30 pm	Evening Discussion

July 10, 2011 – Day two: Workshop on Set-theoretic Foundations of Mathematics

7:30 am	Breakfast
9:00 am	Opening Remark by Harvey Friedman, Workshop Chair Talks
10:30 am	Coffee Break
11:00 am	Talks
12:30 pm	Group Lunch
2:30 pm	Networking Program: Cruise on Lake Traunsee (Please see attached for details)
5:30 pm	Panel Discussion
7:00 pm	Group Dinner
8:30 pm	Evening Discussion

July 11, 2011 – Day Three: Workshop on Category-theoretic Foundations of Mathematics

7:30 am	Breakfast
9:00 am	Opening Remark by Hans Halvorson, Workshop Chair Talks
10:30 am	Coffee Break
11:00 am	Talks
12:30 pm	Group Lunch
2:30 pm	Talks
4:00 pm	Break
4:30 pm	Panel Discussion
6:00 pm	Time for Discussions
7:00 pm	Closing Remarks Group Dinner
8:30 pm	Evening Discussion

July 12, 2011 – Departure Date

7:30 am	Breakfast
1:00 pm	Checkout Time

For more information about the grant program, please visit:
http://www.templeton.org/what-we-fund/funding-priorities/quantum-physics-and-the-nature-of-reality

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It is the idea that categories are "sets in the next dimension" that was the most difficult roadblock for me. I clearly recall the feeling of a breakthrough, which I experienced when I understood that this idea is wrong. Categories are not "sets in the next dimension". They are "partially ordered sets in the next dimension," and "sets in the next dimension" are groupoids.

One of the things that made the "categories" versus "groupoids" choice so difficult for me is that I remember it being emphasized by people I learned mathematics from that the great Grothendieck in his wisdom broke with the old-schoolers and insisted on the importance of considering all morphisms and not only isomorphisms and that this was one of the things that made his approach to algebraic geometry so successful.

(Groupoids are often made of set-level objects and their isomorphisms, while categories are often made of set-level objects and "all" morphisms.)

Univalent Foundations, like ZFC-based foundations and unlike category theory, is a complete foundational system, but it is very different from ZFC. To provide a format for comparison let me suppose that any foundation for mathematics adequate both for human reasoning and for computer verification should have the following three components.

The first component is a formal deduction system: a language and rules of manipulating sentences in this language that are purely formal, such that a record of such manipulations can be verified by a computer program.

The second component is a structure that provides a meaning to the sentences of this language in terms of mental objects intuitively comprehensible to humans.

The third component is a structure that enables humans to encode mathematical ideas in terms of the objects directly associated with the language.

In ZFC-based foundations the first component has two "layers". The first layer is a general mechanism for building deduction systems which is called Predicate Logic and the second a particular deduction system called ZFC obtained by applying this mechanism to a set of operations and axioms.

The second component in ZFC is based on the human ability to intuitively comprehend hierarchies. In fact, the axioms of ZFC can be seen as a collection of properties that all hierarchies satisfy, together with the axiom of infinity, which postulates the existence of an infinite hierarchy.

The third component is a way to encode mathematical notions in terms of hierarchies that starts with rules for encoding mathematical properties of sets. That is why ZFC is often called a set theory.

The original formal deduction system of Univalent Foundations is called the Calculus of Inductive Constructions, or CIC. It was developed by Thierry Coquand and Christine Pauline around 1988 and was based on a combination of ideas from the theory and practice of computer languages with ideas in constructive mathematics. The key names associated with these ideas are de Brujin, Per Martin-Lof and Jean-Yves Girard.

The formal deduction system of the proof assistant Coq is a direct descendant of CIC.

The second component of Univalent Foundations, the structure that provides a direct meaning to the sentences of CIC, is based on Univalent Models.

The objects directly associated with sentences of CIC by these models are called homotopy types. The world of homotopy types is stratified by what we call h-levels, with types of h-level 1 corresponding to logical propositions and types of h-level 2 corresponding to sets. Our intuition about types of higher levels comes mostly from their connection with multidimensional shapes, which was studied by ZFC-based mathematics for several decades.

The third component of Univalent Foundations, a way to encode general mathematical notions in terms of homotopy types, is based on the reversal of Grothendieck's idea from the late seventies considered in our "**∞-*groupoids***" paper.

Both mathematically and philosophically, this is the deepest and least understood part of the story.

I have been working on the ideas that led to the discovery of Univalent Models since 2005 and gave the first public presentation on this subject at LMU (Munich) in November 2009.

While I have constructed my models independently, advances in this direction started to appear as early as 1995 and are associated with the names of Martin Hofmann, Thomas Streicher, Steve Awodey and Michael Warren.

In the Spring of 2010 I suggested to the School of Mathematics that I will organize a special program on new foundations of mathematics in 2012/13, despite the fact that at this time it was not clear that the field would be ready for such a program by then.

I now do my mathematics with a proof assistant and do not have to worry all the time about mistakes in my arguments or about how to convince others that my arguments are correct.

But I think that the sense of urgency that pushed me to hurry with the program remains. Sooner or later computer proof assistants will become the norm, but the longer this process takes the more misery associated with mistakes and with unnecessary self-verification the practitioners of the field will have to endure.

I would like to thank all of those who are trying to understand the ideas of Univalent Foundations, to develop these ideas and to communicate these ideas to others. I know it is difficult.