THE BULLETIN OF SYMBOLIC LOGIC
Volume 20, Number 4, December 2014

REVIEWS

The Association for Symbolic Logic publishes analytical reviews of selected books and articles in the field of symbolic logic. The reviews were published in ***The Journal of Symbolic Logic*** from the founding of the JOURNAL in 1936 until the end of 1999. The Association moved the reviews to this BULLETIN, beginning in 2000.

THE UNIVALENT FOUNDATIONS PROGRAM. ***Homotopy Type Theory: Univalent Foundations of Mathematics***. `http://homotopytypetheory.org/book`, Institute for Advanced Study, 2013, vii + 583 pp.

In the first three months of 2014, there was a debate on the "Foundations of Mathematics" (fom) mailing list: does Vladimir Voevodsky's 'Univalent Foundations' program constitute a true foundation of mathematics?

As real debates about real issues tend to be, this one was acrimonious, chaotic, and confused; it did not reach any conclusion or even consensus about the most basic issues. What startled me most was the simple realization that *trained and respected logicians around the world do not agree on what exactly a 'foundation of mathematics' ought to be*.

Sure, most of us think of a formal system with an in-built notion of 'proof', so the boundaries of provability can be explored. The formal system should be plausibly consistent (we cannot hope for a consistency proof) and allow a representation of (a large part of) mathematics in it, which representation must be absolutely uncontroversial and natural. To give an example: ZF(C) satisfies this criterion; second-order PA, often paraded as 'sufficient for everyday mathematics', in my view does not. The representation of higher types over the real numbers in that system requires cumbersome coding and depends itself on mathematical facts.

But there is more one can demand of a candidate foundation. The formal system should have a 'plausible semantics', by which I mean an ontology of what it speaks about. This ontology must be such that, at least after some indoctrination in the initial stages of their career (experience shows that accepting new things gets harder with age), mathematicians can believe in the reality of this postulated world. For trained set theorists, the world of 'sets' with its 'cumulative hierarchy' represents absolute, Platonic reality, which for some reason or other has the shape of a V.

Furthermore, the ontology should provide a completeness theorem for the underlying logic of the formal system, so that the claim can be upheld that it not only represents existing mathematical proofs but is capable of proving anything that can be established by any mathematical means whatsoever.

So far, we can (I hope) agree. There are other issues: how *natural* is the representation of mathematics; how *feasible* is it; how well can one represent *computations*; but let's leave these aside for now.

Set theory has been criticized for (among many other things) its clumsy-looking representation of ordered pairs and for the fact that its language can express things which have no mathematical meaning, such as "the real number e is an element of the Klein 4-group". I believe a lot of this criticism is facetious. Kuratowski's definition of $\{\{x\}, \{x, y\}\}$ as the

1079-8986/14/2004-0004/$2.20
DOI:10.1017/bsl.2014.31

ordered pair (x, y) simply shows that set theory (which was formulated in the most parsimonious way possible, to make it easier to see what a model would be) can be conservatively extended with new notation (and axioms) for ordered pairs. In a similar way, any formalization of elementary arithmetic requires the introduction of function symbols, which we may safely do. The quip about nonsensical statements in the language of set theory merely reflects the fact that a language which is rich enough to express everything sensible, must necessarily also express meaningless things: a Wittgensteinian language that can rule out all nonsense as 'not well-formed', must be very poor.

It is reasonable to expect from a new foundation, based on a new ontology, that it leads to new mathematics and new concepts. Set theory bore this out from the very start: Cantor unveiled the beautiful world of ordinal and cardinal numbers; and later, the fields of point-set topology and measure theory could not have been developed without set theory.

'O dear', I hear you mutter, 'the guy is a lover of set theory! How can he even be *tolerant* of an attempt to establish a new foundation?' Please bear with me, dear reader; I just wanted to stress that there simply is no denying the (often unreasonable) *success* of set theory.

Are there other foundations? What about category theory? Can category theory be seen as an independent, competing foundation of mathematics? Set theorists often claim that the very definition of categories involves sets and functions and that therefore, category theory presupposes set theory; but this is not so. The pioneering work of Lawvere and, even more pregnantly, Street's 'Cosmoi of internal categories', show that one can set up a theory of categories without other primitive notions than categories. Among these abstract categories we will have the 'category of sets', singled out by its distinctive categorical properties.

And also category theory has given rise to lots of new mathematics. The fantastic, multi-faceted world of topos theory is now a fully fledged mathematical field, as Johnstone's monumental *Sketches of an Elephant* demonstrates.

Are set theory and category theory *competing* foundations? Here I must confess that, multiculturally-minded as I am, an idea of the late topos-theorist Japie Vermeulen appeals to me very much: foundations are like an *atlas for a manifold*: no global picture may exist, but overlapping charts for parts do. And where two foundational 'charts' overlap. there of course they compete; but also, where they overlap, they are essentially equivalent. For set theory and category theory this is certainly true, because these theories are in a way bi-interpretable. Clearly, set theory augmented with a few large cardinal assumptions (which any set theorist worth his salt is willing to adopt) suffices for category theory; conversely, the groundbreaking work by Joyal and Moerdijk on *Algebraic set theory* serves as interpretation in the other direction.

There are fields where the set-theoretic foundation has outlived its usefulness and becomes artificial, such as modern abstract homotopy theory. A modern homotopy theorist hardly thinks of a space as a set, with the loose points somehow glued together by another set, the 'set of open subsets' (nor did the pioneers of topology, Brouwer and Poincaré). Just as often, a 'space' might be a homotopy type, so that there is no distinction between the circle and the punctured plane, or the doubly punctured sphere. Or maybe, a 'space' is just an object of a category with a suitable Quillen model structure.

There is a whole area of mathematics where the set-theoretic interpretation is downright *ugly*: constructive mathematics. Take constructive analysis. Anyone who has had to work through one of the existing unpalatable accounts must agree: this is a pitiful mimicking of the classical theory, an unsavoury hodge-podge of double negations and far too many ε's and δ's. Or consider topology: the very definition of composition of paths in a space requires a nonconstructive case split. One feels especially in constructive mathematics that the set-theoretic background hampers the development of a smooth theory.

Constructive mathematics should exploit the weaker logic, and the often resulting consistency of eccentric axioms, to formulate new, synthetic theories. Synthetic Differential Geometry (Kock, Moerdijk-Reyes) and Synthetic Domain theory (Scott, Hyland) are good examples of what can be done.

Let us get to the book under review. It advocates a development of mathematics in Martin-Löf's type theory (MLTT), but MLTT gets a completely new flavour, based on a novel interpretation of the so-called *Identity Types*, for any student the most mysterious element of the intensional version of MLTT. Although a straightforward semantics for extensional MLTT was given by Seely (and later made more precise by Hoffmann), the intensional version remained elusive for a long time (despite work by various people, among others Moerdijk–Palmgren, Van den Berg, Awodey-Warren). Around the year 2005, a number of people (as far as I am aware: Moerdijk, Awodey, Warren, Voevodsky; in written form by Awodey and Warren) formulated the idea that one might interpret MLTT in a *category with a Quillen model structure* and the identity type on X as the *path object* of X. A dependent type is seen as a morphism (just as, for a set A, an A-indexed family of sets is nothing but a function $B \to A$), but not just any morphism: it needs to be a *fibration* (fibrations form a class of morphisms which is part of the structure of a model category); this is forced on us by the path-lifting property which is expressed by Martin–Löf's elimination rule for Identity types.

Voevodsky's interest in type theory was sparked by working with the proof assistant COQ. When he worked out, in a precise way, how one can obtain a model for MLTT with Identity Types and a 'universe' (a 'type of all types', of course carefully formulated so as to avoid paradoxes) in the model category of simplicial sets, he found that this model validates a further axiom that had not been considered by Martin-Löf or his followers: the *univalence axiom*. In fact, in retrospect it seems Martin-Löf did not specify the Identity types fully: it was hard to put them to real use. The univalence axiom makes for a radical change in this respect.

It appears now possible to undertake a development of mathematics in MLTT+ Univalence, in a way quite different from earlier work, and this is the subject matter of the book under review.

The book has been written by a collective of authors, the participants of a 'special year' devoted to Univalent Foundations, which was organized by Awodey, Coquand and Voevodsky 2012–2013 at the IAS in Princeton. Of course, different participants are responsible for different parts of the text and one expects multiple (and conflicting) definitions, sudden ruptures in style and so on. But the book has been remarkably well edited and the various contributions have coalesced into a whole.

The first part (*Foundations*) deals with the system MLTT itself and with Logic, as MLTT is both a notation system and a system with an in-built logic: every type can also be seen as a 'proposition'. The main novelties here are the *higher inductive types* and the stratification of the world in *n-types* for natural numbers n.

In the original set-up of MLTT, a type is specified by introduction and elimination rules. The introduction rule tells us how to form elements of a type; the elimination rule tells us what we can do with these elements. So, for the type N of natural numbers the introduction rule gives us an element 0 and tells us that for any given element x there is an element $x + 1$; the elimination rule basically allows definition by recursion. However, taking the Identity types seriously, it makes a lot of sense to specify types also by saying things about their Identity types. This gives rise to the higher inductive types. For example, the 'circle' is specified by: a point, and a nontrivial path from that point to itself. This is a very nice synthetic definition. Another great advantage of the higher inductive types is the handling of quotients (notoriously hard to deal with in traditional MLTT).

The stratification of n-types comes from the fact that the Identity type construction can be iterated. For elements x, y of a type A we have a type $\mathrm{Id}_A(x, y)$ of witnesses that x and y are similar (of course the book says 'equal'), or, in the new ontology, a type of paths from x to y. Then, for p, q of type $\mathrm{Id}_A(x, y)$ we have $\mathrm{Id}_{\mathrm{Id}_A(x,y)}(p, q)$ and so on. A type P is a *proposition* (in the book: 'mere proposition') if any two elements of P are similar (i.e., the type $\prod_{x,y:P} \mathrm{Id}_P(x, y)$ is inhabited); a type S is a *set* (or a 0-type) if for any elements x, y of S, $\mathrm{Id}_S(x, y)$ is a proposition. In general, T is an $n + 1$-type if for x, y in T we have that $\mathrm{Id}_T(x, y)$ is an n-type. We see, the world of sets is there, but there is much more.

The second part of the book (*Mathematics*) has 4 chapters: Homotopy Theory, Category Theory, Set Theory, and Analysis. It is very nice to see a lot of elementary homotopy theory coming out of the type theory, almost "for free"; and in a similarly pleasant way we obtain quite a bit of ∞-category theory, culminating in the nontrivial *Rezk completion*. In the chapter on Set Theory, we see the "cumulative hierarchy" arise as a higher inductive type.

The chapter on Analysis, however, was a let-down for me: a straightforward translation into type theory of the set-theoretic definition of the real numbers via Dedekind cuts. I had hoped for something more synthetic.

A genuine drawback of the book (in my view) is the absence of any model theory for MLTT + Univalence. Although at several places one reads things like 'this principle is not part of type theory, but it can be consistently assumed', one gets no clue as to how this consistency is proved. More seriously for a new foundation, this deprives the reader of the opportunity to test his understanding of the ontology ('spaces' and 'paths') against a model. Admittedly, a careful treatment of Voevodsky's model in the category of simplicial sets might require quite a bit of space, but at least an informal sketch would have been appreciated.

But in all, the book is a wonderful achievement in a very short time (maybe this explains the spelling *Komolgorov*. . .) and it is extremely useful to get the word to a large audience.

Jaap van Oosten

Department of Mathematics, Utrecht University, P.O.Box 80010, 4508 TA Utrecht, The Netherlands. j.vanoosten@uu.nl.

Thomas Hales. ***Dense Sphere Packings: A Blueprint for Formal Proofs***. Cambridge University Press, Cambridge, 2012, xiv + 271 pp.

In 1611, Johannes Kepler asserted that the highest density that can be achieved when arranging infinitely many congruent spheres in three-dimensional space is attained by the face-centered cubic packing, in which the spheres are arranged in hexagonal layers much the way that oranges are stacked at the grocery store. In August 1998, Thomas Hales announced a proof of Kepler's conjecture, obtained with his student, Samuel Ferguson. Like the proof of the four-color theorem, the proof of the Kepler conjecture reduced the problem to an extensive calculation that was then carried out by computer. Specifically, the proof shows that any counterexample to the Kepler conjecture would imply the existence of a finite arrangement of spheres satisfying certain properties, giving rise, in turn, to a certain combinatorial structure. Computer code then produced an exhaustive enumeration of the possible combinatorial structures; to be realized geometrically, any such structure would have to satisfy certain inequalities. Using branch-and-bound methods, these inequalities were relaxed to linear ones, at which point linear programming methods were used to demonstrate their infeasibility. In other words, the computations showed that there is no finite arrangement of spheres of the kind guaranteed by a putative counterexample. The proof thus consisted of a traditional mathematical argument (250 pages at the time) combined with a substantial body of computer code used to carry out the calculations.

In 1999, the *Annals of Mathematics* assigned a panel of twelve referees the task of reviewing the proof. After four years, the panel reported that they were "99% certain" that the proof was correct, but did not have the means to verify the correctness of the accompanying code. This unsatisfying state of affairs prompted Hales to embark on a project that he named "Flyspeck," to develop a computer-checked axiomatic proof.

The emerging field of *formal verification* uses logic-based computational methods to ensure the correctness of hardware and software design with respect to specifications, as well as the correctness of mathematical claims. One approach, known as *interactive theorem proving*, has users working with a computational system to construct a detailed deductive proof, starting from a small foundational system of axioms and rules. Such a formal derivation can even be checked independently of the system that constructs it. The technology needed to bridge the gap between such a low-level axiomatic presentation and an ordinary, informal mathematical proof is nontrivial, but there have already been impressive achievements along these lines. One such accomplishment is the formalization of the Feit–Thompson Odd Order Theorem by a team of researchers led by Georges Gonthier, announced in late 2012. (For surveys,