TOPOS THEORY

P. T. Johnstone

TOPOS THEORY

P. T. JOHNSTONE
Dept. of Pure Mathematics and Mathematical Statistics University of Cambridge Cambridge, England

DOVER PUBLICATIONS, INC.
Mineola, New York

Bibliographical Note

This Dover edition, first published by Dover Publications, Inc., in 2014, is an unabridged republication of the work originally published by Academic Press, Inc., New York, in 1977.

International Standard Book Number

ISBN-13: 978-0-486-49336-7
ISBN-10: 0-486-49336-9

Manufactured in the United States by Courier Corporation
49336902 2014
www.doverpublications.com

Dover Books on Mathematics

Handbook of Mathematical Functions: with Formulas, Graphs, and Mathematical Tables, Edited by Milton Abramowitz and Irene A. Stegun. (0-486-61272-4)

An Introduction to the Theory of Groups, Paul Alexandroff. (0-486-48813-6)

Intuitive Concepts in Elementary Topology, B. H. Arnold. (0-486-48199-9)

Logic and Boolean Algebra, Bradford Henry Arnold. (0-486-48385-1)

Introduction to Differentiable Manifolds, Louis Auslander and Robert E. MacKenzie. (0-486-47172-1)

Introduction to Vectors and Tensors: Second Edition–Two Volumes Bound as One, Ray M. Bowen and C.-C. Wang. (0-486-46914-X)

A First Course in Graph Theory, Gary Chartrand and Ping Zhang. (0-486-48368-1)

A Course in Linear Algebra, David B. Damiano and John B. Little. (0-486-46908-5)

Elementary Number Theory: Second Edition, Underwood Dudley. (0-486-46931-X)

Fundamental Concepts of Abstract Algebra, Gertrude Ehrlich. (0-486-48589-7)

Thirteen Books of Euclid's Elements 3 Volume SetDover. (0-486-46118-1)

An Introduction to Differential Equations and Their Applications, Stanley J. Farlow. (0-486-44595-X)

Partial Differential Equations for Scientists and Engineers, Stanley J. Farlow. (0-486-67620-X)

Complex Variables: Second Edition, Stephen D. Fisher. (0-486-40679-2)

Partially Ordered Algebraic Systems, Laszlo Fuchs. (0-486-48387-8)

Ordinary Differential Equations, Jack K. Hale. (0-486-47211-6)

Introduction to Applied Numerical Analysis, Richard W. Hamming. (0-486-48590-0)

Basic Algebra I: Second Edition, Nathan Jacobson. (0-486-47189-6)

Basic Algebra II: Second Edition, Nathan Jacobson. (0-486-47187-X)

Elementary Calculus: An Infinitesimal Approach, H. Jerome Keisler. (0-486-48452-1)

Calculus: An Intuitive and Physical Approach (Second Edition), Morris Kline. (0-486-40453-6)

Elementary Theory and Application of Numerical Analysis: Revised Edition, James E. Miller, David G. Moursund and Charles S. Duris. (0-486-47906-4)

How to Solve Applied Mathematics Problems, B. L. Moiseiwitsch. (0-486-47927-7)

Ordinary Differential Equations and Their Solutions, George Moseley Murphy. (0-486-48591-9)

Advanced Calculus, H. K. Nickerson, D. C. Spencer and N. E. Steenrod. (0-486-48090-9)

Permutation Groups, Donald S. Passman. (0-486-48592-7)

(continued on back flap)

TOPOS
THEORY

Preface

The origins of this book can be traced back to a series of six seminars, which I gave in Cambridge in the winter of 1973/74, and which formed the nucleus of the present chapters 1–6. Further seminars in the same series, covering parts of chapters 0, 7 and 9, were given by Barry Tennison and Robert Seely. By popular request, the notes of these seminars were written up and enjoyed a limited circulation. In the summer of 1974, I began to revise and expand these notes, with the idea that they might some day form a book. During the winter and spring of 1975, whilst at the University of Liverpool, I was able to give a course of lectures covering the material of chapters 0–5 and 8 in some detail. By the end of this period, I had a fairly clear picture of the overall shape of the book; and (encouraged by Michael Butler) I began the actual writing of it in July 1975. From October 1975 to March 1976 I was at the University of Chicago, where there was a weekly seminar on topos theory organized by Saunders Mac Lane and myself; the material covered during this period was drawn mainly from chapters 2, 4, 5, 6 and 9, and the speakers (in addition to myself) were Kathy Edwards, Steve Harris and Steve Landsburg. Also during this period, I wrote the text of chapters 2–5 and most of chapter 6; the remainder of the text was completed during May–July 1976 after my return to Cambridge.

The lectures and seminars mentioned above had a very direct influence on the text of the book, and all those who attended them (in particular those whose names appear above) deserve my thanks for the part they have played in shaping it. But I have also benefited from more informal contacts with many mathematicians at conferences and elsewhere. Among those whose (largely unpublished) ideas I have gladly borrowed are Julian Cole, Radu Diaconescu, Mike Fourman, Peter Freyd, André Joyal and Chris Mulvey. John Gray gave me valuable advice on 2-categorical matters, and Jack Duskin and Barry Tennison helped to improve my understanding of cohomology. And I must thank Jean Bénabou for the many ideas I have consciously or inadvertently borrowed from him, and Tim Brook for his help in the compilation of the bibliography.

There remain four mathematicians to whom I owe a debt which must be acknowledged individually. Myles Tierney introduced me to topos theory through his lectures at Varenna in 1971; looking back on the published version [TV], I still find it incredible that he managed to teach me so much in eight short lectures. Gavin Wraith's help and encouragement have meant a great deal to me, and his Bangor lectures [WB] served as a model for some parts of this book. Like every other worker in topos theory, I owe Bill Lawvere an overwhelming debt in general terms, for his pioneering insights; but I have also benefited at a more personal level from his ideas and conversation. Above all, I have to express my indebtedness to Saunders Mac Lane: but for him I should never have become a topos-theorist in the first place; and the care with which he has read through the original typescript, and provided suggestions for improvement in almost every paragraph, has been altogether out of the ordinary. If there are any major errors or obscurities still remaining in the text, they are surely a testimony to my perversity rather than his lack of vigilance.

On a different, but no less significant, level, I must also thank the Universities of Liverpool and Chicago, and St John's College, Cambridge, for employing me during the writing of the book; Paul Cohn, for accepting it for publication in the L.M.S. Monographs series; and the staff of Academic Press for the efficiency with which they have transformed my amateurish typescript into the book which you see before you.

Cambridge, June 1977 P.T.J.

Introduction

Topos theory has its origins in two separate lines of mathematical development, which remained distinct for nearly ten years. In order to have a balanced appreciation of the significance of the subject, I believe it is necessary to consider the history of these two lines, and to understand why they came together when they did. I therefore begin this Introduction with a (personal, and doubtless strongly biased) historical survey.

The earlier of the two lines begins with the rise of *sheaf theory*, originated in 1945 by J. Leray, developed by H. Cartan and A. Weil among others, and culminating in the published work of J. P. Serre [107], A. Grothendieck [42] and R. Godement [TF]. Like a great deal of homological algebra, the theory of sheaves was originally conceived as a tool of algebraic topology, for axiomatizing the notion of "local coefficient system" which was essential for a good cohomology theory of non-simply-connected spaces; and the full title of Godement's book indicates that it was still viewed in this light in 1958. But well before this date, the power of sheaf theory had been recognized by algebraic and analytic geometers; and in more recent years, its influence has spread into many other areas of mathematics. (For two widely-differing examples, see [49] and [106].)

However, in algebraic geometry it was soon discovered that the topological notion of sheaf was not entirely adequate, in that the only topology available on an abstract algebraic variety or scheme, the Zariski topology, did not have "enough open sets" to provide a good geometric notion of localization. In his work on descent techniques [43] and the étale fundamental group [44], A. Grothendieck observed that to replace "Zariski-open inclusion" by "étale morphism" was a step in the right direction; but unfortunately the schemes which are étale over a given scheme do not in general form a partially ordered set. It was thus necessary to invent the notion of "Grothendieck topology" on an arbitrary category, and the generalized notion of sheaf for such a topology, in order to provide a framework for the development of étale cohomology.

This framework was built up during the "Seminaire de Géometrie Algébrique du Bois Marie" held during 1963–64 by Grothendieck with the assistance of M. Artin, J. Giraud, J. L. Verdier and others. (The proceedings of this seminar were published in a revised and greatly enlarged version [GV], including some notable additional results of P. Deligne, eight years later.) Among the most important results of the original seminar was the theorem of Giraud, which showed that the categories of generalized sheaves which arise in this way can be completely characterized by exactness properties and size conditions; in the light of this result, it quickly became apparent that these categories of sheaves were a more important subject of study than the sites (= categories + topologies) which gave rise to them. In view of this, and because a category with a topology was seen as a "generalized topological space", the (slightly unfortunate) name of *topos* was given to any category satisfying Giraud's axioms.

Nevertheless, toposes were still regarded primarily as vehicles for carrying cohomology theoreies; not only étale cohomology, but also the "fppf" and crystalline cohomologies, and others. The power of the machinery developed by Grothendieck was amply demonstrated by the substantial geometrical results obtained by using these cohomology theories in the succeeding years, culminating in P. Deligne's proof [159] of the famous "Weil conjectures"— the mod-p analogue of the Riemann hypothesis. And the machinery itself was further developed, for example in J. Giraud's work [38] on nonabelian cohomology. But the full import of the dictum that "the topos is more important than the site" seems never to have been appreciated by the Grothendieck school. For example, though they were aware of the cartesian closed structure of toposes ([GV], IV 10), they never exploited this idea to the full along the lines laid down by Eilenberg–Kelly [160]. It was, therefore, necessary that a second line of development should provide the impetus for the elementary theory of toposes.

The starting-point of this second line is generally taken to be F. W. Lawvere's pioneering 1964 paper on the elementary theory of the category of sets [71]. However, I believe that it is necessary to go back a little further, to the proof of the Lubkin–Heron–Freyd–Mitchell embedding theorem for abelian categories [AC]. It was this theorem which, by showing that there is an explicit set of elementary axioms which imply all the (finitary) exactness properties of module categories, paved the way for a truly autonomous development of category theory as a foundation for mathematics.

(Incidentally, the Freyd–Mitchell embedding theorem is frequently regarded as a culmination rather than a starting-point; this is because of what seems to me a misinterpretation (or at least an inversion) of its true significance. It is commonly thought of as saying "If you want to prove something about an abelian category, you might as well assume it is a category of modules"; whereas I believe its true import is "If you want to prove something about categories of modules, you might as well work in a general abelian category"—for the embedding theorem ensures that your result will be true in this generality, and by forgetting the explicit structure of module categories you will be forced to concentrate on the essential aspects of the problem. As an example, compare the module-theoretic proof of the Snake Lemma in [HA] with the abelian-category proof in [CW].)

This theorem was soon followed by Lawvere's paper [71], setting out a list of elementary axioms which, with the addition of the non-elementary axioms of completeness and local smallness, are sufficient to characterize the category of sets. (In a subsequent paper [72], Lawvere provided a similar axiomatization of the category of small categories, and D. Schlomiuk [105] did the same for the category of topological spaces.)

One may well ask why this paper was not immediately followed by the explosion of activity which greeted the introduction of elementary toposes six years later. In retrospect, the answer is that Lawvere's axioms were too specialized: the category of sets is an extremely useful object to have as a foundation for mathematics, but as a subject of axiomatic study it is not (*pace* the activities of Martin, Solovay *et al.*!) tremendously interesting—it is too "rigid" to have any internal structure. In a similar way, if the abelian-category axioms had applied only to the category of abelian groups, and not to categories of modules or of abelian sheaves, they too would have been neglected. So what was needed for the category of sets was an axiomatization which would also cover set-valued functor categories and categories of set-valued sheaves—i.e. the axioms of an elementary topos.

In his subsequent papers ([73], [75]), Lawvere began to investigate the idea that the two-element set $\{true, false\}$ can be regarded as an "object of truth-values" in the category of sets; in particular, he observed that the presence of such an object in an arbitrary category enables us to reduce the Comprehension Axiom to an elementary statement about adjoint functors. The same idea was at the heart of the work of H. Volger ([125], [126]) on logical and semantical categories.

Meanwhile, the embedding-theorem side of things was advanced by M. Barr [2], who formulated the notion of *exact category* and used it as the basis of a non-additive embedding theorem. The closely-related notion of *regular category* was formulated independently by P. A. Grillet [41] and D. H. Van

Osdol [122], who used it in their investigations of general sheaf theory; and Barr himself observed that Giraud's theorem could be regarded as little more than a special case of his embedding theorem. This perhaps represents (logically, if not chronologically) the first coming-together of the two lines of development mentioned earlier.

However, at about the same time Lawvere's attention also turned towards Grothendieck toposes; he observed that every Grothendieck topos has a truth-value object Ω, and that the notion of Grothendieck topology is closely connected with endomorphisms of Ω (see [LH]). During the year 1969–70, Lawvere and M. Tierney (who had earlier contributed to the theory of exact categories) began to investigate the consequences of taking "there exists an object of truth-values" as an axiom; the result was elementary topos theory. A remarkably large proportion of the basic theory was developed in that 12-month period, as will be apparent from the large number of theorems in chapters 1–4 of this book whose proof is credited to Lawvere and Tierney.

Once these theorems became known to mathematicians at large (i.e. after Lawvere's lectures at Zürich and Nice [LN] in the summer of 1970, and the Dalhousie conference [LH] in January 1971), they were immediately taken up and further developed by several people. One of the first and most important was P. Freyd, whose lectures at the University of New South Wales [FK] explored the embedding theory of toposes; in retrospect this seems to have been something of a blind alley, in that the inversion of the usual metatheorem, mentioned above in connection with abelian categories, applies with even more force to topos theory—since the great virtue of the topos axioms is their elementary character, one should not have to appeal to a non-elementary embedding theorem to prove elementary facts about toposes. (Freyd's embedding theorem will not be found in this book; but the most important (and elementary) part of it, which shows that any topos can be embedded in a Boolean topos, is proved in §7.5.) Nevertheless, Freyd's work contained a great many important technical results; in particular his characterization of natural number objects is a theorem of major importance.

Amongst other early workers on topos theory, one should mention J. Bénabou and his student J. Celeyrette in Paris [BC], and A. Kock and G. C. Wraith in Aarhus [KW]. C. J. Mikkelsen, a student of Kock, was the first to prove that one of the Lawvere–Tierney axioms, that of finite colimits, could be deduced from the others; his thesis [84] also contains many important contributions to lattice-theory in a topos.

In view of the Lawvere–Tierney proof of the independence of the continuum hypothesis [117], it became a matter of importance to determine the precise relationship between elementary topos theory and axiomatic set theory. The answer was found independently by J. C. Cole [18], W. Mitchell [85] and G. Osius [92]. W. Mitchell also introduced an idea which has since become central to the subject: namely that each topos gives rise to an internal language which can be used to make "quasi-set-theoretical" statements about objects and morphisms of the topos. Whilst the original idea is due to Mitchell, its most enthusiastic proponent has undoubtedly been J. Bénabou, and his students have used the internal language extensively in recent years.

The next major advance was made by R. Diaconescu, a student of Tierney whose thesis was completed in 1973. Diaconescu's theorem [30] was important not only for the insight it gave into the 2-categorical structure of $\mathfrak{Top}$, but also because it represented the first significant exploitation of the theory of internal categories. (This theory had developed over the years in a rather haphazard way, largely through unpublished work of J. Bénabou.) As an encore, Diaconescu proved the relative Giraud theorem; Giraud himself [39] had proved a relative version of his theorem (by non-elementary means) for Grothendieck toposes, and W. Mitchell had formulated the correct elementary form. But Mitchell was able to prove this only in the special case when the "object of generators" (see 4.43) is 1; it turned out that Diaconescu's theorem was the essential tool needed to prove the general case. At about the same time, P. T. Johnstone [52] also used internal categories in his proof that Grothendieck's construction of the associated sheaf functor could be carried over to the elementary setting.

The next development (which in fact overlapped the previous ones) was the rise of the notion of toposes as theories and the concept of classifying topos. In a sense, this goes right back to Lawvere's work [176] on algebraic theories, but its connection with topos theory began with the work of M. Hakim [45], a student of Grothendieck, on relative schemes, in the course of which she constructed the classifiers for rings and local rings, and established their fundamental properties. In 1972, A. Joyal and G. E. Reyes [RM] isolated the notion of "coherent theory" (= finitary geometric theory, in our terminology), and proved that every such theory has a classifying topos; their work was later extended by Reyes and M. Makkai [82] to cover infinitary geometric theories.

It was F. W. Lawvere [LB] who first observed that, in view of the work of Joyal and Reyes, the theorem of P. Deligne on points of coherent toposes was precisely equivalent to the Gödel–Henkin completeness theorem for finitary geometric theories; and Lawvere too conjectured the "Booleanvalued completeness theorem" for infinitary theories whose topos-theoretic equivalent was proved by M. Barr [4].

Once again, Diaconescu's theorem provided the key to the "relativization" of the Joyal–Reyes results; the decisive step was taken in 1973 by G. C. Wraith, who constructed an object classifier over an arbitrary topos with a natural number object. From there to the general existence theorem for classifying toposes was little more than a formality; it was achieved independently by A. Joyal, M. Tierney [119] and J. Bénabou [8].

This brings our historical survey up to date, at least where major results are concerned. Now let us consider the present position of topos theory, and its future prospects.

The first thing which must be said is that the basic theory of elementary toposes (i.e. the contents of chapters 1–5 of this book) seems to be almost completely worked out. Indeed, I am aware of only one substantial unanswered question arising from these five chapters (namely the existence of finite (pseudo-)colimits in $\mathfrak{Top}$, touched on in §4.2); doubtless there are many other minor points to be cleared up, and several theorems whose proofs will be improved and simplified in time, but the foundations of the subject do appear to be pretty stable. This is of course a bad thing: it is vital to the health of a subject as basic as topos theory that its fundamental tenets should be the subject of continual review and improvement, and I am uncomfortably aware that by writing this book I have contributed largely to the concreting-over of these foundations. My only defence against this charge is that it seemed to me that the solidification was taking place anyway, and it was better that it should happen in print than in an unpublished folklore accessible only to insiders.

The average mathematician, who regards category theory as "generalized abstract nonsense", tends to regard topos theory as generalized abstract category theory. (No doubt it has inherited this reputation from its parent, the Grothendieck approach to algebraic geometry.) And yet S. Mac Lane [179] regards the rise of topos theory as a symptom of the *decline* of abstraction in category theory, and of abstract algebra in general. I am convinced that Mac Lane is right, and that his insight points the way to the most probable future development of topos theory; for almost all the *recent* work of significance in topos theory has been concerned not with toposes as an abstract and isolated area of mathematics, but with toposes as an aid to understanding and clarifying concepts in other areas. (See, for example, [36], [57], [63], [79], [88], [90], [112], [130].)

To take a specific example, consider the general existence theorem for classifying toposes (6.56). One's first reaction on seeing this theorem is to admire its elegance and generality; the second reaction (which comes quite a long time later) is to realize its fundamental uselessness—a quality which, by the way, it shares with the General Adjoint Functor Theorem. For the only possible use of such a theorem is to reduce the study of a particular geometric theory to the study of its generic model (or conversely, to reduce the study of a particular topos to that of the theory whose generic model it contains), and the theorem as proved in §6.5 simply does not provide an effective means of passing from the one to the other. Thus the "syntactic" proof of the same theorem in §7.4, though appreciably messier, is much more valuable in practice—and it is this proof, not the later one given in the earlier chapter, which has inspired most subsequent work on the subject.

In saying that the future of topos theory lies in the clarification of other areas of mathematics through the application of topos-theoretic ideas, I do not wish to imply that, like Grothendieck, I view topos theory as a machine for the demolition of unsolved problems in algebraic geometry or anywhere else. On the contrary, I think it is unlikely that elementary topos theory itself will solve any major outstanding problems of mathematics; but I do believe that the spreading of the topos-theoretic outlook into many areas of mathematical activity will inevitably lead to the deeper understanding of the real features of a problem which is an essential prelude to its correct solution.

What, then, is the topos-theoretic outlook? Briefly, it consists in the rejection of the idea that there is a fixed universe of "constant" sets within which mathematics can and should be developed, and the recognition that the notion of "variable structure" may be more conveniently handled within a universe of *continuously variable* sets than by the method, traditional since the rise of abstract set theory, of considering separately a domain of variation (i.e. a topological space) and a succession of constant structures attached to the points of this domain. In the words of F. W. Lawvere [LB], "Every notion of constancy is relative, being derived perceptually or conceptually as a limiting case of variation, and the undisputed value of such notions in clarifying variation is always limited by that origin. This applies in particular to the notion of constant set, and explains why so much of naïve set theory carries over in some form into the theory of variable sets". It is this generalization of ideas from constant to variable sets which lies at the heart of topos theory; and the reader who keeps it in mind, as an ultimate objective, whilst reading this book, will gain a great deal of understanding thereby.

Next, a few words on some of the things which I have not done in this book.

(1) In the definition of a topos, I have taken cartesian closedness and the existence of Ω as two separate axioms, instead of combining them into a single axiom of power-objects as suggested by A. Kock [66]. (The equivalence of Kock's axiom is, however, covered in the Exercises to chapter 1.) At a practical level, I would defend this decision on two grounds: (a) that there are a number of results in the book (notably in chapter 2) which use only cartesian closedness and not the full topos axioms, and some (e.g. Theorem 1.47) where exponentials and Ω are used in essentially different ways in the same proof; and (b) that if one takes the power-object definition, one is obliged (as in [WB]) to follow it immediately with the rather technical proof that this definition implies cartesian closedness, and one is in danger of losing one's readers at this critical point. On a more philosophical level, I would add (c) that the definition via power-objects is really a set-theorist's rather than a category-theorist's definition of a topos, in that it subordinates the notion of "function" to that of "subset" by means of the set-theoretic device of identifying functions with their graphs. One of the principal features of category theory is that it takes "morphism" as a primitive notion, on a level with (*not*, incidentally, superior to) that of "object"; it is therefore right that the definition of a topos should include its closed structure.

(2) I have not introduced the Mitchell–Bénabou language until rather late in the book, at the end of chapter 5. I know that there are some people whose ideal textbook on topos theory would begin with the definition and just enough development of exactness properties to introduce the language and prove the soundness of its interpretation; thereafter all proofs would be conducted within the formal language. I do not agree with this approach; I believe that it is impossible to appreciate the full power of the Mitchell–Bénabou language until you have had some experience of proving things without it (indeed, this is almost the only place in the book where I have consciously followed a particular ordering of material for pedagogical rather than logical reasons). There is also the point that the formal-language approach breaks down when confronted with the relative Giraud theorem (4.46); whilst the Mitchell–Bénabou language is a very powerful tool in proofs within a single topos, it is not well adapted to proofs in which we have to pass back and forth between two toposes by a geometric morphism. (It is possible that the proof of 4.46 could be shortened by using the language of locally internal categories, but that is a different matter.)

(3) I have already mentioned that Freyd's embedding theorem [FK] will not be found in this book. In consequence, Freyd's concept of well-pointed topos plays a relatively minor role; it is not introduced until §9.3.

(4) I have not included any reference to Freyd's more recent development (unpublished as yet) of the theory of *allegories*. This theory sets out to do for the category of sets and relations what topos theory does for sets and functions; Freyd has been known to maintain that it provides a simpler and more natural basis than topos theory for many of the ideas developed in this book, but I personally remain unconvinced of this.

(5) I have not mentioned the work being done by D. Bourn [13], R. Street [113], [114] and others, on the development of a 2-categorical analogue of topos theory. It appears to me, however, that the fundamentals of this theory have not yet reached a sufficiently definitive state for treatment in book form.

(6) One generalization of topos theory whose omission I do slightly regreat is J. Penon's notion of *quasitopos* [99]. However, I feel that to introduce it early in the book would simply have introduced extra complications in the proofs without any benefits in the form of additional well-known examples; and to introduce it later on would have involved a good deal of duplication. I hope, nevertheless, that O. Wyler's forthcoming notes on quasitoposes (promised in [130]) will help to fill this gap.

(7) The phrase "Grothendieck universe" does not appear anywhere in the book. This is intentional; I have deliberately been as vague as possible (except in §9.3) about the features of the set theory which I am using, since it really doesn't matter. Topos theory is an elementary theory, and its main theorems are not—or ought not to be—dependent on recondite axioms of set theory. (In fact I am a fully paid-up member of the Mathematicians' Liberation Movement founded by J. H. Conway [157].) If pressed, however, I would admit to using a Gödel–Bernays-type set theory having a distinction between small categories (sets) and large categories (proper classes); but I also wish to consider certain "very large" 2-categories (notably $\mathfrak{Cat}$ and $\mathfrak{Top}$) whose objects are themselves large categories. If I wished to be strictly formal about this, I should need to introduce at least one Grothendieck universe; but since all the statements I wish to make about $\mathfrak{Cat}$ and $\mathfrak{Top}$ are (equivalent to) elementary ones, there is no *real* need to do so. In order to retain some set-theoretic respectability, I have limited myself to considering sheaves only on small sites; this has the disadvantage that we cannot state Giraud's theorem in its slickest form (a category is a Grothendieck topos iff it is equivalent to the category of canonical sheaves on itself), but is otherwise not as irksome as the authors of [GV] would have us believe.

Finally, I have to state my position on the most controversial question in the whole of topos theory: how to spell the plural of topos. The reader will