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Preface

It was in the summer of 1977 that it first occurred to me that there was no single text where one could obtain a balanced view of all the mathematical consequences that have flowed from the Stone Representation Theorem, and that it would be useful to have such a book. At that time, however, I did relatively little to pursue this idea; the only thing that I wrote down was a tentative list of chapter headings, which bore relatively little resemblance to the book which eventually emerged. I made a more serious start in the autumn of 1978, when I gave a Part III (graduate) course in Cambridge entitled 'Stone Spaces'; this covered most of the material in chapters I–IV (and would have covered more, but for lack of time). I had the opportunity to recycle a good deal of the material from chapters II and III in January 1979, as part of a course on 'Internal and External Locales' which I was invited to give at the Université Catholique de Louvain in Belgium; but that course went on to consider topos-theoretic applications of locales (written up in [Johnstone 1979]) which were never intended to form part of this book.

The text of the first three chapters (except for section III 4) was written up in the summer of 1979; in the autumn which followed, I gave a continuation of the 'Stone Spaces' course (to a subset of the original audience) which covered most of the material in chapters VI and VII. The writing of the remainder of the text was largely done in the two succeeding summers: chapter IV and sections III 4 and V 1 in 1980, and the rest in 1981. After the text was completed, but before the typesetting began, I had the opportunity to polish up one or two points as a result of a further course of lectures during my sabbatical at McGill University, Montreal, in the winter of 1982.

In writing a book of this kind, one inevitably accumulates more debts of gratitude than can be repaid in a short Preface. My first debt is to the audiences of the lecture-courses mentioned above: particularly to my colleague Martin Hyland and my student Andrew Pitts, whose unfailing enthusiasm for the project did a lot to keep me going; to Francis Borceux, who was responsible for inviting me to Louvain-la-Neuve; and to Michael Barr and Marta Bunge, who invited me to Montreal. Out of the many people who have contributed to my own education in the subjects covered in this book, it would be entirely invidious to select only three; but I shall do this by naming Bernhard Banaschewski, John Isbell and André Joyal. The influence of the first two can be gauged by the frequency with which their names appear in the Notes at the ends of chapters, and by the length of their entries in the Bibliography; Joyal's contribution cannot be similarly measured by what he himself has written, but his influence on my thinking about locales is nonetheless profound (see [Johnstone 1983]).

Preliminary copies of the typescript were circulated to a number of colleagues (including Michael Fourman, Rudolf Hoffmann, Dana Scott, Harold Simmons and Myles Tierney), several of whom offered valuable comments and suggestions for improvement; in this context I must particularly thank Saunders Mac Lane for his expert advice on historical matters. (However, neither he nor any of the others mentioned should be held responsible for any errors which remain; they are mine alone.) Finally, and by no means least, I have to record that my life has been enriched, since I began working on this project, by getting to know Marshall Stone personally. His courteous hospitality, and his keen interest in the present-day descendants of his fundamental theorems of the 1930s, have meant a great deal to me.

It remains only for me to thank David Tranah and his colleagues at Cambridge University Press for their efficiency in the production of the book, for their willing acceptance of all my unreasonable demands in matters of style, and for their meticulousness in keeping me in touch with all stages of the production process – despite the best efforts of the Canadian Post Office to frustrate them during my stay in Montreal.

Cambridge, July 1982 P.T.J.

Advice to the reader

Like a great many research-level books in mathematics, this one is an uneasy compromise between a textbook for the student and a reference work for the specialist. The specialist will presumably need no help in finding what he wants from the book (assuming it's here at all); so these remarks are primarily addressed to the student, or to the lecturer who might be considering using the book as the basis for a graduate course.

First, prerequisites: the reader is presumed to know about as much algebra and general topology as he might have been expected to pick up in a British undergraduate course. In particular, he is presumed (in chapters IV and V, at least) to have some familiarity with commutative rings; but on the other hand, the treatment of lattices is entirely self-contained. (However, the book should not be regarded as a textbook on lattice theory – it misses out far too many important concepts, in particular that of modularity.) The treatment of categories (which are used freely throughout the book) is *not* self-contained; a student who has not met categories before will have to do some background reading to flesh out the bare bones in section I 3. Nevertheless, it would be possible for a course based on the book to proceed simultaneously with a first course in category theory; I should hope that the two would reinforce each other to a large extent. Similar remarks apply to sheaf theory, which is used only in chapter V; the first section of this chapter is not a self-contained introduction to sheaves, but in conjunction with a first course in sheaf theory it should be sufficient to unlock the rest of the chapter.

The numbering system used is rather old-fashioned: each chapter is divided into four sections (a sheer coincidence), and each section is divided into a number (between 5 and 17, but usually around 10) of 'paragraphs', each of which can be regarded as the working-out of a particular idea.

The theorems, lemmas, corollaries, etc. are not numbered; but since every paragraph contains at most one of each, they can be referred to by their paragraph numbers. Thus 'Theorem III 2.4' means the unique theorem in paragraph 4 of section 2 of chapter III. (For references within a given chapter, the chapter number is omitted.)

To try to cope with the conflicting demands of student and specialist, I have labelled certain paragraphs as 'secondary', particularly in the first three chapters. These contain material which is included either for the sake of completeness or because it is going to be needed in a later chapter, but which may be omitted at a first reading without damaging the continuity of the narrative. They are distinguished by being marked with an obelus (†), and printed in slightly smaller type. In the last four chapters, there are a number of paragraphs of primary material which depend on secondary paragraphs in the first three chapters; obviously, when one encounters one of these, the remedy is to go back and read the material which was omitted earlier. (For example, on reaching paragraph IV 2.4 it will be necessary for anyone who has previously omitted paragraphs II 3.5–3.7 to go back and read them.) There is only one comparable instance within the first three chapters, where Lemma II 2.8 is used in the proof of Corollary III 1.3; unfortunately it proved impossible to rearrange the material to avoid this, but the earlier lemma can easily be read out of context.

The exercises are scattered throughout the text, instead of being segregated at the ends of chapters. This is because they are really an integral part of the text, and should be regarded as compulsory for all readers – the result of an exercise is frequently used in a proof in the very next paragraph. For this reason, hints are given for the solution of all but the most routine ones.

Because of the diverse nature of the material covered, the logical dependence relation between the chapters is much more fragmentary than is usually the case. The 'compulsory core' of the book consists of the Introduction and the first two chapters, which are prerequisites for all that follow; thereafter, chapter III is a prerequisite for chapters IV and VII, but that is about all. (There are quite a number of cross-connections between chapters IV and V, and a lesser number between chapters VI and VII; but in each case it would be possible to read the later chapter without having read the earlier one.)

The Bibliography is quite extensive, but even so it does not claim to be a comprehensive listing of all the papers relevant to topics covered in the book. References to the Bibliography are by the name(s) of the author(s) and date of publication, enclosed in square brackets; except that when the author's name occurs naturally in the sentence where the reference is made, only the date is given in brackets. Where the Bibliography lists more than one publication by a given author in a given year, suffix letters are used to distinguish the second and subsequent ones. As far as possible, the dates given are those of first publication (except where specific reference is made to a second or later edition); but details of second editions, translations, etc. are given in the Bibliography itself where appropriate. The year 1984 is used as a conjectural publication date for forthcoming papers about which no more precise information is known.

This final paragraph is addressed primarily to logicians. There is little that is overtly logical in this book, except in chapter V where a certain amount of first-order logic is inescapable. This doesn't imply that I am uninterested in logic; on the contrary, I regard it as one of the most important features of the theory of locales that it enables one to give constructively valid proofs of many results whose counterparts in point-set topology are essentially non-constructive. However, I don't see the need to clutter up a book about mathematics with a lot of references to the logical framework within which one is doing the mathematics: if an argument is constructively valid (and where possible, my arguments usually *are* constructively valid), a professional logician will not need to be told this, whereas the sort of hard-nosed 'working mathematician' who regards logic like a disease will not thank you for telling him anyway. (I hope that he might, however, notice the fact that a constructively valid proof of a given theorem is generally more elegant than one which relies heavily on the law of excluded middle; constructivity is almost as much a matter of style as of logic.) On the other hand, I have not been able to prevent a certain obsession with the axiom of choice from breaking through, particularly in the Notes on the first four chapters. Within the main text of the book, those theorems, lemmas, etc. whose proofs require (some form of) the axiom of choice are distinguished by being marked with an asterisk; I hope that this will not prove distracting to those who don't want to be bothered with such things.

Introduction
Stone's Theorem in historical perspective

This book is about a particular theorem – the Stone Representation Theorem for Boolean algebras – and some of the mathematical consequences which have developed from it in the last 45 years. Inevitably, the author of a book which sets out to chart the development of a mathematical idea in this way is faced with the necessity of compromising between two approaches: the historical, in which one attempts to follow each strand of the development in more or less chronological order (but perhaps misses some of the interconnections *between* the various strands), and the logical or 'genetic' [Mac Lane 1980], in which one uses hindsight to take the most economical and painless route to the main results (but thereby loses some insight into *why* these results ever came to be seen as important).

The particular compromise which I have adopted is to go fairly wholeheartedly for a logical approach in the text itself (the route by which we shall eventually arrive at the proof of Stone's Theorem in section II 4 will strike historically-minded readers as perverse, to say the least), but to begin the book with an Introduction which attempts, first to set the Representation Theorem in the historical context in which Stone proved it, and then to indicate what those subsequent developments were, which led to the point at which the line of exposition I have adopted can be seen to be (as I believe it to be, anyway) an efficient and unifying way of covering a certain rather diverse body of mathematical knowledge. (To reinforce the message of this Introduction, there are also sections of historical and bibliographic notes at the end of each chapter.)

Our historical survey begins with the birth of abstract algebra, which has recently been documented by Saunders Mac Lane in an admirable essay [1981]. Mac Lane traces the first clear instance of an abstract/axiomatic approach to algebra to a paper of Cayley [1854] on group theory. However, group theory was not in the forefront of the drive towards abstraction in algebra which occurred in the early years of this century; perhaps this was because Cayley's representation theorem [1878], by showing that every abstract group was abstractly isomorphic to a 'concrete' group of substitutions (= permutations), removed the need for any abstract development of group theory until a much later date.

If group theory is the oldest branch of abstract algebra, Boolean algebra has a good claim to be the second. Of course, Boole [1847, 1854] and Peirce [1880] were really only concerned with concrete algebras of propositions (or of classes), but Whitehead [1898] and Huntington [1904] both took an abstract approach. However, there seems to have been little interest in non-Boolean lattices before 1930 (apart from the remarkable papers of Dedekind [1897, 1900], which, however, were again concerned with concrete lattices – in this case lattices of ideals), and little development even of the Boolean theory beyond mere juggling with axioms.

Now although Cayley's representation theorem may have delayed the development of abstract group theory, it did at least stabilize the axioms of the subject by demonstrating that they were indeed sufficient to capture 'the algebra of substitutions'. In Boolean algebra, there was a clear need for a similar representation theorem to show that the axioms had captured 'the algebra of classes'; but it was not immediately forthcoming.

Of course, we should not expect such a theorem to say that every Boolean algebra is isomorphic to the algebra of *all* subsets of some set; for just as full permutation groups have certain group-theoretic properties not shared by all groups (for example, if we exclude the group of order two, the property of having trivial centre), so there are lattice-theoretic properties enjoyed by full power-set algebras but not by all Boolean algebras. Let us briefly consider two of these.

In the algebra of all subsets of a set we have, in addition to the binary operations of union and intersection (which are represented by the lattice operations $\vee$ and $\wedge$), the additional possibility of forming unions and intersections of *infinite* families of subsets. We say that a lattice is *complete* if it has infinitary operations $\bigvee$, $\bigwedge$ corresponding to these set-theoretic ones; it is easy to give examples of Boolean algebras which are not complete. Again, in the full power-set PX of a set X, the singleton subsets $\{x\}$, $x \in X$, play a special role: they are not equal to the least element $\emptyset$, but there is nothing strictly between them and $\emptyset$ – equivalently, $\{x\}$ cannot be represented as a union of strictly smaller subsets. An element of a Boolean algebra with this property is called an *atom*; the abundance of atoms in PX is expressed by the fact that, for every $Y \neq \emptyset$, there exists an atom Z with $Z \subseteq Y$. A Boolean algebra with this property is called *atomic*; again, it is easy to give examples of non-atomic Boolean algebras.

Now let B be an abstract Boolean algebra, and let X denote the set of all atoms of B. We may define a map $\phi : B \to PX$ by setting $\phi(b) = \{x \in X | x \leqslant b\}$. It follows easily from the definition of an atom that an atom x satisfies $x \leqslant b \vee c$ if and only if either $x \leqslant b$ or $x \leqslant c$; from this we may deduce that ϕ is a homomorphism of Boolean algebras. Moreover, ϕ is one-to-one if B is atomic, since if $b \neq c$ then the symmetric difference $b \,\Delta\, c$ lies above some atom, which will be in just one of $\phi(b)$ and $\phi(c)$; and ϕ is surjective if B is complete, since then any subset Y of X is the image under ϕ of its join in B. Thus we have proved

Theorem

A Boolean algebra is isomorphic to the algebra of all subsets of some set if and only if it is complete and atomic.

This theorem was first proved by the logicians A. Lindenbaum and A. Tarski (see [Tarski 1935]), and it is clearly an important step towards a general representation theorem. However, it still leaves us powerless to deal with Boolean algebras which are not atomic; some new idea is needed.

At this point there enters the figure of Marshall Stone. Significantly, Stone was neither an algebraist nor a logician; his main work had been in functional analysis, with the study of linear operators in Hilbert space [1932]. It was his work in this area, on the spectral resolution theorem, which led him to the consideration of *algebras of commuting projections* in Hilbert space; it was known that these could be given the structure of Boolean algebras, but they had no natural representations as algebras of subsets. The representation theorem thus became a tool of practical importance to Stone; at the same time, his background in functional analysis gave him a greater familiarity with the new methods being developed in general topology than was available to most algebraists.

(In [1938], Stone sums up his attitude: 'A cardinal principle of modern mathematical research may be stated as a maxim: "One must always topologize"'. But some algebraists were slow to learn this maxim: in 1946 [Hochschild 1947], Garrett Birkhoff was content to define algebra as 'dealing only with operations involving a finite number of elements'; and when challenged by Artin, Mac Lane and others on the importance of topological methods, he replied 'I don't consider this algebra, but this doesn't mean that algebraists can't do it'. Incidentally, Birkhoff had independently arrived at a representation theorem for distributive lattices [1933] which was equivalent to Stone's; but because he missed the topological significance of the theorem, his version had far less influence on the later development of the subject.)

Stone's work on Boolean algebras was published in two long papers in the Transactions of the American Mathematical Society [1936, 1937], though summaries of some of his results had appeared earlier [1934, 1935]. There were two key ideas in Stone's work: one was his realization (described vividly by Mac Lane [1981]) that a Boolean algebra is *the same thing* as a particular sort of ring (namely, one in which every element a satisfies $a^2 = a$). Nowadays, when the equivalence of Boolean rings and Boolean algebras is something that we set as an exercise to undergraduates in their first course on ring theory, it is hard to understand how this fact remained undiscovered for so long. (Actually, Stone's first work in 1932–3 was based entirely on an *informal* analogy with ring theory, and it was not until 1935 that he realized the connection could be made formal; this necessitated the rewriting of a large part of his work on the subject, which explains the delay in publication of his results.) At any rate, the analogy with rings led Stone to a realization of the importance of ideals (and particularly prime ideals) in lattice theory; it is the set of prime ideals of a Boolean algebra which provides the carrier set for Stone's representation. (Notice the contrast with the Lindenbaum–Tarski representation, in which the carrier set is composed of *elements* of the Boolean algebra.)

Stone's second key idea was the introduction of topology. He observed that the set of prime ideals of a Boolean algebra can be made into a topological space in a natural way, in which the open sets correspond to arbitrary ideals of the algebra. (Specifically, to an ideal I we associate the open set of all prime ideals which do not contain I.) In this topology, the *clopen* sets (those which are both open and closed) correspond to principal ideals, and hence to elements of the algebra; so we can recover (an isomorphic copy of) the original algebra from its space of prime ideals.

Now this was a really bold idea. Although the practitioners of abstract general topology (notably the Polish school of Sierpiński [1928], Kuratowski [1933], *et al.*) had by the early thirties developed considerable expertise in the construction of spaces with particular properties, the motivation of the subject was still geometrical – the study of subsets of Euclidean space, and spaces constructed therefrom – and (so far as I know) nobody had previously had the idea of applying these techniques to the study of spaces constructed from purely algebraic data such as a Boolean algebra.

However, Stone went ahead and did just that. Of course, given any topological space X, the clopen subsets of X are closed under finite union, intersection and complementation, and so form a sub-Boolean-algebra of PX. But Stone showed that the spaces which arise as the prime ideal spaces of their Boolean algebras of clopen subsets can be characterized in purely topological terms, as being compact, Hausdorff and totally disconnected. (He called such spaces 'Boolean spaces'; subsequent authors have chosen to honour him by christening them 'Stone spaces'.)

Moreover, any homomorphism of Boolean algebras gives rise to a continuous map in the opposite direction between their prime ideal spaces; and any continuous map of Stone spaces gives rise to a homomorphism in the opposite direction between their clopen-set algebras. The constructions 'prime ideal space' and 'clopen-set algebra' are thus examples of (contravariant) *functors*; and together they form one of the earliest nontrivial examples of an *equivalence of categories*. All this was proved in detail by Stone, although the categorical language in which we now express it was not introduced until the following decade; but Stone's Theorem was undoubtedly one of the major influences which prepared the mathematical world for the introduction of categories by Eilenberg and Mac Lane [1942, 1945]. At any rate, the *meaning* of the equivalence was clear: it meant that any algebraic fact about Boolean algebras could be translated into a topological fact about Stone spaces, and *vice versa*. The way was thus immediately open for developing applications of Stone's Theorem in both algebra and topology.

In fact the first applications were in topology and functional analysis. Two of them were already present in Stone's [1937] paper: his construction of the maximal compactification of a completely regular space, and his generalization of the Weierstrass approximation theorem. The Stone–Čech compactification was of course discovered independently (see [Stone 1962]) by Stone and by Čech [1937], but the methods of the two were substantially different. Čech's work can be seen as a natural extension of the work of Urysohn [1925a] and Tychonoff [1929] on embedding spaces in products; in fact his construction was a relatively simple development of that used by Tychonoff in his proof that completely regular spaces are precisely the subspaces of compact Hausdorff spaces. In contrast, Stone's construction used algebraic properties of the ring $C^*(X)$ of bounded continuous real-valued functions on the space X. This in turn raised the problem of characterizing $C^*(X)$ in algebraic terms – a problem which was again solved independently by two people, Stone [1940, 1941] working with real-valued functions, and Gelfand [1939, 1941] with complex ones, thus laying the foundations of the important subject of Gelfand duality. (See also [Kakutani 1940, 1941], [Krein and Krein 1940, 1943], [Kaplansky 1947, 1948a], [Milgram 1949], [Anderson and Blair 1959], [Gillman and Jerison 1960], [Henriksen, Isbell and Johnson 1961], [Mulvey 1978a, 1979a], etc.)

Stone's [1937] construction of his compactification also contained a detailed proof of its universal property – once again, as noted by Mac Lane [1970], anticipating an important trend in category theory. It can thus be considered as a first step in the 'algebraization' of the category of compact Hausdorff spaces, a process brought to a successful conclusion by Manes' [1969] proof (which relied heavily on Stone's compactification) that this category is indeed algebraic in the technical sense. (See also [Edgar 1973], [Semadeni 1974], and [Manes 1980].)

Another direction of application was initiated by Stone in [1937a]; in considering the topological equivalent of the condition of completeness for Boolean algebras, he introduced the important notion of *extremal disconnectedness*. Further work on extremally disconnected spaces [Stone 1949] confirmed their importance in functional analysis, and led up to the work of Gleason [1958], Rainwater [1959], Iliadis [1963], Banaschewski [1967, 1971] and Dyckhoff [1972, 1976] on projective topological spaces – once again, importing ideas from algebra (in this case, homological algebra) into categories of topological spaces. More recently, Johnstone [1979b, 1980a, 1981] has pointed out the sheaf-theoretic and logical ideas underlying this connection.

In yet another paper published in 1937 [1937b], Stone generalized his representation theorem to non-Boolean distributive lattices, at the same time introducing the non-Hausdorff cousins of Stone spaces which we now call *coherent spaces*. Although these have received less subsequent attention than Stone spaces, the work of Hochster [1969], Priestley [1970, 1972] and Joyal [1971, 1971a] is worth mentioning. In another direction, [Stone 1937b] paved the way for the study of topological concepts from a lattice-theoretic viewpoint, initiated by Wallman [1938] and pursued by McKinsey and Tarski [1944], Nöbeling [1954], Lesieur [1954], Ehresmann [1957], Papert [1964], Dowker and Papert [1966], Isbell [1972] and Simmons [1978], among others. (A close relative of this line of development is the study of topological posets and lattices: [Frink 1942], [Nachbin 1950], [Ward 1954], [Anderson 1959, 1961, 1962], [Strauss 1968], [Choe 1969], [Lawson 1969, 1970, 1973], [Scott 1972], [Hofmann and Stralka 1976], [Semilattices 1980], etc.)

In recent years, the lattice-theoretic approach to topology has merged in the study of general sheaf theory and topos theory. The origins of sheaf theory [Leray 1945, 1946], [Cartan 1949] owe little if anything to the work of Stone; but the generalized sheaf theory pioneered by Grothendieck and his followers around 1963 [Giraud 1963], [Verdier 1964], and still more the elementary theory of toposes introduced by Lawvere and Tierney in 1970 [Lawvere 1971], [Tierney 1973], have increasingly focused attention on the fact that the important aspect of a space (from a sheaf-theoretic point of view) is not its set of points but its lattice of open subsets. (This is not the place for a detailed history of sheaf theory or topos theory; we refer the reader to [Gray 1979] and the Introduction to [Johnstone 1977].)

Another area where the influence of Stone's work has been strongly felt is the representation theory of rings and more general algebraic systems. The foundation-stone of this theory is Birkhoff's subdirect decomposition theorem [1944], which displays none of the influence of Stone's topological ideas, but it was soon realized [Jacobson 1945], [Arens and Kaplansky 1948] that much sharper representation theorems could be obtained by introducing topologies in the fashion of Stone's Theorem. Further important work in this direction was done by Gillman [1957], Henriksen and Jerison [1965a], Pierce [1967], Dauns and Hofmann [1968], Keimel [1971], Hofmann [1972], Davey [1973] and Cornish [1977]; in recent years this line, too, has developed strong links with topos theory [Mulvey 1974, 1979], [Kennison 1976], [Tierney 1976], [Johnstone 1977a], [Coste 1979].

It remains to consider two areas of mathematics in which, like the dog in the night-time [Doyle 1892], the influence of Stone's Theorem is more conspicuous by its absence than by its presence. One of these is category theory itself. Mac Lane [1970] has pointed out how the categorical ideas present in Stone's [1937] paper were not directly followed up by the founders of category theory: in particular, the notion of adjoint functor, though present implicitly in Stone's description of his compactification, and strongly suggested by the functional-analytic notion of adjoint operator, was not explicitly introduced into category theory until 1958 [Kan 1958]. Stone himself [1970] has analysed the reasons for this failure, pointing out that the algebraic and algebraic-topological background of the pioneers of category theory naturally meant that the proto-categorical ideas arising from general topology and functional analysis did not form a part of their experience.

(However, it should not be thought that Stone's work has had *no* influence on category theory. There is one area in particular – the categorical study of duality theorems – which, whilst it also owes a good deal to the duality theory of Pontryagin [1934] and van Kampen [1935], draws a very large part of its inspiration from the duality theorems of Stone and Gelfand already mentioned. For work in this area, see [Hofmann 1970], [Isbell 1972a, 1974], [Hofmann, Mislove and Stralka 1974], [Keimel and Werner 1974], [Lambek and Rattray 1978, 1979], [Lawson 1979] and [Barr 1979].)

The other area where one searches in vain for the influence of Stone's Theorem is in algebraic geometry, with the rise of the 'Zariski topology'. It was sometime in the late forties (see [Zariski 1952]) that O. Zariski realized how one might define a topology on any abstract algebraic variety, by taking its algebraic subsets as closed sets; the precise date is difficult to determine, since Zariski himself does not seem to have attached much importance to the idea. (There is no mention of the Zariski topology in the first edition of Weil's book [1946] on algebraic geometry, although it plays a central role in the second edition [1962].) It was not until the work of Serre [1955] that the Zariski topology became an important tool in the application of topological methods (in this case, sheaf cohomology) to abstract algebraic geometry. There is an obvious similarity between the topologies introduced by Zariski and Stone, and indeed Dieudonné [1974] asserts that Zariski was influenced by Stone's work; but there seems to be no acknowledgement of this influence in Zariski's own papers.

The refoundation of algebraic geometry using schemes in place of varieties, begun by Grothendieck [1959, 1960] in the late fifties, brought the Zariski and Stone topologies even closer together: indeed, the latter is just the special case of the former applied to the spectrum of a Boolean ring. But again, one will not find any reference to Stone in the work of Grothendieck, even though his use of the word 'spectrum' is an obvious echo of [Stone 1940], and Grothendieck, with his background in functional analysis, must have been familiar with Stone's work in that field. Again, when the Zariski topology made its first appearance in a book on commutative algebra, as opposed to algebraic geometry, [Bourbaki 1961a], there was no mention of Stone's name. (The Zariski topology does not occur in [Zariski and Samuel 1958].)

One area which has not been mentioned in this survey is mathematical logic. This is not because Stone's work has failed to have an influence here, but because until recently (if one discounts such papers as [Łoś and Ryll-Nardzewski 1954]) the full extent of that influence has rarely been made explicit. It is only since the rise of elementary topos theory, and the consequent interest in coherent logic ([Reyes 1974], [Makkai and Reyes