**Some remarks on research in Mathematics**
**Extract from a Preface to**
**'Topology and Groupoids', by Ronald Brown (2006)**
**available from amazon.com**

I will be pleased if the exposition of this book can be improved in radical ways. Young mathematicians should be aware of the temporary mature of mathematical exposition. The attempt to form a 'final view' reminds one of the schoolboy question: what would happen if you laid worms in a straight line from Marble Arch to Picadilly Circus? Answer: one of them would be bound to wiggle and spoil it all.

Some might argue that the groupoid worm has here not only wiggled from its accustomed place in topology, but become altogether too big for its boots, to which a worm, after all, has no rights. But I hope many will find it interesting to trace through this first attempt to answer, in part, the questions: Is it possible to rewrite homotopy theory, substituting the word groupoid for the word group, and making other consequential changes? If this is done, is the result more pleasing?

These questions, both of the form 'What if...?', came to acquire for me a force and an obsession when pursued into the topic of *higher homotopy groupoids*. The scribbling of countless squares and cubes and their compositions lead to a conviction in 1966 that the standard group theory, once it was rephrased as a groupoid theory, had a generalisation to higher dimensions. Gradually, collaborations with Chris Spencer in 1971–1974, with Philip Higgins since 1974, with J.-L. Loday from 1981, and work of my research students, A. Razak Salleh, Keith Dakin, Nick Ashley, David Jones, Graham Ellis and Ghaffar Mosa, at Bangor, and Philip Higgins' research students Jim Howie and John Taylor at London and Durham, made the theory take shape. In this way a worry of the algebraic topologists of the 1930s as to why the higher homotopy group were abelian, and so less complicated than the fundamental group, came to seem a genuine question. The surprising answer is that the higher homotopy *groupoids* are non-abelian, and are just right for doing many aspects of homotopy theory. In particular, they satisfy a version of the Van Kampen theorem which enables explicit and direct computations to be made. It will be interesting to see if the higher dimensional theory will come to bear a relation to the standard group theory similar to that of many-variable to one-variable calculus.

But the higher dimensional theory is a story in which we cannot embark in this book. We now give the changes that have been made in this new edition.

A section on function spaces in the category of $k$-spaces has been added the chapter 5. One of the reasons is that the material is quite difficult to find elsewhere. Another reason for its inclusion is that it will suggest to the reader that there are still matters to be decided on the appropriate setting for our intuitive notions of continuity. In any case, the generalisation from spaces to $k$-spaces makes the proofs if anything simpler.

I am grateful to a number of people for comments, particularly Eldon Dyer and Peter May who suggested that chapter 7 needed clarification, and Daniel Grayson who suggested the notation $[(X, i), (Y, u)]$ now used in chapter 7 to replace the original $X//u$, which was non-standard and too brief. (But this double slash is used in a new context in chapter 9.) In the event, chapter 7 has been completely revised to make use of the term *cofibration* rather than HEP. The idea of *fibration of groupoid*, which came to light towards the end of the writing of the first edition and so appeared there only in an exercise, has now been incorporated into the main text. However, I have not included fibrations of spaces, since to do so would have enlarged the text unduly, or force the omission of material for which no other textbook account is available.

In chapter 8, an error in section 8.2 has been corrected. Also free groupoids have now been used to define the notion of path in a graph. An exercise on the computation of the fundamental group of a union of non-connected spaces has now been incorporated into the text, as an illustration of the methods and because of its intrinsic importance. This result is used in a new section which gives a proof of the Jordan Curve Theorem, and some new results on the Phragmen-Brouwer Property.

In chapter 9, section 9.4 on the existence of covering groupoids has been rewritten to give a clearer idea of the notion of action of groupoids on sets. Section 9.5 includes some results on topologising the fundamental groupoid. The relation between covering spaces of $X$ and covering groupoids of $\pi X$ has been clarified by adding section 9.6, which gives an account of the equivalence of the categories of these objects. This enables an algebraic account of the theory of regular covering spaces. Section 9.7 gives a new account of pullbacks of covering spaces and covering morphisms, using exact sequences. Section 9.8 gives an account of the Nielsen-Schreier and Kurosch subgroup theorems, using groupoid versions, due to Hasse and Higgins, of the more traditional covering space proofs.

Sections 9.9 to 9.10 give the first account in any text of the theory of the fundamental groupoid of an orbit space. This uses work of John Taylor an Philip Higgins, and is a good illustration of the utility of the groupoid methods, since a group version is considerably more awkward in the statement of results and in the proofs. I am grateful to Ross Geoghegan who, in a review of a paper by M. A. Armstrong, pointed out the desirability of having Armstrong's results on the computation of the fundamental group of an orbit space available in a text.

The Bibliography has been extended for this edition. It would be impossible to make such a Bibliography complete, and I apologise in advance for any omissions or lack of balance. The main intention has been to show the subject matter of this book as part of a wide mathematical scene; occasionally it is used to give the reader an opportunity to explore some aspects which are not so well represented in texts; other times, the Bibliography is used to acknowledge clear debts. The Notes at the ends of chapters, and referential material in the text have the aim of giving an impression of what the subject is about, of how mathematics is an activity involving people, and that it is a subject in a state of active development, even in its foundations.

I would like to thank the following for helpful comments and criticisms: Philip Higgins, Alan Pears, Guy Hirsch, Peter May, Terry Wall, Frank Adams, Jim Dugundji, R. E. Mosher. I also thank all those who wrote and pointed out misprints and obscurities.

I would like to conclude this preface on a personal note, which I hope may be of use to readers starting on, or aspiring to, a career in mathematical research, and wondering what that might entail. There is little in print on the methodology of mathematical research. There is material on problem solving, but there is little on evaluation, on problem choice, or on problem and concept formulation. There is material on the psychology of invention, but not so much on the training and development of invention, nor so much on the ends to which one should harness whatever invention one has. There are autobiographies available, but some perhaps give the impression that to do research in mathematics it is helpful to be a genius in the first place.

At the time of writing the first edition of this book, I was not clear as to what direction I wanted to take in my research, and to some extent writing the book was a displacement activity, distracting attention from the necessity of decision. However, as the book progressed and I tried to make the exposition clear, difficulties in the subject began to emerge. As draft succeeded draft, I became clearer about what I did not know, and a new range of possibilities began to arise. The original intention was a standard exposition of known, but scattered, material. This turned into an idiosyncratic treatment which itself suggested a new research line which has kept me busy ever since.

So I would like to commend to readers the idea that writing and rewriting mathematics with an intention to make things clear, well organised and comprehensible, and perhaps with some particular formal changes in mind, may in itself be a stimulus to further mathematical activity. These ideas are confirmed by some remarks of the composer Maurice Ravel, who argued for copying from other composers. If you have something to contribute, then this will appear of itself, and if you have no new ideas in this area, then at the least you will have made things clear to yourself. Thus the quality of your understanding is improved. There is also the simple joy of a well-crafted work.

There is also an argument for the *quality of misunderstanding*. It may be easy for you to feel that you do not understand because you are stupid. But your lack of understanding may be a reaction to a lack of clarity in the current expositions, or a feeling that the current expositions require something more to carry conviction. These expositions may prove results, but not explain them in a way you find agreeable. The only way to tell is to try and write an account for yourself, and see if you can make the matter clearer. For example, the material on the gluing theorem in chapter 7 was my response to a lack of understanding of the complicated proofs in the literature of results such as 7.5.3 (Corollary 2). By the time of the final draft, I was clear why these results were true.

The account of groupoids which has dominated both editions of this book came about in the following way. It was annoying to have a Van Kampen theorem which did not compute the fundamental group of a circle. I found that the account of the Van Kampen theorem given by Olum could be generalised to yield this computation. So I started an exposition of Olum's non-abelian cohomology, with the laudable motivation that this would also introduce cohomological ideas to the reader. Unfortunately, when I looked at the 30 pages of my draft, I had to admit that they were pretty boring.

At the same period I had been pursuing some references on free groups. These lead me to an article by Philip Higgins, which introduced free products with amalgamation of groupoids. So it seemed reasonable to set an exercise on the fundamental groupoid $\pi_1(U \cup V)$ of a union of spaces. Since the result was not in the literature as such, it seemed reasonable to write out a solution. The solution turned out to have the qualities of elegance, concision and clarity which I had been hoping for, but had not obtained, in my previous account. This suggested that the exposition should be turned round to give groupoids a central rather than peripheral rôle, particularly in view of a warm reception to a seminar I gave on the topic to the London Algebra Seminar in 1965. In 1967, I met Professor G. W. Mackey of Harvard University who told me of his work using groupoids in ergodic theory. This suggested that the groupoid concept had a wider application than I had envisaged, and so I also worked up chapter 9 on covering spaces, emphasising the groupoid viewpoint. It is perhaps only now possible to see the many disparate strands of work in which the notion of groupoid is usefully involved.

I would like to acknowledge here the help of two people who started me on a mathematical career. I have a great debt to the late Professor J. H. C. Whitehead, who was patient with my hesitancies and confusions. His many successes in exposing the formalities underlying geometric phenomena are a background to this text, and one part of my aims was to give an exposition of his lemmas on the homotopy type of adjunction spaces. From him I also absorbed the attitude of not giving up a mathematical idea until its essentials had been extracted, whatever the apparent relevance or otherwise to current fashions.

My second debt is to Professor M. G. Barratt, who initiated me into the practicalities and impracticalities of mathematical research. As to the impracticalities, I once got a ten-page letter from him which ended: 'Dawn breaks; I hope nothing else does!' As to the practicalities, I remember thinking to myself after a long session with Michael: 'If Michael Barratt can try out one damn fool thing after another, why can't I?' This has seemed a reasonable way of proceeding ever since. What is not so clear is why the really foolish projects (such as higher homotopy groupoids, based on flimsy evidence, and counter to current traditions) have turned out the most fun.