Pais's private discussions seem to have been as "recent" in 2000 as they were in 1982 and 1986. The whole question of Born's interpretation has been analyzed in detail by Beller (1990), and as early as 1980 Linda Wessels pointed out that in none of his original collision papers did Born propose the interpretation that was later associated with his name (Wessels, 1980). Pais refers neither to Beller nor to Wessels. Another example of Pais's historiography is his brief treatment of the Konopinski-Uhlenbeck (KU) theory, an alternative to Enrico Fermi's theory of beta decay. After Jim Lawson had shown that the experimental beta spectrum agreed with Fermi's theory, "as a result the KU theory vanished from the scene" (p. 315). As shown by Franklin (1990), the story is much more complex, and the KU alternative was not simply disproved by Lawson's data. Perhaps needless to say, Pais does not refer to Franklin's work.

Of course I realize that citation standards differ in scholarly and non-scholarly works, but in a book with about one thousand references I see no reason why some of the best historical studies are not included. If for no other reason, it would simply be a help to the readers. In conclusion, Pais's last book will probably appeal to many physicists and lay readers. It includes, in some of its chapters, information and insights that cannot be easily found elsewhere, but on the whole the book has little to offer the serious student of the history of modern physics.

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Helge Kragh
History of Science Department
University of Aarhus
8000 Aarhus, Denmark
E-mail address: ivhhk@ifa.au.dk

PII: S1355-2198(02)00008-4

Conceptual mathematics: a first introduction to categories

F. William Lawvere and Stephen H. Schanuel, Cambridge University Press, Cambridge, 1997 (reprinted with corrections 1998), xvi + 358 pp., index, hbk, ISBN 0-521-47249-0, pbk, ISBN 0-521-47817-0

A primer of infinitesimal analysis

John Lane Bell, Cambridge University Press, Cambridge, 1998, pp. xiii + 122, bibl., index, hbk, ISBN 0-521-62401-0

Casting scientific or mathematical research activity in the form of programmes with a view to gauging its progressiveness is no straightforward business, as Imre Lakatos discovered. The category theory "programme", now over half a century old, has certainly become too large to be judged as a united enterprise whose members share a common mission. Speaking about Bayesian statistics, Edwin Jaynes could imagine a time when its methods had become so pervasive that its practitioners found their common interests insufficiently extensive to cause them to gather together for conferences, just as the time when researchers could meet up to discuss the uses of Fourier transforms is long past. Well, category theory has already progressed a certain way towards this stage. Its penetration into the various branches of mathematics has been uneven, but in some cases it has been profound. For instance, any algebraic topologist or algebraic geometer just must use a considerable amount of category theory as part of their job. It has also made inroads into logic and, from there, to theoretical computer science (see e.g., Taylor, 1999). Its reach even extends to mathematical physics where, for example, we find topological quantum field theories defined in terms of functors between categories (Atiyah, 1988), and the intriguing prospect that higher-dimensional categories will feature in subsequent developments.

Despite this widespread diffusion of category-theoretic constructions into the world of mathematics, there is a group of researchers who identify themselves as category theorists and who do still participate in conferences. What the holding of such meetings reflects is one end of a range of levels of commitment to category theory. Here we find a more daring vision of the role of category theory in the construction of mathematics and the mathematical sciences, one which has at times given outsiders the sense of a kind of fanaticism. What this stronger version maintains is that category-theoretic tools and principles should provide the guidance necessary to develop mathematics and its applications in the "right" direction. The most vocal advocate of this position, one which he has held over several decades, is William Lawvere, who in the mid-1960s took the bold step of formulating a category of categories as a new foundation for mathematics. Moreover, it was Lawvere's belief that category theory could be used to reformulate mathematical physics which led to the approach to analysis and differential geometry to which Bell's book provides an introduction.

Both of the books under review are essentially mathematics textbooks couched at an elementary level of exposition. While there is some explicit philosophical motivation contained in each, more perhaps than in a typical undergraduate text, it amounts to no more than a few pages in total. In this sense it may appear to the reader to be a little peculiar to review these works in a journal of this nature. I should then first explain my reasons for hoping that the exercise will prove worthwhile.

Despite its impressive past, the philosophy of mathematics has become something of a minority activity in the Anglophone world today, and the little that there is almost all cast in the logicistic mould. While the philosophy of physics carries out an extensive treatment of contemporary issues of relevance to mainstream theoretical physics, this is far from being the case in philosophy of mathematics. One may argue, of course, that the philosophical issues arising from the existence of mathematical knowledge are of a nature peculiar to that discipline. For instance, it might appear that the possibility of the reduction of mathematics to some or other system of higher order logic or set theory acts as a warrant to ignore geometry in all its many contemporary manifestations. But the price to pay for following this route is that the soul of modern mathematics lies uncharted, as a consequence of which philosophers of mathematics can offer nothing to their colleagues in philosophy of physics in a period when mathematicians and physicists are having such creative dialogues.

My hunch is that the path forward to a satisfactory philosophy of mathematics cannot avoid a serious encounter with category theory. Just as set theory owes its existence, to some degree, to problems in the representation of functions by Fourier series, category theory too came into being, several decades later, from very concrete problems in algebraic topology in the 1940s, and it continues to make contact with mainstream developments to this day. Furthermore, among the new perspectives it offers on foundational issues, category theory has important things to say about conceptualisations of space. I would, therefore, encourage philosophers to ensure that they are in a position to appraise the perspectives which motivate and surface periodically within the books under review. Let us now then turn to the books themselves, beginning with *Conceptual Mathematics*.

Lawvere and Schanuel's book is written as a handful of articles, interspersed by a series of sessions and exercises. The sessions take the form of a dialogue between a teacher and some bright pupils, a format which, from the *Meno* to *Proofs and Refutations*, has admirably served to convey the acquisition of mathematical knowledge. In these sessions, the class works at a leisurely pace through the denser material contained in the articles. This didactic setting strikes a chord with a theme that one often encounters in the writing of category theorists to the effect that, if only people were taught in a category-theoretic spirit from early on in their education, then the perception that the set-membership relationship is easier to grasp than the arrows and objects approach of category theory would be reversed. This book may be seen as the means to carry out the first step of the necessary experiment. The authors have thus had to strike a difficult balance between showing how categorical formulations capture constructions in "real" mathematics, while assuming very little mathematical knowledge on the part of the reader. This they do admirably well. However, as this introductory text has only scratched the surface of category-theoretic constructions, the aspiring conceptual mathematician may wonder where to turn next. Unfortunately, though, she faces a total absence of bibliographical assistance from the book. Normally, one gets referred to Mac Lane's (1998) *Categories for the Working Mathematician*. But, as the title suggests, this book works from the presumption that the reader knows enough mathematics to see the value of the constructions introduced. Help may be at hand, however, as I hear on the grapevine that Lawvere and Schanuel are in the process of producing a sequel.

There is plenty for the philosopher to chew over from the book's message that one must see the maps or arrows as no less fundamental than the objects. For example, in Session 6 the authors discuss how two different aspects of mappings manifest themselves through arrows within a category either to or from small objects. In the former case, one is dealing with the idea of sorting or classification, in the latter with the idea of sampling. These ideas are central to thinking, and mathematical thinking in particular. We think of a fibre bundle as sorting the total space into fibres, while a loop in a space samples that space for its connectivity.

As the title suggests, one of the key philosophical issues arising from the book concerns the nature of mathematical concepts. Mathematicians have complained for years that neither the syntactical investigations of the proof theorists nor the set-based semantics of the model theorists gives much insight into these concepts, which they claim is what mathematics is about. The authors of *Conceptual Mathematics* do not themselves attempt to define what they mean by a concept, but we may discern from the appearance of 'concept' in the index coupled with the term 'coconcept' what is intended. To give an idea to the uninitiated of what is at stake here, category theorists like to find what is common to a variety of situations. So, rather than define on each occasion what you mean by, say, the product of two sets, directed graphs, lattice elements, logical propositions, or whatever, better to define via patterns of arrows the concept of the product of two objects and then see whether and how different categories support this definition. The definition of the product of two objects, A and B, is an object, C, with arrows from C to A and C to B, which is 'universal' in this respect: that for any object D with arrows to A and B, there is a unique arrow from D to C which allows these arrows to factor through the product. The set-theoretic versions of product turn out to provide instances of category theoretic products of sets and graphs, which are only defined up to isomorphism, but the category-theoretic perspective also gives us the conceptual link to other products such as the meet of elements in a lattice and the conjunction of propositions.

One arrives at the dual (or *co*-) concept by the standard category theoretic construction of reversing the arrows. Thus, if the arrows in the definition of product are reversed we arrive at the concept of a *coproduct*, which corresponds to the notion of disjoint sum in the category of sets, but the more complicated notion of free product in many algebraic categories. This kind of unification of concepts can be continued by moving up a level of generality higher and viewing products as examples of limits, all of which are preserved by certain types of functors between categories. The question then arises for the philosopher as to the extent to which the category theoretic treatment succeeds in capturing what there is of the conceptual in mathematics.

In the final session of the book, we meet up with categories known as toposes, whose formulation owes much to Lawvere. Toposes are categories that can support higher-order constructive reasoning. Aside from their logical aspect, one should not forget their geometric origins (see McLarty, 1990 for a history of topos theory which argues forcefully against the view that toposes were devised merely as alternatives to the category of sets). The existence of toposes presents two possibilities. You can prove a result constructively, then see what its interpretation in various toposes amounts to. For example, a constructive result about diagonalising a real matrix when interpreted in the topos of sheaves over the real line amounts to a result about diagonalising a matrix of continuous real functions. The second way to benefit from your weaker logic is to postulate stronger axioms that are inconsistent with classical logic. You may then be able to find toposes in which these stronger axioms hold. An example of this situation is given by Brouwer's classically false axiom of continuous choice, from which he derives the theorem that all functions from the reals to the reals are uniformly continuous. There are toposes in which this axiom and theorem hold internally. An attractive picture thus emerges that mathematics should be carried out in the framework best suited to the subject matter at hand (see, e.g., Bell, 1986). So, for instance, in what is called the *effective* topos, one has all the advantages of higher-order logic, while being assured that all functions will be recursive.

Now, the programme to which Bell's *Primer* offers an introduction sought to find a suitable arena in which the smoothness of functions was assured. This required a topos in which a classically inconsistent axiom, the Kock–Lawvere axiom, holds. The theory postulates the existence of a commutative ring object, R, thought of as the line, together with two specified points 0 and 1. From R we can pick out a subobject of infinitesimals: $D = \{d \in R; d^2 = 0\}$, which in this theory will not be the singleton $\{0\}$. The Kock–Lawvere axiom states that there is a bijective correspondence between maps from D to R and pairs of elements of R, i.e., each such map is of the form $f(d) = a + bd$, with a and b in R, and different (a, b) corresponding to different maps. Informally, we think of D as being an interval of R about 0 large enough to define the gradient of a function at 0, but not large enough to be bent by any function.

It is easy to see that this axiom is inconsistent with classical logic. We just define the function $f(x) = 0$, if $x = 0$, and $f(x) = 1$ otherwise. We then have $f(0) = 0 = a + b0 = a$. Now, as there must be a non-zero $d \in D, 1 = f(d) = 0 + bd$. Squaring both sides $1 = b^2d^2 = 0$. The contradiction was caused by our being able to apply the law of excluded middle in the definition of f. The elements of D are, in fact, those elements of R not provably different from 0, i.e. $D = \{d \in R : \neg\neg(d = 0)\}$.

With these nilpotent infinitesimals in hand, one can reduce the calculus to a simple form of algebra. Bell's *Primer* sets out to instruct the reader in the use of this algebra. I shall not comment on the material contained in the chapters except to say that it includes topics one would expect to find in an introductory course to the calculus: areas and volumes, moments of inertia, the catenary, the heat equation and Euler's equations, Taylor's theorem, etc. What I would like to dwell on are two sets of issues arising from this reformulation of the known. These concern methodology and philosophy.

Let us begin with the methodological issues. Nilpotent infinitesimals do seem to allow for a much more intuitive development of differential geometry than does the modern set-theoretic version. While the naïve notion of a tangent vector imagines a straight line segment based at a point on a manifold brushing its surface, to formulate this notion rigorously without invoking the idea that the manifold is embedded in an ambient space, mathematicians have devised three equivalent, but rather artificial, devices. A tangent vector is either:

(a) an equivalence class of paths,
(b) an equivalence class of ordered pairs consisting of a chart and a vector, or
(c) a derivation, i.e., a certain type of map from the set of smooth real functions on the manifold to the reals.

Now, in contrast to these convoluted definitions, synthetic differential geometry views a tangent vector simply as a map from D to the manifold M. D is small enough that, although it is straight, it can still lie in the manifold like a splinter. A vector field then is easily defined as a type of map from $M \times D \rightarrow M$, which allows one to associate it with a mapping $D \rightarrow M^M$, an infinitesimal flow.

The possibility of such an intuitive formulation might suggest a bright future for the theory. This at least was how it seemed to the prominent mathematician Gian-Carlo Rota, who for several years as editor of the journal *Advances in Mathematics* wrote punchy, opinionated book reviews. Part of his review of a book by Moerdijk and Reyes (1991), in which models for the Kock–Lawvere axiom were constructed, runs as follows:

> One always had the feeling that "technical" assumptions in differential geometry were really a way of paying lip service to the bullying demands of analytic rigor. At last, the language has been found where we can talk about differential geometry without being forced to consider Hölder conditions, degrees of differentiability, and other extraneous notions. The bad news, of course, is that we are being required instead to express ourselves in the language of categories and topoi. But it seems that this language is here to stay, more so, at any rate, than the ponderous apparatus of hard analysis that for years has been restraining the free flight of our geometric intuition (Rota, 1991, p. 114).

Now, if 'the free flight of our geometric intuition' can be made possible, one would expect that the gains from adopting the new framework would be substantial. However, it should be noted that, as yet, few in the way of novel results have been achieved by this programme. Of course, this may be because its conceptual potential has yet to be properly exploited. After all, not so many people have worked on synthetic differential geometry, and many of them do not come from a background in differential geometry. But the thought may occur to the reader that, since non-standard analysis, despite some notable successes, has not found great favour with the mathematical community, there is no reason why synthetic differential geometry should fare any better. Might it be that despite the fact that a theory appears to offer considerable simplifications that what is being reformulated is sufficiently well understood by the expert in the old formalism? Is there an entrenched conservatism in the mathematical community which offers resistance to change? I believe there are some very interesting questions for the methodologist to tackle here.

Now let us address what I consider to be some broader issues for philosophy. We have almost come to expect as a right that developments in geometry should impact on physics. Just think of Riemannian geometry as it used in general relativity, connections on fibre bundles as the mathematicians' version of gauge field theory, and more recently Alain Connes' reformulation of the standard model using noncommutative geometry. So, if category theorists can demonstrate that a form of microlinear space is conceptually elegant and consistent, we might expect some ramifications in physical theory. To date, one could not say that topos theory has taken the world of physics by storm, although Isham and Butterfield (1998) have found uses for it in quantum mechanics, and a search of the Web shows that there are some physicists in Omsk who are using precisely the toposes appropriate for synthetic differential geometry to reformulate general relativity. My question concerns whether such applications are a prerequisite for philosophical treatment.

If a sequel to Torretti's (1978) fine *Philosophy of Geometry from Riemann to Poincaré* (1986) is ever written, you can be sure that it would be filled largely with discussions of general relativity. You might think that it is only right for the philosophy of geometry to be wrapped up with issues about the physical world, especially if you picture mathematicians as people playing games, only some of which physicists find interesting. But mathematicians do have profound reasons for developing their theories, which do not completely coincide with those of the physicists, even in cases where, like gauge field theory and fibre bundle theory, there is considerable overlap of content. Surely a philosophy of geometry should be interested also in the uniquely mathematical reasons for the development of geometry. To gain a more complete understanding of spatial conceptualisation, it must be right to understand both the physical and the mathematical motivation behind contemporary developments. However, as there is no philosophy of geometry within the philosophy of mathematics today, this is impossible, and so fibre bundle theory, noncommutative geometry, spin networks and foams, bundle gerbes, synthetic differential geometry, and all the rest go unobserved as pieces of mathematical theorising.

I shall conclude by returning to the two books. *Conceptual Mathematics* provides an excellent introductory account to categories for those who are starting from scratch. It treats material which will appear simple and familiar to many philosophers, but in an unfamiliar way. On the other hand, the audience for *A Primer of Infinitesimal Analysis* is harder to identify. It may serve to whet the reader's appetite, but any philosopher who wishes to embark on a study of this form of infinitesimal analysis will have to possess a good grounding in category theory as a prerequisite to understanding the topos theoretic framework. However, anyone in that position will probably then find the bulk of the book rather simple. The technical details of the framework, contained in the final chapter and appendix, are rather too brief to be of much assistance.

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David Corfield
Department of History and Philosophy of Science
Cambridge University, Cambridge, UK
E-mail address: dnc24@cam.ac.uk

PII: S1355-2198(02)00009-6

Controversy and consensus in nuclear beta decay 1911–1934 by Carsten Jensen

Finn Aaserud, Helge Kragh, Erik Rüdinger, Roger H. Stuewer (Eds.), Burkhaüser-Verlag, Basel, 2000, xv + 217 pp., US $79.95, ISBN 3-7643-5319-9

This book is the doctoral dissertation of Carsten Jensen, researched at the University of Copenhagen under the supervision of Erik Rüdinger, who was the director of the Niels Bohr Institute until Finn Aaserud succeeded him in 1989. A few months after receiving his Ph.D. in 1990, Jensen passed away. The two mentioned directors, together with Helge Kragh and Roger H. Stuewer, edited Jensen's dissertation and ten years later brought it to publication. Some newer historical references have been added, quotations were translated or paraphrased, some photographs included but—according to the editors—they made "few changes of substance." Jensen taught in a Danish Gymnasium while writing his dissertation, which is a rather complete study of a central episode in early nuclear physics. It is well worth the efforts of the distinguished corps of editors to make it available to historians of science.

At a conference held at the University of Minnesota in 1977 (Stuewer, 1979), Hans Bethe argued that, as regards nuclear physics, "…it is certain that 1932 marks a completely new start for both experiment and theory. The neutron was discovered and as far as theory was concerned, it was now possible for the first time to think of a rational quantum mechanics of the nucleus." The "controversies" of Jensen's thesis bear directly on the hypothesis that the nucleus contained electrons, which was the main obstacle to applying quantum mechanics in the nucleus. Magnetic deflection experiments in 1900 by Becquerel, the discoverer of radioactivity, showed that beta rays were fast electrons. When it became clear that these electrons originated from the nucleus (itself discovered only in 1911), it was natural to assume that they were