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YVES MEYER
INSTITUT UNIVERSITAIRE DE FRANCE
E-mail address: ymeyer@frulm63.bitnet

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Scenes from the history of real functions, by Fyodor A. Medvedev. Birkhäuser Verlag, Basel, 1991, 265 pp., $98.00. ISBN 3-7643-2572-0

The book is a translation by Roger Cooke from the Russian original published in Moscow in 1975 with the title *Essays on the history of the theory of functions of a real variable.*

The central concern of the book is with real functions of a real variable, but in places it leads into some aspects of functional analysis. For example, consideration is given to the function spaces L^2 and L^p and to work of W. H. Young on the notion of conjugate pairs of classes of functions such that the product of two functions f, g, one from each of the two classes of a pair, is integrable in the sense of Lebesgue. Medvedev points out that in a work by Burkill, developing the ideas of Young, there was an error, the correction of which by Birnbaum and Orlicz accompanied the creation of Orlicz spaces.

In the first chapter of his book Medvedev regards the theory of functions as a subject to be distinguished, although not very precisely, from what he refers to as classical analysis. He asserts that if from classical analysis is excluded the theory of differential equations and the theory of functions of a complex variable, then the theory of functions of a real variable can be viewed as a larger, deeper, and more general version of classical analysis. An important part of the difference is that point set theory is regarded as belonging to the theory of functions but not to classical analysis.

In the second chapter, on the history of the function concept, Medvedev touches briefly on what he sees as the notion of functional dependence in ancient times and the Middle Ages. He then passes quickly to the seventeenth century and to what he called the Eulerian period, from the work of John Bernoulli in 1718, relying not only on some of the writings of Newton but also on the investigations of Leibniz, James Bernoulli, and others, to the publication in 1822 of Fourier's *Théorie analytique de la chaleur.* Medvedev goes into careful detail in discussing whether the concepts of function due to Euler, Condorcet, Lacroix, and Fourier were, in fact, beginning to take the shape of what has come to be known as the Dirichlet definition of function and had in that respect reached a culmination in the writing of Fourier. Medvedev states (on pages 48 and 49) that it seems to him that the Dirichlet notion of functional dependence lies at the foundation of Fourier's entire investigation.

In this discussion Medvedev refers to the views of Hawkins as expressed in the latter's book, *Lebesgue's theory of integration.* Medvedev writes: "Hawkins's arguments as to the concept of a function in the eighteenth and early nineteenth centuries, especially regarding Fourier, cannot be considered convincing." The author's arguments and statements with reference to what Hawkins had to say are interesting, but he is moderate in taking issue with Hawkins. I also found interesting the following general observations on page 53 in Medvedev's book.

> It is difficult to say what degree of "arbitrariness" in a functional correspondence Fourier actually had in mind in his writings. If we compare these writings with the "Dirichlet definition", we cannot help noticing not only their external similarity, but also that Fourier's definition, taken literally, is significantly more general than that of Dirichlet.
>
> ...
>
> ... the general concepts introduced by mathematicians are almost always rather vague and elusive, and only gradually acquire more or less sharp boundary lines, and the more general the concept the greater its vagueness and the longer the vagueness persists. This is not a defect, rather a virtue of general concepts. It is the vagueness and flexibility of new concepts that opens a wide field of application to them. Such was precisely the case with Fourier's definition of a functional correspondence.

Also in the second chapter are discussions of definitions of functions as given by Lobachevskii (in 1834) and by Dirichlet (in 1837). Lobachevskii's definition implies continuity of the function: "The general concept of a function requires that a function of x be defined as a number given for each x and varying gradually with x." Medvedev quotes from Dirichlet's paper of 1837 (the one that gave rise to the widespread assignment to Dirichlet of authorship of the general definition of a function used in the later decades of the nineteenth century and on into the twentieth century). In the paper Dirichlet speaks only about continuous functions, but he does stress that the law by which values are assigned to $f(x)$, apart from requiring the continuous variation of $f(x)$ as x varies, can be completely arbitrary. Dirichlet was fully aware of the existence of discontinuous functions and also of the fact that he was not the first to emphasize the arbitrariness of the assignment of values to $f(x)$. Medvedev ascribes to Hankel responsibility for the fact that the general concept of a function of one real variable is mostly connected with the name of Dirichlet. Medvedev states that in a paper of 1870 Hankel brought into general use the phrase "definition of a function according to Dirichlet".

Late in Chapter 2 Medvedev comes to the concept of a function as a correspondence that transforms each element of a certain abstract set into a definite element of another (possibly the same) abstract set. He writes:

> For the argument and the value of a function to be regarded as elements of abstract sets, it is necessary that such sets become an object of study. They became such in the abstract set theory created by Cantor, Dedekind, and others. ... Evidently this definition of a function was first stated by Dedekind in 1887.

The citation here is to Dedekind's *Was sind und was sollen die Zahlen*? The same general concept of a function appears in a work of Cantor published in 1895–97, apparently as his own idea, independently of Dedekind.

Chapter 3 is about various kinds of convergence of infinite series or sequences of functions: various types of uniform convergence, convergence almost everywhere, in measure, some of the types of convergence that are important in functional analysis, and still other types. This is the longest chapter in the book and the most detailed. It is not feasible to review more than a few of the sections of the chapter.

The emergence of the notion of uniform convergence is traced by the author, starting from the erroneous assertion made by Cauchy in his book of 1821, *Analyse algébrique*, to the effect that the function defined on an interval by an everywhere convergence series of continuous functions is itself continuous on the interval. As is well known, Abel observed that a counterexample is provided by a certain convergent trigonometric series of sines, the sum of which is discontinuous at odd multiples of π. Medvedev asserts that the first works with which the concept of uniform convergence is usually linked are a paper of Ph. L. Seidel in 1847 and one by Stokes in 1848. Seidel did not define uniform convergence. Instead, he introduced the notion of *arbitrarily slow convergence at a point.* Given a series of continuous functions, convergent in a neighborhood of a point x_0, with $R_n(x)$ the sum at x of the remainder after n terms, Seidel distinguished two cases: (i) there is some interval enclosing x_0 such that $|R_n(x)|$ can be made less than any given positive number simultaneously for every x in the interval for all sufficiently large n; or (ii) this cannot be accomplished, no matter how short the interval is made. Seidel showed that in the first case the sum of the series is continuous at x_0 but that if the sum function has a jump discontinuity at x_0, the second case must prevail. In the second case Seidel called the convergence at x_0 *arbitrarily slow.* He did not actually use the name *uniform convergence* for the first case.

Seidel's paper went unnoticed for some time. It was published in the *Sitzungsberichte der Bayerische Akademie der Wissenschaften.* Not until 1870 did mathematicians begin to refer to Seidel's paper. Medvedev says that the work of Stokes remained unknown even longer. He discusses the work of Stokes in connection with a notion that Medvedev calls *generalized* uniform convergence, basing his discussion on a paper about Stokes published by Hardy in the

Proceedings of the Cambridge Philosophical Society, 1918.

Cauchy corrected his error of 1821 in a paper published in 1853. He did not use the name *uniform convergence*, but he explicitly imposed on the convergent sequence of continuous functions the inequality condition on remainders that characterizes uniform convergence. This paper apparently went unnoticed at the time. Pringsheim, in his report on the foundations of general function theory in the German encyclopedia of the mathematical sciences (Vol. II, A1), asserts that the term *uniform convergence* is due to Weierstrass, who introduced it in his lectures.

Medvedev discusses many other contributions to the study of uniform convergence, both in its usual sense and in modified senses, with reference to continuity of the limit function and also with reference to the legitimacy of term-by-term differentiation or integration of a series. There were important contributions by Arzelà, Bendixson, Dini, Osgood, and Hobson. There is a type of convergence now known as quasi-uniform convergence. This terminology is due to Borel, but the concept is due to Arzelà, who called it *convergenza uniforme a tratti* and used it in an important long paper published in Bologna in 1899–1900.

In connection with convergence in measure, Medvedev discusses works by Arzelà, Egorov, Lebesgue, and F. Riesz.

Prior to his discussion of the function class L^2 and the Riesz-Fischer theorem, Medvedev dwells at some length on work by Harnack published in 1880. Harnack studied the mean-square convergence of the Fourier series of a Riemann integrable function $f(x)$, aiming to prove that the partial sums of the Fourier series converge in mean-square to a function $\varphi(x)$ such that the integral (over the basic interval) of the squared difference between $f(x)$ and $\varphi(x)$ is zero. There were many things wrong with the attempted proof, of course, as Harnack soon recognized and tried to cope with.

In connection with §3.5 of Chapter 3, I must point out a misstatement on page 115. Medvedev asserts that in Fréchet's thesis (published in 1906) he proved that for every measurable and almost everywhere finite function $f(x)$ defined on an interval there exists a sequence of continuous functions converging to $f(x)$ almost everywhere. This is an inaccurate attribution. In Fréchet's thesis there is the following theorem: "Each function $f(x)$ in the Baire classification can be considered as the limit of a sequence of polynomials $p_n(x)$ converging to $f(x)$ except on a set of measure zero." The theorem mentioned by Medvedev as being in Fréchet's thesis is not there and (as far as is known) was never published by Fréchet. However, the theorem is attributed to Fréchet by Natanson in the English translation from Russian of his book, *Theory of functions of a real variable* (1955). The theorem is true, to be sure. An account of the relationship of Fréchet and Lebesgue to the theorem is explained in a paper that I published in joint authorship with Pierre Dugac after I discovered traces of related ideas in letters from Lebesgue to Fréchet in the Archives of the Académie des Sciences in Paris. See *Quatre lettres de Lebesgue à Fréchet* in Rev. Hist. Sci., 1981 34/2. See, in particular, pages 166–167 of this paper. In one of Lebesgue's letters to Fréchet in 1905, he uses the theorem from Fréchet's thesis and his own explanation of how, for a given measurable function $f(x)$, there must exist a function of Baire class at most 2 that is equal to $f(x)$ almost everywhere. From this follows the theorem attributed to Fréchet.

The third chapter concludes with an eleven-page section on the Baire classification of functions as well as on the long paper of Lebesgue (in 1905) on analytically representable functions. Baire's work, starting in 1897, was published at length in his thesis in 1899. Baire proved that the limit of a sequence of Baire functions that converges at all points is a Baire function and that the cardinality of the class of Baire functions is the cardinality of the continuum. Before 1904, however, it was not known if there actually existed Baire functions of classes greater than 2. In 1904 Lebesgue proved the existence of functions in every Baire class.

Chapter 4, entitled *The derivative and the integral in their historical connection,* begins with mention of the method of exhaustion in Greek mathematics and of what Medvedev calls differential methods in the work of Babylonian astronomers in the second century B.C. The author passes rapidly over what he refers to as the rudiments of integral and differential methods in ancient and medieval times, observing that the sorts of methods that were applied to different classes of problems were not connected with each other in any way. A major theme of this chapter is the change over time in the status of the integral concept in relation to that of differentiation; the integral concept is sometimes primary, at other times secondary.

In his brief account of Galileo's treatment of uniformly accelerated motion, culminating (in modern notation) in the formula

$$x = \int_0^t gt\,dt = \frac{1}{2}gt^2$$

for the distance traversed by a body in time t with constant acceleration g, Medvedev declares that this is the first example of an indefinite integral in the history of science. In dealing (very briefly) with the works of Gregory and Barrow, Medvedev asserts that "they actually established the mutually inverse character of the concepts of derivative and indefinite integral." In this connection Medvedev cites the three-volume *History of mathematics from ancient times to the beginning of the nineteenth century,* edited by A. P. Yuskevich, printed (in Russian) in Moscow, 1970–72. But then Medvedev continues: "Nevertheless neither Gregory nor Barrow became the creator of the differential and integral calculus, first of all because their notions were too geometric or kinematic and second because they were insufficiently algorithmic."

In §4.3, in writing about the analysis of Newton and Leibniz, Medvedev reports that Kolmogorov, in a talk given on the 250th anniversary of the death of Leibniz, said that the fundamental idea that guided Newton in his scientific activity was that of the mathematization of natural science. In the mathematization of motion the operation of differentiation had to come to the fore, and the appropriate idea of the integral became that of the indefinite integral. Another mathematical apparatus was needed for convenience in the description of motion. That apparatus—algebra—was at hand from its previous development, mainly for use in the solution of equations. Medvedev states that "it is in algebra that the notion of a variable quantity arises." At this point he introduces (perhaps not surprisingly at the time his book was written) a quotation from F. Engels's *The dielectics of nature*: "The turning point in mathematics was Descartes's *variable magnitude.* With that came *motion* and hence *dielectics* in mathematics, and at once also *of necessity the differential and integral calculus.*" (The emphasis is that of Medvedev's book.)

As Chapter 4 continues, Medvedev points out that during the nineteenth century the concepts of derivative and integral began to diverge more and more from the scheme of analysis envisaged as the continuing development of differential and integral calculus with enlargement of the body of transcendental functions. Medvedev writes (on page 181):

> The theoretically beautiful method of obtaining the value of a definite integral by the Newton-Leibniz formula soon exhausted its possibilities even in the one-dimensional case, so that Luzin could write with some justice in 1933 that for 150 years after the death of Euler, mathematicians were unable to make any breach in the ring of integrations he had forged.

It should be noted that by the Newton-Leibniz formula Medvedev means the formula

$$\int_a^b f'(x)dx = f(b) - f(a),$$

even though he says of it in a footnote that Newton stated the formula explicitly only in geometric disguise and that it does not seem to occur explicitly in the writings of Leibniz. However, Medvedev says, the formula follows easily from the approach of Leibniz to integration as the inverse of differentiation and his explicit mention of the need for an additive constant.

A reversal of the order of emphasis on differentiation and integration came in 1823 with Cauchy's presentation of the concept of the definite integral as the limit of a sum, followed by his demonstration that a definite integral with a variable upper limit furnishes a primitive of a given continuous function $f(x)$—that is, that

$$\frac{d}{dx}\int_a^x f(t)\,dt = f(x).$$

Riemann's definition of the definite integral led to the study of integration in ways that were not connected with differentiation. There were studies of differentiation in its own right, such as much of the important work of Dini on the four derivates of a function at a point. In 1881 Volterra proved the existence of a function $f(x)$ defined on $[0, 1]$ having a derivative $f'(x)$ at each point and such that $f'(x)$ is bounded but not Riemann integrable. Thus the primitive of $f'(x)$ cannot be reconstructed by using Riemann's integral. On the face of it, this appeared to destroy the validity of the fundamental notion that differentiation and integration are mutually inverse operations. It was, therefore, one of the triumphs of Lebesgue's thesis that in it he was able to prove that, for a function $f(x)$ with a bounded derivative (as in the foregoing situation dealt with by Volterra), $f'(x)$ is integrable in the sense of Lebesgue and the integral of $f'(t)$ over the interval $[0, x]$ is a primitive of $f'(x)$.

Medvedev also calls attention to another achievement of Lebesgue that is interesting and, I believe, not well known. Lebesgue proved, without any use of integration, that every continuous function defined on a closed interval has a primitive defined on that interval. The proof is not complicated. It is in a short paper appearing in volume 29 (1905) of the *Bulletin des sciences mathématiques*.

The last part of Chapter 4 deals with the extension of Lebesgue's theory of integration to functions of several real variables defined in Euclidean space and to some of the far-reaching generalizations of that theory that arose out of ideas of Stieltjes, Radon, Fréchet, Nikodym, Kolmogorov, and Caratheodory.

The title of the fifth and final chapter is *Nondifferentiable continuous functions.* Actually, quite a bit of the narrative is about how mathematicians gradually moved away from their tendency to think that continuous functions were, in the main, differentiable. The practice and experience of the seventeenth and eighteenth centuries had led mathematicians to expect that every function had a derivative except at a few isolated exceptional points. Instead of inquiring about the existence of a derivative, they set out to calculate it. After a brief introductory section Medvedev discusses a paper of 1806 by Ampère about the notion of a derivative. This paper has sometimes been misinterpreted as having the intent of proving that an arbitrary continuous function necessarily has a derivative. Medvedev says that what Ampère attempted was "to prove that every function that is analytic in the sense of Lagrange has a derivative everywhere except for individual isolated values of the variable." I think that a more satisfactory account of Ampère's paper is given by Grabiner in her book on *The origins of Cauchy's rigorous calculus.*

Medvedev writes that "In the 1870s a crushing blow was delivered to the faith of mathematicians that a continuous function necessarily has a derivative, though perhaps not everywhere...." In 1870 Hankel, using the method of condensation of singularities, obtained the first examples of continuous functions having no derivatives on the everywhere dense set of rational points. Weierstrass in 1872 presented to the Berlin Academy of Sciences his now well-known example of a continuous and nowhere differentiable function. It was not actually published until 1875. Meanwhile, in 1873 Darboux had presented another such example at a meeting of the French Mathematical Society.

The chapter closes with an interesting commentary on how a point of view was reversed by the separately published papers of Banach and S. Mazurkiewicz in 1931. In these papers it was shown that the set of continuous but nondifferentiable functions is a set of the second category in the Banach space of *all* continuous functions, while the differentiable functions form only a set of first category in that space. Thus the earlier view of regarding nondifferentiable functions as pathological had to be altered. (Hermite had declared "I turn away in horror and disgust from this growing plague of nondifferentiable functions.") It now appeared that among all continuous functions the normal thing was for a specimen to be nondifferentiable, while the differentiable functions formed a meager lot, abnormal by comparison with the general run of continuous functions.

I enjoyed the book and learned a great deal from it. Having the English translation is very useful. The bibliography would be more useful, in my opinion, if it provided the full citation of the original journal publication of a paper, as well as the location of the paper in the author's collected works, instead of giving just the title and the original year (but not place) of publication, along with a full reference to the paper's location in the collected works. In the book, the references to many papers are handled in this less desirable way, even when the journals in question are among the standard and commonly available ones.

There are numerous references to work that, unfortunately for some of us, is available only in Russian.

Medvedev's book contains useful insights. In some places the narrative is a bit wordy and rambling. That, however, is only a minor criticism of a valuable work of scholarship.

ANGUS E. TAYLOR
UNIVERSITY OF CALIFORNIA AT BERKELEY

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An introduction to harmonic analysis on semisimple Lie groups, by V. S. Varadarajan. Cambridge Studies in Advanced Math., vol. 16, Cambridge Univ. Press, Cambridge and New York, 1989, x+316 pp., $69.50. ISBN 0-521-34156-6

Semisimple Lie groups are symmetry groups that occur in surprisingly many situations. They are the isometry groups of Riemannian symmetric spaces, the analytic automorphism groups of bounded symmetric domains, the groups from which Eisenstein series and cusp forms are constructed in analytic number theory, the conformal groups of general relativity, the groups whose representations correspond to elementary particles, ... They should form part of the basic toolkit of every modern mathematician; but, in fact, the theory is relatively unknown because it is not easily accessible.

The reason for the inaccessibility of semisimple Lie group theory is clear to anyone who has tried to learn or to teach it: One must navigate a path too complicated to follow without a good vehicle and a good map. But it's well worth the effort: that path leads to a breathtaking mathematical vista.

Varadarajan's book *Harmonic analysis on semisimple Lie groups* is the best introduction to harmonic analysis on semisimple Lie groups from the analytic viewpoint. It is neither a textbook nor a monograph in the usual sense, but rather a sort of pedagogic discourse that exposes the reader to semisimple Lie theory in a useful and informative way. After reading this book, one can either stop with a pretty good understanding of the theory and its role in harmonic analysis (if not in geometry, probability, or physics), or one can continue to study the theory with a reasonable background and an excellent sense of direction. Also, and this is no small matter, the book is a pleasure to read.

There are other important viewpoints for harmonic analysis on semisimple Lie groups and their homogeneous spaces. One has a viewpoint oriented toward linear algebraic groups that includes the theory of p-adic semisimple groups, a viewpoint oriented toward number theory that includes automorphic representation theory, a viewpoint of Riemannian geometry and symmetric spaces, a viewpoint of particle physics, and their various mixtures. But life is too short to discuss those here.

Varadarajan's book begins with an interesting introduction to harmonic analysis, with reference to classical Fourier analysis and several accessible applications. Then the book starts in a serious way with a quick sketch of harmonic analysis on compact groups. The Peter-Weyl Theorem, which extends the method of harmonic analysis from Fourier development of periodic functions of one variable to functions on general compact topological groups, is