Osterwalder-Schrader positivity property $E_{\varepsilon\Lambda}\theta X \cdot X \geqslant 0$ where θ is the reflection in a hyperplane Π.

Once again, quantum physics has turned into probability theory. The theory of random functions indexed by higher-dimensional spaces has been largely the province of those doing statistical mechanics. The imaginary time approach to constructive quantum field theory led to an extraordinarily fertile interaction of quantum physics and statistical mechanics.

The ultraviolet problem in two dimensions is relatively easy. Glimm and Jaffe do not give details in this book of their solution of the much harder ultraviolet problem in three dimensions. A variety of techniques from statistical mechanics is used to control the infrared limit, most notably correlation inequalities and cluster expansions. In Part I, among other things, the authors give beautiful expositions of these techniques in the simplest cases, and this eases the way for the quite difficult applications to field theory.

Once the Euclidean random field ϕ has been constructed, the corresponding quantum field may be obtained. There is one proviso: ϕ may not be ergodic under translations, which means that the quantum field may not have a unique vacuum. This is not a technicality. Indeed, ergodicity may fall, leading to a phase transition. The successes of constructive quantum field theory discussed here by Glimm and Jaffe have gone far beyond showing the existence of models—phase transitions, broken symmetry, particle structure, the scattering matrix, and other topics of physical interest have been thoroughly explored.

Functional integration has been far more successful in quantum physics than those of us who first learned the purely Hilbert-space approach ever dreamed. There is a mystery in this. Perhaps the mathematical trick of analytical continuation in time, which is applicable in some but not all situations, is not the key to the mystery. Perhaps probability theory has been so successful because the phenomena of quantum physics are inherently random phenomena. Whether this speculation is correct, only non-imaginary time will tell.

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Real elliptic curves, by Norman L. Alling, Mathematics Studies, Vol. 54, North-Holland Publishing Company, Amsterdam, 1981, xii + 350 pp., $36.25 US/Dfl. 85.00 paperback. ISBN 0-4448-6233-1

The author has (in collaboration with N. Greenleaf [**2**]) developed an interesting approach to real elliptic curves as an object of study in their own right, and not as a special case of complex analysis (as the universal imbedding subject). The theory was present in classical literature going back to 1882 (Klein [**8**]), and the historical context has stimulated the author to make a scholarly survey of elliptic functions from even before Gauss. This survey occupies about two-thirds of the book and by itself makes for a valuable source of pleasurable reading and painless reference for any mathematician. The subject was the very mainstream of mathematics for its most creative century and it carried number theory and applied mathematics along in its wake. Indeed the text generates such momentum that the book tends to stick to the reader's hands like glue until he or she has finished the historical portion. The remaining third deals with the author's more special interest, real elliptic curves. Although this part is self-contained and rewarding to the reader, there is the inevitable requirement of increased will power.

Whatever the author's intentions were (and he seems impartial), the classical survey would necessarily make the case for complex analysis as the proper vehicle for elliptic curves. The great gems support the case, but we cite only three simple ones for brevity.

First, consider Gauss's arithmetic-geometric mean. If (a, b) is a pair of positive reals, then a transformation is defined

$$\text{(G1)} \qquad T(a, b) = (a', b'), \qquad a' = (a+b)/2, \qquad b' = \sqrt{ab}\,.$$

It is verified that the integral

$$\text{(G2)} \qquad I(a, b) = \int_0^{2\pi} d\theta / \left(a^2 \cos^2\theta + b^2 \sin^2\theta\right)^{1/2}$$

satisfies the invariance $I(a, b) = I(a', b')$. The transformation which does this is scarcely obvious; it is

$$\text{(G3)} \qquad \sin\theta = 2a\sin\theta' / \left((a+b)\cos^2\theta' + 2a\sin^2\theta'\right).$$

If the transformation T is iterated so that $\lim T^n = (M, M)$ as $n \to \infty$ (M is designated as the arithmetic-geometric mean), then $I(a, b) = I(M, M)$ and this equals simply $2\pi/M$. This constitutes a result which is Gauss's main claim to knowledge of the role of complex analysis, particularly in doubly periodic structures and modular functions as well. There is a well-documented folklore which reveals that Gauss was slow to publish this work because his main interest was not in the "pure" mathematics but in the search for missing asteroids **[6]**. (The discovery was made in stages between 1791 and 1818 and published as "astronomy".) In hindsight, it is easy to deplore the evasion of complex analysis by this trick. It really consists of finding $J(2\tau)$ from $J(\tau)$, in modern terms, or, of taking the elliptic integral of (G2) and doubling one of the complex periods, but not the other. (See the reviewer's exposition **[5]**.) No historical survey can avoid the indelicate questions of priority, raised under the Shadow of Gauss's famous intransigence toward his emerging rivals (see Bell **[3]**). The author finds that the evidence discourages such polemics, however, when all the facts are considered. Indeed, Siegel's **[10]** argument for Fagnano's priority (1718) is included.

This brings us directly to the second gem, Abel's (1827) proof of the addition theorem of Euler (1751). The simplest model for it is the addition formula for sin a. If we write

$$\text{(A1)} \qquad f(a, b) = \sin a \cos b + \cos a \sin b$$

we find by the rules of differentiation that

$$\text{(A2)} \qquad \partial f/\partial a = \partial f/\partial b$$

so that f is constant when $a + b$ is. Thus

$$\text{(A3)} \qquad f(a, b) = g(a + b) = \sin(a + b)$$

since $g(a)$ must clearly equal $\sin a$ (if we set $b = 0$ in (A3)). Of course, this is circular (logically) since the trigonometric definitions are artificial. It is more natural to write

$$\text{(A1)}' \qquad w = u\sqrt{1 - v^2} + v\sqrt{1 - u^2}$$

and then to note (as easily) that

$$\text{(A2)}' \qquad \sqrt{1 - u^2}\,\partial w/\partial u = \sqrt{1 - v^2}\,\partial w/\partial v;$$

therefore

$$\text{(A3)}' \qquad w = G\left(\int_0^u du/(1 - u^2)^{1/2} + \int_0^v dv/(1 - v^2)^{1/2}\right).$$

If $v = 0$, we find w becomes u so $u = G(\int_0^u du/(1 - u^2)^{1/2})$. (Of course G is the sine function.) Thus in (A3)′, if we operate with the inverse of G it follows that

$$\text{(A4)}' \qquad \int_0^w dw/(1 - w^2)^{1/2} = \int_0^u du/(1 - u^2)^{1/2} + \int_0^v dv/(1 - v^2)^{1/2}$$

as a consequence of (A1)′ (the addition theorem for sines). For elliptic functions the manipulation is a mandatory ritual for "hard analysts", and it is now seen as a short calculation which can be even written in the margin (by any slacker who neglected it before). The ingenuity consists in a new starting equation (A1)″ (of Euler) but the rest follows:

$$\text{(A1)}'' \qquad w = \left(u(1 - v^4)^{1/2} + v(1 - u^4)^{1/2}\right)/(1 + u^2v^2);$$

$$\text{(A2)}'' \qquad (1 - u^4)^{1/2}\partial w/\partial u = (1 - v^4)^{1/2}\partial w/\partial v;$$

$$\text{(A4)}'' \qquad \int_0^w dw/(1 - w^4)^{1/2} = \int_0^u du/(1 - u^4)^{1/2} + \int_0^v dv/(1 - v^4)^{1/2}.$$

This is Euler's addition theorem, but who can really claim to understand it this way, as a real-analytic phenomenon?

The third gem is perhaps the most persuasive argument for complex analysis as the true level of understanding (over real analysis). It suggests that the physicists might have discovered everything by themselves (even sooner)! It involves the circular pendulum. In the usual coordinates, the length is l, the gravitational constant is g and the angular displacement from equilibrium (the low point) is θ:

$$l\theta'' + g\sin\theta = 0. \tag{P1}$$

The period in time t becomes expressed as an elliptic integral. We now consider what would happen if gravity were reversed and the pendulum swung overhead from the same starting angle. This could be accomplished by $g \to -g$ but we use $t \to it$. Thus the same elliptic function leads to both a real and a complex period. The formulas are of less interest than the concepts (but are given in the text). Surprisingly, this idea, due to Appel (1878), has attracted far less attention than it deserves (see **[11]**).

The other gems by comparison are mostly too advanced, involving the very hard core of special functions. Their net effect is to create the case for complex analysis as the true source of insight. The problems in the reals are more special and circumstantial, but they exist. For example, Legendre's normal form of an elliptic curve (using complex birational transformations) is

$$w^2 = (1 - z^2)(1 - k^2z^2), \qquad (k^2 \neq 0, 1). \tag{L1}$$

If only real transformations were permitted the best we could do in analogous circumstances would be

$$w^2 = \pm(1 \pm z^2)(1 \pm k^2z^2), \tag{L2}$$

with independent signs. There are, assuredly, different k in (L1) which lead to birationally equivalent curves (over C). These k are determined by the unique value of the (Klein) modulus

$$J = (k^4 + 14k^2 + 1)^3/108k^2(k^2 - 1)^4. \tag{L3}$$

The invariance classes of (L2) over R are more complicated, but they require no machinery more recondite than linear (real) fractional transformations $z \to (az + b)/(cz + d)$.

The topological situation is more interesting. A real function field can be visualized by a complex function field restricted to those elements which are real on a curve (say the real axis). Because a real field is invariant under complex conjugation, we may imagine that either half of the Riemann surface (say the upper half-plane) is its domain of definition. Thus we are dealing with a Riemann surface with a boundary, or a compact surface with an identification in pairs. This paves the way for nonorientable Riemann surfaces to enter as domains of definition. These are called "Klein surfaces" to preserve the special requirement that Riemann surfaces are orientable. These surfaces are manifolds which could have boundaries, the locus of fixed points of the identification under conjugation (like the real axis).

Since we start with a period parallelogram (torus), of genus one, the conjugation isomorphism in question can produce only one of three well-known types:

the annulus;
the Klein bottle;
the Moebius strip.

Each of these manifolds has one real parameter to serve as modulus. They correspond in an interesting fashion to the moduli for which the Klein invariant J is real; all three occur for $J = 1$ (the square shaped period parallelogram). The author treats these cases by an explicit computation supported by powerful abstract arguments.

These Klein surfaces seem to have been described in relatively few places, considering their intuitive appeal. The author cites only a sampling, most notably Schiffer and Spencer [**9**] among others, as well as some earlier papers [**1**]. Here the reviewer would suggest a missed opportunity to document the dawn of the scissors and paste age (at least in this context). Surely there were more witnesses to this event than to the birth of elliptic functions, but their number seems to make their testimony hard to obtain. In this text, the author tries to show the evolution of his own ideas (as well as those of the classicists). He therefore knowingly risks an unpolished approach. An undesirable side effect is that the level of abstraction is also high, perhaps out of proportion with the objectives. The author considers Chevalley [**4**] to be the "standard reference", citing Hensel and Landsberg [**7**] as a kind of classic counterpoise. Perhaps there is some middle way. What seems to be the central problem, the classification of the Klein surfaces of (L2) as annulus, Klein bottle or Moebius strip, seemed more convincingly geometric in the earlier work [**2**]. Also the diagrams tend to be too abstract, when some cutting and pasting might be more in harmony with the purpose. Yet the author leaves no question of the justification of abstractionism for more advanced research.

The text has only a tolerably small number of typographical errors. In a subject this classical we expect to miss the grand style of the hand typesetters. Yet the IBM Selectric is a poor substitute, when we consider that here for instance boldface is replaced by a smaller size italic type that hides the headings.

In summary, the text makes a worth-while effort to present a neoclassical idea (Riemann surfaces with boundaries) in its historical context and it should be of interest to mathematicians in topology, analysis, and algebra. For a sympathetic outsider (for whom the text was intended), the more creative (recent) results may present more of an effort than the author envisioned. But the approach is honest and the effort would be rewarding.

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Module categories of analytic groups, by Andy R. Magid, Cambridge Tracts in Math., vol. 81, Cambridge Univ. Press, New York, 1982, x + 134 pp., $29.50.

The relationship between a group G and the collection of its finite-dimensional linear representations (or the category Mod(G) of finite-dimensional G-modules) is often subtle. For compact Lie groups, there are classical duality results affirming that the group is recoverable from a knowledge of its representations and how they tensor. For example, in case G is abelian, Pontryagin duality gives an isomorphism between G and $\hat{\hat{G}}$. Here the dual group $\hat{G}$ consists of the 1-dimensional representations of G (complex-valued characters), the product of characters corresponding to the tensor product of associated representations.

Tannaka duality [**5**] does something similar for arbitrary compact Lie groups. The role of $\hat{G}$ is played by the collection of all finite-dimensional representations of G, whose "representations" are in turn identified with elements of G. In Chevalley's formulation [**1**], one forms the Hopf algebra $R(G)$ of **C**-valued "representative functions" (matrix coordinate functions for representations of G), with a coproduct reflecting the product in G. Because G is compact, $R(G)$ is finitely generated, hence gives rise to a complex linear algebraic group $\overline{G}$. The points of $\overline{G}$ can be thought of as algebra homomorphisms $R(G) \to \mathbf{C}$, by identifying $R(G)$ with functions on $\overline{G}$. Duality means that G is realized as the group of real points of $\overline{G}$. In this formulation, $R(G)$ plays the role of a dual group, encapsulating the structure of Mod(G) as a category with tensor products.

In a long series of joint papers (1957–1969), G. Hochschild and G. D. Mostow explored the Hopf algebra of representative functions of an arbitrary complex analytic group (cf. [**3**]). In case G is semisimple, its finite-dimensional representation theory is essentially that of its compact real form; so $R(G)$ is finitely generated and gives G the structure of an algebraic group. But in general the story is far more complicated. In particular, distinct groups may give rise to the "same" category Mod(G). This happens in a fairly transparent way when G fails to have a faithful finite-dimensional (analytic) representation,