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CAN CONSTRUCTIVE MATHEMATICS BE APPLIED IN PHYSICS?

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ABSTRACT. The nature of modern constructive mathematics, and its applications, actual and potential, to classical and quantum physics, are discussed.

KEY WORDS: constructive mathematics, quantum mechanics, recursive, unbounded operator

1. INTRODUCTION

In this article I would like first to explain what some practitioners of 'constructive mathematics' mean by that phrase, and then to examine certain aspects of classical and quantum physics from a constructive point of view. In particular, having discussed constructive mathematics in general in Section 1, I shall deal, in the succeeding sections, with the constructive significance of the work of Pour-El and Richards, the reality of a constructive theory of unbounded operators, and constructive mathematics as a medium for the foundations of quantum physics.

There are several substantial references for constructive mathematics to which the reader can refer for more details on the subject; among these are [1, 2, 3, 9, 22, 27].

2. CONSTRUCTIVE MATHEMATICS

What *is* constructive mathematics? According to the pioneers of the subject, such as Brouwer [11], Markov [19, 20], and Bishop [2], it is mathematics carried out under the requirement that 'existence' must be strictly interpreted as 'computability' (in some sense that depends on the particular outlook of the individual pioneer). If you adopt this view, then, almost inevitably, you find yourself using a different logic from the usual classical one.

This new logic, known as *intuitionistic logic*, was originally abstracted from constructive mathematical practice. For Brouwer, at least, mathematics had priority over logic; the latter was derived from a close in-

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spection of the principles needed, and in some cases rejected, when one works constructively. On the other hand, Brouwer's introspection led him to formulate a number of principles of his 'intuitionism' that led to results apparently in conflict with aspects of classical mathematics.

This gave rise to the traditional view of constructive mathematics as an approach, based on hard-line philosophical views about existence, that leads to the replacement of basic, and frequently perceived as essential, rules of logic by eccentric principles that may lead to 'theorems' flatly contradicting well-known results of classical mathematics. Although this view represents a gross misunderstanding of Brouwer's ideas, and an equally gross undervaluation of his contribution to the clarification of distinctions between the classical and constructive approaches, in light of the intractability of Brouwer and some other leading exponents of constructivism, it is not surprising that classical mathematicians, led by Hilbert and believing that their life's work was under attack, have often reacted to constructive mathematics with vigorous opposition.

There is, however, a polemic-free, modern view of constructive mathematics. This view, propounded by Fred Richman [24] and which I share, is based on our thirty years of investigating constructive mathematics in the style of Errett Bishop [2]. It seems clear to us that, in practice,

constructive mathematics is none other than
mathematics carried out with intuitionistic logic

For us, the distinctive feature of constructive mathematics is its *methodology*, rather than its content. In other words, we can work constructively – that is, using intuitionistic logic – with any objects of mathematics, not just those of some special 'constructive' type (whatever that may mean).

But why would anyone choose to work in this way? Because *mathematics developed with intuitionistic logic has more interpretations/models than its classical counterpart.* In particular, every theorem proved with intuitionistic logic can be modelled within

- recursive function theory (one form of computability theory with a very precise notion of algorithm);
- Brouwer's intuitionistic mathematics;
- any formal system for computable mathematics that I know of, such as Weihrauch's TTE [26, 28, 29] or Martin-Löf's Theory of Types [21]; and
- classical (that is, traditional) mathematics.

So, at the cost of some logical principles, we gain more models. (The computational models are, of course, the ones that we are most interested in; if and when they are not of any interest, one might as well use classical logic.)

Now, the classical mathematician might still ask: if you are interested in computational questions, why not just use recursive function theory with classical logic? To follow this path has two disadvantages in my view. First, it immediately ties us down to a recursive interpretation, making it difficult, perhaps in some instances impossible, to translate our results into other formal systems for computable mathematics. Secondly, proofs in, for example, recursive analysis tend to involve machinery (such as the construction of complicated recursive functions and the verification that they are recursive) that obscures the analytic trail being followed.

Classical logic permits ‘decisions’ that no computer (real or idealised) can make in general. For example, we know that a computer may be unable to decide, for a given nonnegative number x, between the alternatives $x = 0$ and $x > 0$: the input x may be positive but so close to 0 that the computer sets its floating-point representation equal to 0. (This is the problem of *underflow*.) Thus we would expect the statement[1]

$$\forall x \in \mathbf{R}\ (x = 0 \vee x \neq 0),$$

where **R** denotes the set of real numbers, to be unprovable with a logic that truly captures the principles used in computation.

There seem to be two ways in which one can approach questions of computability in mathematics. In the first of these, one uses classical logic and is then forced to pin down the admissible types of algorithm (such as recursive ones) to avoid making decisions, such as the foregoing one about the equality of real numbers, that cannot be made in computational practice. In the second approach, one uses intuitionistic logic, which automatically takes care of the types of decision that are permitted, and one then argues, *with whatever mathematical objects one chooses* (not just, for example, recursive objects), in the normal style of the analyst, algebraist, geometer, and so on. As well as leading to more models of the resulting mathematics, the second approach enables the mathematician to read and write constructive mathematics in the customary way, without the special, and at times cumbersome, apparatus of recursive function theory. Thus, if you open Bishop’s book [2] at any page, what you see will immediately be identifiable as analysis; you may, of course, wonder why Bishop adopts certain proof strategies, until you realise that the standard strategies make essential use of classical logic (usually some form of the law of excluded middle – see below).

Let me now give the standard constructive/computational interpretations of the logical connectives and quantifiers:

- $P \vee Q$: we have either a proof of P or a proof of Q.
- $P \wedge Q$: we have a proof of P and a proof of Q.
- $\neg P$: assuming P, we can derive a contradiction (such as $0 = 1$).
- $P \Rightarrow Q$: we can convert[2] any proof of P into a proof of Q.
- $\exists x P(x)$: there is an algorithm which computes an object x and demonstrates that $P(x)$ holds.
- $\forall x \in A\ P(x)$: there is an algorithm which, applied to an object x and a proof that $x \in A$, demonstrates that $P(x)$ holds.

Thus to prove $P \vee Q$ it is not sufficient, as it is classically, to prove $\neg(\neg P \wedge \neg Q)$, since from a proof of the latter statement we cannot, in general, produce either a proof of P or a proof of Q. Note how the constructive interpretation of the connective $\vee$ captures the notion of undecidability that arises naturally at even the simplest level of computation, such as with underflow.

From the time of Brouwer, constructive mathematicians have made use of a number of intuitionistically undecidable principles in order to show that certain classical propositions cannot be proved constructively in their usual form. Among these principles are the *limited principle of omniscience*,

LPO For any binary sequence (a_n), either $a_n = 0$ for all n or else there exists n such that $a_n = 1$,

and the *lesser limited principle of omniscience*,

LLPO For any binary sequence (a_n) with at most one term equal to 1, either $a_n = 0$ for all even n or else $a_n = 0$ for all odd n,

each of which is a special case of the *law of excluded middle* (**LEM**),

$$P \vee \neg P,$$

which is certainly nonconstructive. In fact, there is a model theory which shows that each of LPO, LLPO, and LEM is independent of Heyting arithmetic – Peano arithmetic with intuitionistic logic (see under Kripke and Beth models in [13]).

Many apparently innocent classical propositions turn out to be essentially nonconstructive, in that they imply LPO, LLPO, or even LEM. Here are some examples, with the implied principle placed in parentheses after each statement:

– $\forall x \in \mathbf{R}\ (x = 0 \vee x \neq 0)$. (LPO)

– The *least-upper-bound principle*: Each nonempty subset S of $\mathbf{R}$ that is bounded above has a least upper bound. (LPO)
– Every real number is either rational or irrational. (LPO)
– $\forall x \in \mathbf{R}\ (x \geqslant 0 \vee x \leqslant 0)$. (LLPO)
– If $x, y \in \mathbf{R}$ and $xy = 0$, then $x = 0$ or $y = 0$. (LLPO)
– A uniformly continuous function from $[0, 1]$ to $\mathbf{R}$ attains its bounds. (LLPO)
– The *Intermediate Value Theorem*: If $f : [0, 1] \to \mathbf{R}$ is a continuous function with $f(0) < 0 < f(1)$, then there exists $x \in (0, 1)$ such that $f(x) = 0$. (LLPO)

Fortunately, there are constructive substitutes for such inadmissible propositions:

- Although the comparison of two real numbers is a problem, we can use rational approximations to prove that if $a < b$, then for all $x \in \mathbf{R}$ either $a < x$ or $x < b$ (so the real line can be split into two overlapping parts).
- The conclusion of the least-upper-bound principle holds if we add the hypothesis that for all real numbers a, b with $a < b$, either b is an upper bound of S or else there exists $x \in S$ with $x > a$.
- The conclusion of the Intermediate Value Theorem holds if we add the hypothesis that f is *locally nonzero*, in the sense that each subinterval of $[0, 1]$ contains points at which the value of f is different from 0.

I would now like to turn from these general remarks about constructive mathematics to consideration of constructive approaches to physics, beginning with the classical world of waves.

3. THE WORK OF POUR-EL AND RICHARDS

In the past twenty-or-so years, Marian Pour-El and the late Ian Richards have developed a new approach to computability, which reached its definitive form in their book *Computability in Analysis and Physics* [23]. Their approach is based on the idea of a *computability structure* on a Banach space X: that is, a nonempty set $\mathcal{S}$ of sequences in X satisfying three very natural axioms designed to capture the essential properties of intuitively computable sequences. The results derived from these axioms can immediately be interpreted recursively, if one so wishes, but such an interpretation is not necessary.

By an *effective generating set* for X they mean a computable sequence in X whose linear span is dense in X. For example, the monomials $1, x,$

$x^2, \ldots$ form an effective generating set for the natural computability structure on the Banach space $\mathcal{C}[0, 1]$ of continuous, real-valued mappings on the closed interval $[0, 1]$.

For our purposes, the most significant of the Pour-El and Richards results is their *First Main Theorem*, which establishes a link between boundedness and the preservation of computability for classically closed linear operators between Banach spaces:

Let T be a closed linear mapping between Banach spaces X and Y with computability structures, and suppose that there exists an effective generating set (e_n) for the computability structure on X, such that the sequence (Te_n) is computable in Y. Then the following conditions are equivalent.

(i) *T is bounded.*
(ii) *T maps computable elements of its domain to computable elements of Y.*

This theorem, which is proved (as are all the results of Pour-El and Richards) using classical logic, has several interesting applications, among the most startling of which concerns the initial-value problem associated with the wave equation:

$$(1) \qquad \nabla^2 u = u_{tt}, \quad u(\mathbf{x}, 0) = f(x), \quad u_t(\mathbf{x}, 0) = 0,$$

where $\mathbf{x}$ ranges over the cube $[-1, 1]^3$ in $\mathbf{R}^3$, $0 \leqslant t < 2$, and the initial function f is defined and continuous on the cube $[-3, 3]^3$, in $\mathbf{R}^3$. Here, of course, the suffix t denotes partial differentiation with respect to time. The classical solution of (1) is provided by *Kirchhoff's formula,*

$$u(\mathbf{x}, t) = \int\int_{\text{unit sphere}} (f(\mathbf{x} + t\mathbf{n}) + t\nabla f(\mathbf{x} + t\mathbf{n}) \cdot \mathbf{n})\, \mathrm{d}S(\mathbf{n}),$$

where $\mathrm{d}S(\mathbf{n})$ is the area measure on the unit sphere, normalised so that the total area of the sphere is 1. For fixed t, the operator

$$(2) \qquad f \mapsto \int\int_{\text{unit sphere}} (f(\mathbf{x} + t\mathbf{n}) + t\nabla f(\mathbf{x} + t\mathbf{n}) \cdot \mathbf{n})\, \mathrm{d}S(\mathbf{n}),$$

is closed and unbounded, and maps monomials in x, y, z to computable elements of its range; since those monomials form an effective generating set for the computability structure under consideration, the First Main Theorem shows that there exists a computable continuous function f such that the solution $u(\mathbf{x}, t)$ of (1) at time $t = 1$ is a noncomputable continuous function of $\mathbf{x}$.

Obviously, this raises at least one philosophical question: What does it mean to say that a physical process, such as the propagation of a wave, could have associated parameters that are noncomputable at time $t = 1$? On the other hand, both this physical application of the First Main Theorem, and the theorem itself, appear to have annihilatory implications for the role of constructive mathematics in physics, classical and quantum. I would like to concentrate on the latter aspect of the theorem.

The proof of the First Main Theorem requires an infinite recursively enumerable subset A of $\mathbf{N}$ that is not recursive. There is no problem with that in recursive constructive mathematics. But there are two problems with the proof: one is a lemma which Pour-El and Richards prove nonconstructively (using the least-element principle for subsets of $\mathbf{N}$), but which, as I showed in the Appendix to [5], can be given a fully constructive proof. The other problem, again analysed in detail in [5], lies with the authors' notion of *closed operator* T. For them, *closed* has the usual classical meaning:

$$(x_n \to x \wedge Tx_n \to y) \Rightarrow (Tx\downarrow \wedge\ Tx = y),$$

where we use the notation $Tx\downarrow$ to signify that x belongs to the domain of the operator T. A fully recursive development would work with the notion of *recursively closed*, which requires that if (x_n) converges recursively to x and (Tx_n) converges recursively to y, then $Tx\downarrow$ and $Tx = y$. The crux of the Pour-El and Richards proof is an application of closedness when (x_n) converges recursively to x, and (Tx_n) converges, *but not recursively*, to y. So the proof is not a fully recursive one, and, since it hinges on the nonrecursive convergence of (Tx_n) to y, does not appear likely to be adaptable to a fully recursive form.

Thus, while there may be some significant, fully constructive analogue of the First Main Theorem, a careful analysis (see [5]) reveals that the Pour-El and Richards proof of that theorem has little or no significant constructive content.

What *can* be said constructively about the First Main Theorem? It has been suggested ([17, 18]) that since in constructive mathematics an unbounded operator, closed or not, would have to take computable elements to computable elements, the theorem destroys all hope of a constructive theory of unbounded operators and hence one of quantum physics. This is a serious misunderstanding of the constructive significance of the First Main Theorem. What the theorem actually tells us is that if T is a closed unbounded linear mapping between Banach spaces X and Y with computability structures, then *there will be elements of X whose membership of the domain of T can only be proved using classical logic*. This may seem a strange interpretation, but it is not if one appreciates the view, expounded in the first section of this paper, that constructive mathematics deals with all the normal objects of mathematics but uses only intuitionistic logic.[3]

The classical derivation of Kirchhoff's formula can be adapted, with minor modifications, to produce a constructive one; so, constructively, Kirchhoff's formula gives an explicit solution of the initial value problem (1) associated with the wave equation. However, although, using classical logic in the First Main Theorem, it is possible to produce a situation in which the initial value problem has a noncomputable solution $u(\mathbf{x}, 1)$, this situation cannot occur if we use intuitionistic logic. The classical set-up actually produces an initial data function f for (1) whose membership of the domain of the Kirchhoff operator (2) can only be demonstrated using classical logic; from a constructive point of view, we are unable, and cannot hope, to prove that the function f belongs to that domain.

To understand this point, we need to appreciate that there are models of constructive mathematics in which only 'computable' objects appear; one such model is the recursive constructive mathematics developed by Markov and his school [19]. If we had a proof, using intuitionistic logic, that the function f is in the domain of the Kirchhoff operator T, then Tf would be a well-defined object of recursive constructive mathematics, which, being non-recursive (this is clear from the Pour-El and Richards proof), it is not.

The Pour-El and Richards First Main Theorem has, unfortunately, given rise to some serious misapprehensions about the scope of a constructive theory of unbounded operators:

> any unbounded (closed linear) operator T (meeting the PE-R conditions*) cannot be constructively known to preserve constructivity. ... In this sense, such unbounded operators cannot be recognized as constructive operators by the type of constructivist program in question. ([17, pp. 231–232])

By the *PE-R conditions** the author of this claim means some reasonable adaptations of the hypotheses of the original Pour-El and Richards First Main Theorem.

Let me repeat, in summary form, the argument underlying these claims. The Pour-El and Richards result shows, classically, that, under reasonable conditions (met, for example, by the Kirchhoff operator associated with the wave equation), an unbounded operator must map some computable element of its domain to a noncomputable element of its range. Since constructive mathematics cannot recognise noncomputable objects, it follows that such a mapping cannot be constructively defined.

The flaw in this argument lies in the final deduction ('it follows that...'). As I have already indicated, the correct deduction from an analysis of the Pour-El and Richards theorem is that if T is an unbounded operator satisfying the PE-R conditions*, then there exist computable objects whose membership of the domain of T can be established using classical logic but cannot be proved with intuitionistic logic. From a constructive standpoint, these objects will be well defined – in the recursive model, the objects are recursive – but we will never be able to show that they lie in the domain of T. This situation certainly does not justify, or even contribute any evidence towards, a view that constructive mathematics cannot deal with unbounded operators.

In fact, there is no problem with a constructive theory of the differentiation operator $\mathrm{d}/\mathrm{d}x$ on the Banach space $\mathcal{C}[0, 1]$, or with the Kirchhoff operator associated with the initial value problem (1). Both of these closed unbounded operators are well defined constructively and amenable to a fully constructive analysis of their properties.

Ishihara and I have a paper [8] in preparation in which we prove, constructively, a number of results, such as the following two, about the domains of unbounded linear operators:

- *If T is a densely defined unbounded linear operator on a Hilbert space H such that T^* is densely defined, then there exists $x \in H$ such that Tx is undefined.*
- *Let T be an unbounded linear partial mapping of a Banach space X into a normed space Y, and x a point of X such that $x \notin \mathrm{domain}(T)$. For each y in the domain of T there exists $z \in X$ such that if $Tz\downarrow$, then $x \neq y$.*

Each of these results is trivial to prove classically, and the second has little, if any, classical content; but – and this is typical of many similar situations in constructive analysis – each requires some serious effort to establish constructively.

Of more significance in the present debate is the doctoral thesis [14] of Feng Ye, which contains constructive proofs of the spectral decomposition of unitary operators, the Spectral Theorem for unbounded selfadjoint operators, and Stone's Theorem. It also contains detailed constructive proofs of the selfadjointness of the position, Hamiltonian, and angular momentum operators of quantum mechanics, a topic to which I now turn.

4. QUANTUM MECHANICS

...the quantum world is a hostile environment for constructivism.
In particular, the major theorem of Gleason [15] characterizing the measures on the closed subspaces of Hilbert space of dimension $\geqslant 3$ (and hence the possible probability measures over quantum events as ordinarily identified) is not constructively demonstrable (in either the sense of intuitionism or that of Bishop-constructivism). ([16, pp. 221–222])

Neither of these claims is justifiable. The first is partly based on a misapprehension of the constructive significance of the Pour-El and Richards

First Main Theorem that I have already dealt with. However, there is another argument used to support the claim, an argument that represents another common misunderstanding of constructive mathematics – namely, that the domains of constructive functions should have decidable membership. This amounts to saying that instead of proving statements of the form

$$\forall x \in A\ (P(x) \Rightarrow Q(x)),$$

the constructive mathematician should be proving ones like

$$\forall x (x \in A \vee \neg(x \in A)) \Rightarrow \forall x \in A\ (P(x) \Rightarrow Q(x)).$$

I see no justification for this extraordinary imposition on the constructivist. In recursive mathematics, the classical theory of computability, the domains of partial recursive functions from **N** to **N** are recursively enumerable (that is, effectively countable) but not necessarily recursive (that is, decidable). Why should the transition from classical to constructive mathematics require the introduction of the extra hypothesis of decidability of domains? To insist on this requirement would cause constructive mathematics to have negligible content and to lose its recursive interpretation. In particular, it would make impossible a constructive development of quantum mechanics even in a one-dimensional context, since the basic operators of position and momentum have domains with undecidable membership.

A remark of Dummett,

A mathematical concept is not [constructively] meaningfully applicable apart from an idealized mathematician's having a constructive method that shows that it applies [13],

has been invoked as the basis of an argument for decidable domains in constructive mathematics. I read Dummett's remark, and his subsequent statement that

In sum, no proof-independent mathematical facts are countenanced,

as saying that if, for example, I claim that an object x has the property P, then I have to justify that claim by an intuitionistic, and therefore direct, proof; I am not permitted to 'justify' it merely by showing that it cannot fail to hold. Such an interpretation leads naturally into the intuitionistic one of universal quantification. Consider, for example, the statement

(3) $\forall x \in \mathbf{R}^+\ \exists y \in \mathbf{R}\ (xy = 1),$

where $\mathbf{R}^+$ denotes the set of all positive real numbers. According to Bishop ([2, p. 19]), a positive real number consists of more than just a real number x: it consists of the number x together with data that witness the fact that x

is positive. Moreover, a real number x is really a sequence (x_n) of rational numbers together with data D witnessing that x is *regular*, in the sense that

$$\forall m \forall n (|x_m - x_n| \leqslant \tfrac{1}{m} + \tfrac{1}{n}).$$

The data for a positive real number consist of a sequence x; the corresponding information D witnessing that x is regular; and a positive integer N such that $x_N > 1/N$, witnessing that x is positive. A constructive proof of (3) takes those data, not just x itself, constructs a sequence $y = (y_n)$ of rational numbers, shows that $|y_m - y_n| \leqslant \frac{1}{m} + \frac{1}{n}$ for all m and n, and then demonstrates that $xy = 1$.

More generally, a constructive proof of the statement

$$\forall x \in A \; P(x)$$

uses the data consisting of an object x *and a proof that* x *satisfies the conditions for membership of* A to show that $P(x)$ holds. Nowhere do we, or should we, require a method that decides, for any x in the universe within which we are working, that x does, or does not, belong to A.

Incidentally, the problem of decidable domains arises even in the context of bounded partial operators on 1-dimensional spaces. To see this, let θ be any real number, and let T be the partial operator on $\mathbf{R}$, with domain

$$\mathbf{R}\theta = \{t\theta : t \in \mathbf{R}\},$$

such that $T(t\theta) = t\theta$ for all $t \in \mathbf{R}$; deciding whether or not 1 is in the domain of T is equivalent to deciding whether or not $\theta = 0$.

The second claim stated at the start of this section, the claim relating to Gleason's Theorem, is false: as Richman and I have recently shown, Gleason's Theorem is constructively provable [25]. What was shown in [16] is that a result classically equivalent to Gleason's Theorem – not the theorem itself – is essentially nonconstructive.

Let me expand on this, beginning with the classical statement of Gleason's Theorem.

GLEASON'S THEOREM [15]. *Let H be a separable Hilbert space of dimension at least* 3, *and let μ be a measure on the projections of H–that is, a mapping of the set of projections into the nonnegative reals such that*

$$\mu\left(\sum_{n=1}^{\infty} P_n\right) = \sum_{n=1}^{\infty} \mu(P_n)$$

for each sequence (P_n) of pairwise orthogonal projections. Then there exists an operator A of trace class on H such that $\mu(P) = \mathrm{Tr}(AP)$ for each projection P.

We recall here that an operator A on H is of *trace class*[4] if the series $\sum_{n=1}^{\infty}\langle |A|e_n, e_n\rangle$ converges for some, and hence any, orthonormal basis $(e_n)_{n=1}^{\infty}$ of H, where the operator $|A| = \sqrt{A^*A}$ is the absolute value of A; in that case, the *trace* of A,

$$\mathrm{Tr}(A) = \sum_{n=1}^{\infty}\langle Ae_n, e_n\rangle$$

exists and is independent of the orthonormal basis (e_n).

Why is Gleason's Theorem such a significant touchstone in this context? It is because the theorem is used, in the traditional development of the foundations of quantum mechanics, to characterise probability measures for quantum events; such a measure must apply, in particular, to 'yes–no' questions, which are represented in the standard model by projections on a separable Hilbert space.

Gleason's original proof of his theorem is highly ingenious, drawing together strands from several parts of analysis. It also appears to be highly nonconstructive: for example it uses more than once the nonconstructive result that a (uniformly) continuous, real-valued function on a compact space attains its bounds. (Constructively, we can prove that such a function has a supremum and an infimum, but not, in general, that these extreme values are attained.) In 1985, Cooke, Keane, and Moran [12] published an elementary proof of Gleason's Theorem, one that avoids Gleason's appeal to the representation theory of the orthogonal group. This proof, too, has highly nonconstructive features. Indeed, at first – maybe second – glance, neither Gleason's nor the 'elementary' proof of the theorem appear to give mush encouragement in the search for a constructive proof.

Hellman [16] went so far as to produce an example which appeared to show that Gleason's Theorem was not constructively provable. What this example actually did was to show that a certain statement about the diagonalisation of quadratic forms, which is classically equivalent to Gleason's Theorem in the 3-dimensional case, is essentially nonconstructive (in fact, it implies LLPO). However, this statement is classically, *but not constructively*, equivalent to Gleason's Theorem; so there remained the possibility that Gleason's Theorem, as originally stated, can be proved constructively.

Taking up this challenge while visiting me in 1997, Fred Richman,[5] by a tour de force of technique and ingenuity, finally produced a constructive proof of Gleason's original theorem. The proof involves a careful mixture of Gleason's ideas and the elementary technique of Cooke, Keane, and Moran. There are several places where the constructive problems were substantial; in particular, it was no trivial matter to get round the inapplicability of the classical principle that extreme values of a continuous real function on a compact set are attained.

So Gleason's Theorem *is* constructively provable after all, and the second argument, quoted at the start of this section, against constructive mathematics as a medium for quantum mechanics, fails. Indeed, taken with the very recent constructive work of Feng Ye on the (unbounded) operators of quantum mechanics [14], Richman's proof of Gleason's Theorem strongly endorses my view that a fully constructive development of quantum mechanics is not only possible, but perhaps not far off.[6]

NOTES

[1] The statement $x \neq 0$ means $|x| > 0$; it is constructively stronger than the statement $\neg(x = 0)$.

[2] This interpretation of implication, while more natural than the classical one of *material implication*, in which $(P \Rightarrow Q)$ is equivalent to $(\neg P \vee Q)$, has not completely satisfied all researchers using constructive logic. Shortly before he died, Bishop communicated to the author his dissatisfaction with the standard constructive interpretation of implication. Unfortunately, he left nothing more than very rudimentary sketches of his ideas for its improvement.

[3] Of course, this is not the 'radical constructivist' viewpoint which is argued against in [17] and [18]; but I would maintain that it is misleading, and probably pointless, to base an argument against *radical* constructivism on a classical result – the First Main Theorem – that makes essential use of a nonconstructive notion (in this case, closedness, as against, for example, recursive closedness).

[4] For constructive information on trace-class operators, see [4] and [10].

[5] Although my name is on the paper [25], I have no hesitation in acknowledging that the bulk of the work on it was done by Richman.

[6] In this context it is worth mentioning related work, in progress, on constructive operator algebra theory, by Bridges and Dediu [6]; see also [7]. Operator algebras form another classical setting for the foundations of quantum mechanics.

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