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A Non-Standard Analysis of a Cultural Icon: The Case of Paul Halmos

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Abstract. We examine Paul Halmos' comments on category theory, Dedekind cuts, devil worship, logic, and Robinson's infinitesimals. Halmos' scepticism about category theory derives from his philosophical position of naive set-theoretic realism. In the words of an MAA biography, Halmos thought that mathematics is "certainty" and "architecture" yet 20th century logic teaches us is that mathematics is full of uncertainty or more precisely incompleteness. If the term *architecture* meant to imply that mathematics is one great solid castle, then modern logic tends to teach us the opposite lession, namely that the castle is floating in midair. Halmos' realism tends to color his judgment of purely scientific aspects of logic and the way it is practiced and applied. He often expressed distaste for nonstandard models, and made a sustained effort to eliminate first-order logic, the logicians' concept of *interpretation*, and the syntactic vs semantic distinction. He felt that these were *vague*, and sought to replace them all by his *polyadic algebra*. Halmos claimed that Robinson's framework is "unnecessary" but Henson and Keisler argue that Robinson's framework allows one to dig deeper into set-theoretic resources than is common in Archimedean mathematics. This can potentially prove theorems not accessible by standard methods, undermining Halmos' criticisms.

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1. Introduction

Fifty years ago, the *Pacific Journal of Mathematics* published a pair of papers in the same issue, each containing a proof of a conjecture in functional analysis known as the Smith–Halmos conjecture. The event had philosophical ramifications due to the fact that one of these proofs involved methods that were not only unusual for functional analysis but also challenged both historical thinking about the evolution of analysis and foundational thinking in mathematics. The present article explores these and related issues.

Paul Halmos was a 20th century expert in functional analysis. His textbooks on measure theory, Hilbert spaces, and finite dimensional vector spaces are well written, still relevant, and highly praised.[1]

Following the Aronszajn–Smith proof of the existence of invariant subspaces for compact operators [2], Smith and Halmos conjectured that the same should be true for more general classes of operators, such as operators with a compact square. A proof in the more general case of polynomially compact operators in [4] (exploiting Robinson's infinitesimals) was a notable event in functional analysis. Simultaneously the same journal published an infinitesimal-free proof [10].

In 1991, Dauben interviewed the distinguished model theorist C.C. Chang about the Bernstein–Robinson paper. Even a quarter century later (and after [28] superseded the 1966 results) Chang still seemed a bit sore about Bernstein and Robinson not getting enough credit, for he insisted that

> once you know something is true, it is easier to find other proofs. Major credit must go to Robinson.[2] (Chang quoted in [6, p. 327])

[1] See http://www.maa.org/news/paul-halmos-a-life-in-mathematics.
[2] Chang's reference to Robinson is certainly shorthand for Bernstein–Robinson.

Robinson himself supports Chang's reading:

> As for the Halmos standard 'translation', it was all very nice, but the NSA (i.e., *nonstandard analysis*) proof was quite natural, while the standard proof required an argument that would not have been so easy to spot without first seeing the NSA version. (Reported by Moshe Machover, private communication)

In a course at Hebrew University in the late 1960s, Robinson said:

> Halmos was proud of his proof but in the end all he did was rewrite our proof in a language he was educated in. (Reported by Shmuel Dahari, private communication).

Halmos himself essentially agreed with this sentiment when he wrote that the purpose of his paper was

> to show that by appropriate *small modifications*[,] the Bernstein–Robinson proof can be converted ... into one that is expressible in the standard framework of classical analysis. [10, p. 433] (emphasis added)

Further details can be found in Sect. 2.

Subsequently Halmos expressed reservations about Robinson's framework, and described researchers working in the framework as *converts* (see Sect. 6.1).

What philosophical outlook shaped Halmos' attitude toward Robinson's framework, and prompted his critical remarks concerning fellow experts? Following [19], we provide an analysis that can hardly be described as *standard* of a little-known aspect of a mathematical *cultural icon*.

2. Paraphrase

The invariant subspace conjecture of Smith and Halmos was first proved by Bernstein and Robinson, and published in the *Pacific Journal of Mathematics* (*PJM*). A number of scholars would have been more comfortable had Halmos' infinitesimal-free paraphrase of the proof in [4] (for which Halmos was apparently the referee) appeared in the *next* issue of the *PJM* rather than being published simulataneously in the same issue as [10].

The Bernstein–Robinson proof is presented in detail in [7].

Halmos claimed two decades later that he received the manuscript by Bernstein and Robinson "early in 1966" in [14, p. 320], but that date is certainly incorrect. Dauben documents a letter from Halmos to Robinson acknowledging receipt of the manuscript, and dated 19 june 1964 (see [6, p. 328,note66]). Thus Halmos was in possession of the Bernstein–Robinson manuscript even *prior to* its submission for publication on 5 july 1964.

Several specialists have privately testified that Halmos was most likely the referee for the Bernstein–Robinson paper.[3] A slightly delayed publication of Halmos' paraphrase (say, in the following issue of the *PJM*) would have

[3] See also a related discussion at http://mathoverflow.net/questions/225455.

avoided the effect of weakening the Bernstein–Robinson priority claim on the result, and may have constituted a more appropriate use of publication timetables. There have been several cases of scholars affected by the marginalisation campaign against Robinson's framework who ended up suffering in terms of employment as a result, indicating that such issues are not purely *academic*.

Here by "Robinson's framework" we mean Robinson's rigorous justification of Leibnizian infinitesimal procedures in the framework of modern mathematics (viz., the Zermelo–Fraenkel set theory with the axiom of choice), as developed in [32] and [33]. Robinson exploited the theory of types in presenting his framework. Alternative presentations involve ultraproduct constructions; see e.g., [29].

3. Indispensability Argument of Henson and Keisler

Halmos explicitly referred to his own paper as a "translation" (of the Bernstein–Robinson proof). However he did not think of it as an *awkward* translation, and on the contrary used it to justify his claim in [14] that NSA is unnecessary because it can always be translated. The following year, Henson and Keisler published a paper [17] that was a reaction to a widespread belief at the time that Robinson's framework is unnecessary, and in particular provided a rebuttal of Halmos' claims.

3.1. Second-Order Arithmetic

Henson and Keisler point out that a nonstandard extension of second order Arithmetic is not a conservative extension of second-order Arithmetic, but is rather closely related to third-order theory. This is because, roughly, nonstandard arguments often rely on *saturation* techniques that typically involve third-order theory. They go on to argue against the type of fallacy contained in Halmos' position that Robinson's framework is unnecessary. The gist of their argument is that since most mathematics takes place at second-order level, there may well be nonstandard proofs whose standard translations, while theoretically possible, may well be humanly incomprehensible. They conclude as follows:

> This shows that in principle there are theorems which can be proved with nonstandard analysis but cannot be proved by the usual standard methods. The problem of finding a specific and mathematically natural example of such a theorem remains open. [17, p. 377]

In this spirit, [36] use the language of Robinson's framework in order to avoid a large number of iterative arguments to manage a large hierarchy of parameters. Ultraproducts form a bridge between discrete and continuous analysis [9].

3.2. Rebuttal of Halmos' Claims

Halmos formulated a pair of claims concerning Robinson's framework, which are closely related but perhaps not identical:

1. Robinson had a language and not an idea.

2. Robinson introduced a special tool, too special, and other tools can do everything it can, so it's all a matter of taste.

In Halmos' own words:

> If they had done it in Telegu [sic][4] instead, I would have found their paper even more difficult to decode, but the extra difficulty would have been one of degree, not of kind. [14, p. 204]

Even though Halmos calls it a "language" in (1) and a "tool" in (2), the underlying claim is essentially the same: just as you can express your mathematics in English, French, or Telugu and it does not make any difference, so similarly you can do your mathematics in traditional set-ups or in a Robinsonian logical contraption.

The rebuttal is the same in both cases, and was already provided by the Henson–Keisler argument and the example of Tao's work, as discussed in Sect. 3.

Today, Robinson's framework is neither a language, idea, or tool, but rather is a branch of modern mathematics with its own domain, set of tools, collection of key results, and numerous applications.

4. Dedekind Cuts and Category Theory

The following comment by Halmos needs to be addressed:

> Here is a somewhat unfair analogy: Dedekind cuts. It's unfair because it's even more narrowly focused, but perhaps it will suggest what I mean. No, we don't have to learn it (Dedekind cuts or nonstandard analysis): it's a special tool, too special, and other tools can do everything it does. It's all a matter of taste. [14, p. 204]

Halmos seems to view both Dedekind cuts and category theory with disfavor. On the other hand, one who doesn't favor cuts should apparently favor category theory, since excising cuts would make the real line a *category*, i.e., something without a strict set-theoretic definition.

4.1. Category Theory Viewed by Some

Halmos' attitude to Robinson's framework is somewhat comparable to Halmos' attitude to category theory, at the expense of which he also made disparaging remarks:

> A microscopic examination of such similarities might lead to category theory, a subject that is viewed by some with the same kind of suspicion as logic, but not to the same extent. [14, p. 205]

In his essay "Applied mathematics is bad mathematics," Halmos claimed that when applied mathematicians describe category theory as "abstract nonsense," they mean it [11, p. 15], but provided no evidence to substantiate his claim that applied mathematicians feel this way, or that such sentiments are due to anyone but himself.

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[4] The correct spelling is *Telugu*.

Halmos sought to identify categories with universal algebras, thus reducing category theory to set theory in [12].

Category theory is today one of the fastest growing industries, with avid advocates like David Kazhdan. Halmos might have pigeon-holed Kazhdan a "convert" as well (see Sect. 6.1), but it wouldn't have helped Halmos' reputation.

4.2. Bridge Between Discrete and Continuous

Robinson's framework is a fruitful modern research area that has attracted many researchers, as noted in Sect. 3.2. Halmos predicted that

> in the foreseeable future ... *discrete* mathematics will be an increasingly useful tool in the attempt to understand the world, and ... analysis will therefore play a proportionally smaller role. [11, p. 19] (emphasis added)

What Halmos may not have anticipated is that, in fact, the ultraproducts form a bridge between discrete and continuous analysis as mentioned above.

5. Halmos and Logic

The algebraic approach to logic has a long history starting with Boole, continuing with Peirce and Schröder, and reaching a high point with the Löwenheim–Skolem theorem. Subsequently it went out of fashion to a certain extent, but the work of Tarski on Boolean algebras with operators eventually led to his cylindric algebras, i.e., Bolean algebras with *quantifiers* as the added operators. The Tarski school has proved a number of difficult, and perhaps even *deep*, results about this class of algebras.

5.1. From cylindric to polyadic algebras

Halmos became interested in this topic, as he discusses in his book [14], where one finds some remarks on polyadic vs cylindric algebras; see also [16]. Whether or not there are any contributions of substance by Halmos to logic proper is a delicate question. His polyadic formalism differs from the cylindric counterpart, but the theory in his book is a straightforward translation of first order logic, thus not *deep* by any means. Neither polyadic nor cylindric algebras made a major contribution to logic and its applications, and are of marginal interest today.

In later work on probability, the algebraic formalism was dropped in favor of working within first order logic. Halmos' translation of the completeness theorem, i.e., his representation theorem, is rather complicated. Thus, Fenstad gave a simplified presentation and used this work to give a rather general representation theorem for logical probabilities in [8].

Halmos' feelings about logic in general and Robinson's framework in particular are neatly summarized in a limerick dating from 1957, and republished on page 216 in his book:

> If you think that your paper is vacuous,
> Use the first-order functional calculus.
> It then becomes logic,
> And, as if by magic,
> The obvious is hailed as miraculous.

It has to be admitted that Halmos and Bishop had something in common, namely literary talent (see Sect. 6.2). The limerick aptly summarizes the import of Halmos' own contribution to logic.

5.2. Quixotic Battle Against Formal Logic

A passage in Halmos' book reproduced in his article "An Autobiography of Polyadic Algebras" is part of his attack on *formal logic* (as opposed to *symbolic logic* favored by Halmos), and runs as follows:

> When I asked a logician what a variable was, I was told that it was just a 'letter' or a 'symbol'. Those words do not belong to the vocabulary of mathematics; I found the explanation that used them unhelpful–*vague*. When I asked what 'interpretation' meant, I was answered in bewildering detail (set, correspondence, substitution, satisfied formulas). In comparison with the truth that I learned later (homomorphism), the answer seemed to me unhelpful–forced, ad hoc. It was a thrill to learn the truth–to begin to see that formal logic might be just a flat photograph of some *solid* mathematics–it was a thrill and a challenge. [14, p. 208], [16, p. 385–386]

Some issues need to be clarified in connection with this passage:

1. What is Halmos' problem with formal logic exactly?
2. What is wrong with the term *interpretation*?
3. In what way does replacing the term *interpretation* by the term *homomorphism* help?
4. What is *unsolid* about formal logic?

Exploring these questions may help understand Halmos' 36 year battle (1964–2000) against anything nonstandard.[5] What Halmos seems to be reacting against is a distinction taken for granted in modern logic, namely that between syntax and semantics. Roughly, this means that one can have a *theory* at the syntactic level which does not *mean* anything until one *interprets* it in a specific model to get meaning (*semantics*). This view presupposes a possibility of having *distinct* models for the same *theory*.

5.3. Mathematics as One Great Thing

Halmos' position against such dualities appears to stem from a naive set-theoretic realism (already on display in his opposition to category theory; see Sect. 4). Halmos seems opposed to the idea that there are distinct levels of

[5] There is yet another dig against non-standard models in his 2000 article cited above, one of the last ones he wrote.

things in mathematics: you can have a *theory* of a distinct level of mathematical *Sein* than an *interpretation* thereof. Halmos apparently prefers to see all mathematics as made of the same cloth:

> I see mathematics, the part of human knowledge that I call mathematics, as one thing–one great, glorious thing. (Halmos quoted in [1, p. 234])

Now the 'one great thing' comment suggests that all mathematical objects are sets, and sets differ in degree of complexity but they do not differ in kind.

In this sense, *homomorphisms* are more *solid* than *interpretations*, in that talking about homomorphisms implies that the domain and the range are of the same kind, thereby escaping the duality of theory/interpretation that seems to threaten the solidity of naive set-theoretic realism. Perhaps Halmos' polyadic algebras could be understood as an attempt to undo *formal logic* with its threatening dualities and inherent possibilities of unsolid (a.k.a., nonstandard) models. A related point was made by G. Lolli, in the context of an analysis of Halmos' views, in the following terms:

> ... the deep reason for the opposition, depreciation and misunderstandings concerning logic among mathematicians lies in their inability or unwillingness to accept the binomium language-metalanguage as a mathematical tool; they don't even seem capable of understanding its sense. This could be due to their habit of talking in an informal quasi-natural language, where metalanguage is flattened on the language itself, or the languages are absorbed in the metalanguage, a habit legitimated and reinforced by the set-theoretical framework. [27]

Having identified the set-theoretic source of the problem, Lolli concludes:

> They should know however, as everybody is now aware, that this very identification is the source of dangerous circularities. Only the conceptual distinction, at least in principle, of language and metalanguage avoids the paradoxes. (ibid.)

6. A Rhetorical Analysis

In addition to scientific arguments, Halmos resorted on several occasions to excesses of language aimed at marginalizing Robinson's framework, as we document in this section.

6.1. Halmos on Types of Worship

Halmos may have been a leading expert in his field, but so was Edward Nelson (see e.g., [30]), and so is Peter Loeb (see e.g., [26]). Halmos had the following to say about their relation to Abraham Robinson's framework:

> ...for some *converts* (such as Pete Loeb and Ed Nelson), it's a religion, ... For some others, who are against it (for instance Errett Bishop), it's an equally emotional issue–they regard it as *devil worship*. [14, p. 204] (emphasis added)

Halmos' description of both Nelson and Loeb as "converts" in the comment quoted above raises questions of motivation behind applying this kind of epithet to fellow leading mathematicians, or for that matter of invoking Errett Bishop on "devil worship," remarks that are dangerously close to the category of expletives. In point of fact Bishop never used such a term in reference to Robinson's infinitesimals (see more on *devil worship* in Sect. 6.2). Halmos sought to create the impression of a balanced presentation of *both sides* of the controversy by mentioning *both* Nelson *and* Bishop, but in fact both of his *sides* serve only as a vehicle for an attempt to demonize Robinson's framework.

6.2. Errett Bishop

Halmos' remarks concerning *devil worship* in Sect. 6.1 deserve closer scrutiny. Bishop's verse on the *neat devil* that is classical mathematics, from his essay "Schizophrenia in contemporary mathematics," run as follows:

> The devil is very neat. It is his pride
> To keep his house in order. Every bit
> Of trivia has its place. He takes great pains
> To see that nothing ever does not fit.
> And yet his guests are queasy. All their food,
> Served with a flair and pleasant to the eye,
> Goes through like sawdust. Pity the perfect host!
> The devil thinks and thinks and he cannot cry.

(See [5, p. 14].) For additional details on Bishop's antics see [21–23]. The "Schizophrenia" essay says not a word about Robinson's framework, and all the *devil* material (verse or prose) targets classical mathematics *as a whole*, including Halmos' favorate subjects such as invariant subspaces. Bishop's poem was published earlier but composed later than his teacher[6] Halmos' limerick; see Sect. 5.1. Halmos' claim that Bishop regarded Robinson's framework as devil worship appears to be merely a smear-by-proxy attack on Robinson. It is certainly possible that Bishop may have made private remarks along these lines to Halmos, who was after all his advisor. Still, Halmos' purported quote of Bishop cited in Sect. 6.1 is taken out of context.

We are not sure whether there is an official philosophical term for such a rhetorical technique, but at any rate it is not the unique occurrence of such a technique in Halmos. He did something similar with regard to category theory, while positioning himself safely behind the broad backs of unnamed applied mathematicians; see Sect. 4.

6.3. Underworld

What would be the point of using mocking epithets like "dredged up from the underworld," as Halmos did in his 1990 article, in describing Robinson's accomplishment with regard to infinitesimals:

[6] Apparently in more than one area.

> The modern theory of nonstandard analysis *dredged* the forbidden concepts *up from the underworld* and is trying to reinstate them at the right side of Cauchy's throne. [15, p. 569]

Halmos may have been more moderate in his language than Connes who used some objectionable vitriol in referring to Robinson's framework (see [20,24]), but in the end Halmos' attitude is comparable to Connes', that other leading expert. In fact, in his book Halmos broadened his criticism of Robinson to a broader criticism of logic:

> The logician's attention to the nuts and bolts of mathematics, to the symbols and words (0 and + and "or" and "and"), to their order (∀∃ or ∃∀), and to their grouping (parentheses) can strike *the* mathematician as pettifogging ... [14, p. 205]

The definite article attached to "mathematician" is the issue here, for it presupposes that there is just one thing that counts as being a mathematician. 'Some' would make it more accurate, but significantly blunt the force of the remark.

Here Halmos is apparently alluding to Robinson's approach to infinitesimals via the theory of types, with its reliance on the "nuts and bolts" of logic. If Halmos wished to publish an evaluation of Robinson's framework, he could have been expected to have done enough research to discover a more elementary analytical approach. This is the ultrapower approach, already exploited in [18] and popularized by Luxemburg in the CalTech Lecture Notes and e.g., in [29], namely over two decades prior to the publication of Halmos' book.

The sweeping and sarcastic critique Halmos presents fails to inform the reader that there does exist an accessible analytical approach to infinitesimals [25]. The existence of such an approach makes much of Halmos' vitriol rather misplaced. There might exist more abstract approaches that he does not appreciate, but the same can be said about many fields in mathematics. There are certainly textbooks in, for example, differential geometry that are more accessible than other textbooks in differential geometry. The existence of the more abstract textbooks generally does not lead sceptical scholars to speak of differential geometry as being "dredged up from the underworld."

7. Conclusion

Robinson's characterisation of Bishop's "attempt to describe the philosophical and historical background of [the] remarkable endeavor" of the constructive approach to mathematics, as "more vigorous than accurate" [34, p. 921] applies equally well to Halmos' take on logical issues, conditioned by his naive set-theoretic realism. Such a philosophical *parti pris* led Halmos to reject not merely Robinson's infinitesimals but also broad swaths of standard techniques and applications, ranging from a modern logical toolkit like first-order logic to applied mathematics. Halmos' attempted reform of logic is a radical project that bears similarity to his student Errett Bishop's even more radical opposition to classical mathematics as a whole, as analyzed elsewhere.

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