Foreword

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1 Significance of this book

This book made an important contribution to a debate about the meaning of mathematics that started about a century before its 1967 publication, and is still going on today. That debate is about the meaning of "non-constructive" proofs, in which one proves that something exists by assuming it does not exist, and then deriving a contradiction, without showing a way to construct the thing in question. As an example of the kind of proof in question, we might consider the "fundamental theorem of algebra", according to which every non-constant polynomial equation $f(z) = 0$ has a solution (among the complex numbers). To prove this theorem, do we have to show how to calculate a solution? Or is it enough to derive a contradiction from the assumption that $f(z)$ is never zero?

My favorite quotation from Errett Bishop is this:

> Meaningful distinctions need to be preserved.[5]

He is talking about the distinction between constructive and non-constructive proof. This quotation encapsulates his approach to the matter. His predecessors had taken an all-or-nothing approach, either maintaining that *only* constructive proofs are correct, and non-constructive proofs are illusory or just wrong, or else maintaining that non-constructive proofs are valid, and while computational information might be interesting in special cases, it is of no philosophical significance. There was, naturally, very little productive interplay between these camps, only exchanges of polemics.

Bishop changed that situation with this book. He simply applied a technique that has been used many times in mathematics: he worked in the common ground, so that both "classical mathematics" (that is, with proofs of existence by contradiction allowed) and previously existing varieties of constructive mathematics (which made claims contradicting classical mathematics) could be viewed as generalizing the body of mathematics that Bishop developed in this book. The surprising thing was that this common body of mathematics turned out to be quite large! Bishop showed that it encompassed the main tools of mathematical analysis. That surprised everyone, constructivists and classical mathematicians alike.

Previously, both sides believed one had to make a choice: Either

(1) reject non-constructive proofs, and with it reject much of modern mathematics, but keep your philosophical purity; or

(2) deny that there is any philosophical problem with proving things exist without constructing them.

Since very few were willing to reject most of modern mathematics, choice (1) had been made by almost nobody; in practice the mathematical community was proving existence theorems by any means possible, and not worrying whether the proof provided a construction of whatever was proved to "exist." But not everyone wholeheartedly believed that the distinction between constructive and nonconstructive proof was meaningless; it just seemed that the price of attaching any importance to that distinction was unaffordably high. Bishop showed the mathematical community a way to acknowledge the importance of that distinction, without putting the main body of mathematics at risk.

Bishop was not neutral on the issue: he made it clear that he believed that if a human proves something exists, he or she should show how to construct it. But the *mathematics* he wrote allows one to take a low-key, non-confrontational attitude towards the issue, and simply provide constructive proofs that can be accepted as valid by *everyone*.

To understand the significance of Bishop's book, one needs some understanding of the past history of constructivity. A proper treatment of that history is far too long for a foreword, where a succinct summary is required. In his review [21], Abraham Robinson gave such a summary in *just one paragraph.* We quote the first half of this masterpiece of brevity:

> In the second half of the nineteenth century, Leopold Kronecker made a determined attempt to turn mathematics away from its trend of ever increasing abstraction. His approach was based on the principle that in order to be meaningful, an existential assertion has to be buttressed by the actual construction of the object in question. Thus, a procedure that leads us to infer the existence of a mathematical object from purely formal-deductive considerations, e.g. by the use of the principle of the excluded middle, is regarded as inadequate or even misleading. Kronecker lent substance to his point of view by actually realizing the constructive approach in his lectures. Later, Brouwer, whose approach was based on the same attitude, went beyond Kronecker by developing a theory of the continuum, which may be called conditionally constructive, since it accepts the idea of a sequence of free choices as the basis of the theory of real numbers (just as even the most restrictive point of view accepts the unlimited counting process as the basis of arithmetic). Brouwer's school of thought—intuitionism—has remained the most vigorous of the several constructivist trends that have developed since Kronecker.

What we shall add to Robinson's paragraph is a few quotations to document the point that *everyone else* (other than Bishop) believed that if one accepted a constructivist philosophy, then one would have to give up much of classical mathematics. One of the chief advocates of that viewpoint was David Hilbert, one of the most famous mathematicians of the twentieth century. Here is Hilbert in 1927, speaking to the Hamburg Mathematical Seminar [23] (page 426).

> For, compared with the immense expanse of modern mathematics, what would the wretched remnants mean, the few isolated results, incomplete and unrelated, that the intuitionists have obtained?

That was forty years before Bishop; but, in the next thirty years, despite the efforts of Heyting, Markov, and Kolmogorov, nothing changed in the prevalent viewpoint. When Fraenkel and Bar-Hillel wrote their famous book *Foundations of Set Theory* [14], they devoted a whole chapter (whose primary author was Fraenkel) to the *Intuitionistic conceptions of mathematics*; on page 263 we find

> In view of the mutilated shape which mathematics assumes according to intuitionistic principles it is not astonishing that a small minority only of mathematicians has been ready to accept the intuitionistic attitude; this situation will probably not change very much when the adherents ... will succeed in formulating their principles and inferences in a less dogmatic and more comprehensible and consistent form than done up to now.

They continue with the following remarks that seem, in a way, to foreshadow Bishop:

> Even after the partial failure of the Hilbert school regarding consistency proofs one must not forget that the very existence of mathematics and the wide range of its applications during many centuries seem to show that modern analysis is not just nonsensical or meaningless ... Hence we may trust that, sooner or later, methods will be found by which those methodical doubts which have not been answered so far can be dissolved. In short, the prevailing conviction is that one should not cut off the leg to heal the toe.

It was to be sooner rather than later: Bishop's book was only nine years in the future. Bishop's preface was quite explicit about his purpose:

> to present the constructive point of view, to show that the constructive program can succeed, and to lay a foundation for further work. These immediate ends tend to an ultimate goal—to hasten the inevitable day when constructive mathematics will be the accepted norm.

2 How this book was received

Bishop's book was reviewed by four quite different people.[1] The first reviewer was Abraham Robinson [21], the founder of nonstandard analysis, who could be expected to be unsympathetic to the constructivist viewpoint, since it is so far from the viewpoint of nonstandard analysis. Incidentally, Bishop also reviewed Robinson's book, and was definitely not sympathetic; Keisler said that choosing Bishop to review Robinson was like choosing a teetotaler to taste wines. Something similar probably applies to the choice of Robinson to review Bishop. Robinson's review begins with the summary of the work of Kronecker, Brouwer, and others quoted above; then he continues with the following tribute, all the more meaningful coming from Robinson:

> The present author's point of view is essentially Kronecker's. He rejects the formalized versions of intuitionism produced by Heyting ... as well as Brouwer's theory of the continuum. On this basis, he provides a constructive development of some of the most important areas of classical and modern analysis and uses his great knowledge and power as an analyst to cope constructively with topics as advanced as the duality theory of locally compact abelian groups and the theory of Banach algebras.

But Robinson ended his review with some negative remarks, which are so unspecific and unsubstantiated that they must be

[1] Surprisingly, there was no review in *Zentralblatt*. The book's publication was listed in *Zentralblatt* **183**, p. 15, without a review.

taken to show Robinson's fundamental philosophical differences with Bishop rather than be taken at face value:

> The sections of the book that attempt to describe the philosophical and historical background of this remarkable endeavor are more vigorous than accurate and tend to belittle or ignore the efforts of others who have worked in the same general direction.

The second reviewer was Gabriel Stolzenberg [22], who was sympathetic to Bishop's viewpoint and had worked with him during the book's preparation. His review explained Bishop's viewpoint in detail, with attention to the points that often caused confusion among classically-trained mathematicians. He also placed Bishop's work in historical context with a discussion of Brouwer and Weyl. Stolzenberg's review is too long to quote extensively in a foreword, but the reader with the time to study this book would do well to read Stolzenberg's review as well. Stolzenberg's high opinion of the book is clear: He says that Bishop has "demonstrate[d] to the classical mathematician what the intuitionists ... did not: that to replace the classical system by the constructive one does not in any way mutilate the great classical theories of mathematics. Not at all. If anything, it strengthens them, and shows them ... to be far grander than we had known."

The third reviewer was John Myhill [19], a logician who later developed formal set theories suitable for formalizing Bishop's mathematics. Myhill was even more lavish in his praise than Stolzenberg: "[T]he reviewer believes this book to be the most important work on constructive mathematics ever written." Myhill reviewed both the book and Bishop's essay [4], and since Stolzenberg had, in Myhill's opinion, done a good job of reviewing the book, his review focused on the logical aspects of Bishop's work, which he explained as well as possible using tools available at that time. Logicians studying this book will certainly want to read Bishop's essay and Myhill's review as well.

The fourth reviewer (not chronologically) was B. van Rootselaar, a Dutchman familiar with the intuitionistic tradition of

Brouwer. He notes [24] that Bishop "takes continuity in the sense of uniform continuity", and says that "this policy accounts to a large extent for the smoothness of the development." In Brouwer's intuitionism, continuity and uniform continuity are guaranteed by the "bar theorem" and the "fan theorem". Van Rootselaar took the view, which I have heard expressed orally in Amsterdam, Utrecht, and Nijmegen, that Bishop was in part merely rediscovering what intuitionists had long known:

> A comparison with intuitionistic mathematics modulo the bar theorem shows not too great differences, although some material has been developed in a more general setting in intuitionism.

However, van Rootselaar concluded his review with words of praise:

> It contains a substantial piece of constructive analysis and will take a worthy place among other textbooks on analysis written from a classical point of view. It certainly deserves to attract the attention of students and working mathematicians and it is likely that it will do so because of its elegance of exposition and because it advances well beyond the elementary facts of analysis.

Van Rootselaar levied a more substantial criticism as well:

> [Bishop] stresses the equal hypothesis interpretations of classical theorems, which is misleading since the hypotheses are for the greater part only verbally equal.

To explain this criticism: any classical theorem is likely to have more than one constructive formulation, all classically equivalent. An "equal hypothesis" version has the same hypotheses, but constructively weaker conclusions; an "equal conclusion" version has constructively stronger hypotheses. Of course there can be hybrid versions as well. Equal conclusion versions are usually more useful. Van Rootselaar's criticism was exactly the reason why Bishop and Cheng, and later Bishop and Bridges, revised the treatment of measure theory.

Finally, van Rootselaar agreed with Robinson about the more philosophical parts of the book: “The first chapter, the appendices and the notes reveal a dogmatic attitude of the author.” In other words, neither Robinson nor van Rootselaar agreed with Bishop on philosophical matters. They would, of course, have disagreed even more with each other!

Bishop had a considerable reputation as a mathematician already in 1967–enough to command respect and attention when he began to espouse a minority view of the meaning of mathematics. I was in the audience when Bishop lectured at Stanford in 1969. He was received as a celebrity and spoke in a large lecture hall to a standing-room only audience, including all the senior professors of mathematics.

Stanford was not the only place where Bishop's lectures attracted large audiences and caused discussions. For example, he was invited to address the International Congress of Mathematicians in Moscow, 1966; he gave the Hedrik Lectures at the Mathematical Association of America's summer meeting in 1969; he gave the Colloquium Lectures at the 1969 summer meeting of the American Mathematical Society; in addition to these lectures he gave numerous one-hour invited addresses at national and regional meetings, according to Stefan Warschawski [25]. After the publication of his book (according to Nerode [20]), he made a tour of the eastern (U.S.) universities, and told Nerode that he was trying to communicate his viewpoint directly to the mathematical community, rather than through the logicians. After the trip was over, he told Nerode that the trip may have been counterproductive; he felt that his mathematical audiences were not taking the work seriously. He was surprised to get a more sympathetic hearing from the logicians.

Bishop also told Nerode about

> tribulations in the reviewing process when he submitted the book for publication. He mentioned that one of the referee's reports said explicitly that it was a disservice to mathematics to contemplate publica-

> tion of this book. He could not understand, and was hurt by, such a lack of appreciation of his ideas. One of the reasons for the lecture tour was to be sure that his ideas got a hearing.

Bishop also told Nerode that his students had experienced similar difficulties in developing their careers, and that he had ceased to take students because of these problems. On the other hand, Bishop was pleased at the warm reception he got from Myhill, Friedman, Constable, and other logicians.

When Bishop lectured at Stanford in 1969, I was a graduate student, and my thesis in some sense grew out of Bishop's lecture: it dealt with a logical problem that was related to Bishop's work. I subsequently wrote a book [3] collecting the various approaches to axiomatizing Bishop's work. The timeline for my own book illustrates that the mathematical community has quite a bit of inertia: Bishop published his book in 1967, and mine, which was based on the work of others whose work was directly stimulated by Bishop's book, appeared 18 years later. We will see, when we look to the literature for the effect of Bishop's book, that this time lag was not unusual.

3 What happened afterwards

The first to react were those who had given Bishop's lectures a warm reception: the logicians. John Myhill and then Harvey Friedman developed intuitionistic set theories, adequate to formalize Bishop's book. They kept the framework of classical set theory, changing only the logic and making minor changes to the axioms. Solomon Feferman developed theories of "explicit mathematics" with the same purpose, but closer to Bishop's ideas. Martin-Löf had already been working on his theories, and probably was not influenced by Bishop, although his theories could be viewed as suitable for formalizing Bishop's work. (See [3] for descriptions of all these theories and references to the original publications.) In the summer of 1968, one year after the publication of Bishop's book, there was a conference on *Intuitionism and*

Proof Theory in Buffalo, New York. Bishop himself presented a paper [4], in which he indicated an approach to providing a logical foundation for constructive mathematics. All four of the above-mentioned logicians were present at the conference, and very likely all four were in the audience at Bishop's talk. Probably the Buffalo conference played an important role in attracting their attention to the problem of formulating theories adequate to formalize Bishop's work.

But not all logicians reacted, even within a few years. Six years after Bishop's book, in 1973, a second edition of Fraenkel and Bar-Hillels' book was published. The chapter on intuitionistic conceptions of mathematics is extensively revised; there are references to numerous works that appeared after the first edition, mostly by logicians. But Bishop's book is not mentioned or referenced! That seems surprising, especially in view of the widespread discussion of his ideas in the years immediately following 1967. I offer this partial explanation: Fraenkel and Bar-Hillel's second edition reflects only the work of logicians, and, by 1973, the papers of Friedman, Myhill, and Feferman had not yet appeared.

There were also mathematicians who took up Bishop's research program more directly, rather than studying its logical foundations. This group included Douglas Bridges, Bill Julian, Y. K. Chan, Ray Mines, Fred Richman, and Wim Ruitenberg. They produced several books and many papers extending Bishop's work. Bridges's book, *Constructive Functional Analysis*, appeared in 1979, but was mostly written in New Zealand in 1976, according to the preface. Then Bridges lectured in Oxford in 1981, and invited Richman to help him write *Varieties of Constructive Mathematics* [8], which, except for one chapter on algebra, is a work about the different possible foundational positions whose common core is Bishop's constructive mathematics.

After the initial foray into algebra just mentioned (proving the existence and uniqueness of splitting fields), Richman plunged headlong into the difficulties of constructive algebra, and, with the help of Mines and Ruitenberg, wrote *A Course in Constructive Algebra* [18], published in 1988 and dedicated to

Errett Bishop.

Bishop's book contains a chapter on measure theory, but the treatment was already judged inadequate by Bishop himself, as discussed above in connection with van Rootselaar's review. A better constructive measure theory was developed by Bishop and Cheng [7]. Partly in order to include this new theory, Bishop was engaged in revising his book for a second edition. He took on Bridges as a co-author, but then died before the second edition was finished. Bridges completed the book and it was published under joint authorship in 1985 [6].

The bibliography of this second edition has 93 entries; the first edition (reprinted here) has 17. Only about ten of the 93 appeared before 1967; so after twenty years, Bishop's book had resulted in three or four other books and several dozen research papers. He had certainly demonstrated that the constructive program could succeed, and the further work for which he had laid the foundation was in progress. But his followers could be counted on the fingers of two hands. The "inevitable day when constructive mathematics will be the accepted norm" seemed as far away as ever, and it still seems so today. The revolution didn't happen.

But perhaps his influence has simply been more subtle. Remember that Bishop said "meaningful distinctions need to be preserved." Bishop clearly made the mathematical community more aware than it had been of the meaningful distinction between constructing something, and proving that something exists without constructing it. He did not convince more than a handful of people that the latter is meaningless, any more than did Brouwer. But he did show by example how to make that distinction in branches of mathematics where it had not been thought before to be a meaningful distinction.

It is difficult to assess the extent to which the mathematics community in 2012 is aware of that distinction. It is my opinion that most mathematicians are aware of whether a given existence proof provides an algorithm or not, and find that a meaningful distinction worth preserving, but only in individual proofs. They are not aware of the connection between logic and algorithms, and they are not aware of the subtleties of representing abstract objects such as metric spaces and operators in a numerically meaningful way, and how these ideas can be used to preserve that meaningful distinction "globally." For example, consider number theory as presented, say, in the two volume textbook [10, 11]; it is very careful to distinguish between "effective" proofs and non-effective proofs. (Number theorists have used the word "effective" to mean "constructive" since long before Bishop.)

Logical work on constructive mathematics is alive and well, witness for example Kohlenbach's book [17]; he has applied his logical methodology to prove theorems in approximation theory, whose proofs (freed from logical trappings) have appeared in mathematics journals with non-logician co-authors who brought the problems to his attention. In spite of isolated successes like these, the widespread change of philosophical attitude that Bishop was trying to provoke just did not take place; most mathematicians are unaware of the meaningful distinction that Bishop wanted to preserve.

In attempting to assess the impact of Bishop's book, there is a complicating factor: the computer. During those decades, the widespread availability and use of computers by mathematicians has influenced attitudes at least as much as Bishop did. Certainly number theory these days almost always involves extensive computer-assisted numerical experimentation. There is, for example, *A Course in Computational Algebraic Number Theory* [9], with no apparent influence of Bishop.

It is even difficult to tell whether there really is *increased* interest in "effectivity", since number theorists were always interested in that distinction. This claim can be demonstrated by the fact that two different mathematicians were awarded a Fields medal for proofs of the same theorem: the first proof non-constructive, the second one (partially) constructive. Klaus Roth was awarded the Fields medal in 1958, and Alan Baker was awarded the Fields medal in 1970 for the work in [1].[2]

[2]The theorem in question is this: given an algebraic number α and $\epsilon > 0$, there are only finitely many rational numbers p/q with $|\alpha - p/q| < q^{-(2+\epsilon)}$. Baker showed how to effectively bound the number (but not the size) of

4 Relations to computer proofs

Bishop began his work when computers were, as depicted in the film *Dr. Strangelove*, large and expensive, and possessed only by very large organizations. Nevertheless, in Appendix B of his book, pp. 356–357, he wrote:

> It is clear that many of the results in this book could be programmed for a computer, by some such procedure as that indicated above. In particular, it is likely that most of the results of Chaps. 2, 4, 5, 9, 10, and 11 could be presented as computer programs. As an example, a complete separable metric space X can be described by a sequence of real numbers, and therefore by a sequence of integers, simply by listing the distances between each pair of elements of a given countable dense set. . . . As written, this book is person-oriented rather than computer-oriented. It would be of great interest to have a computer-oriented version.

What did Bishop have in mind by a "computer-oriented version"? Presumably he meant a computer program that could check the correctness of constructive proofs, and extract the underlying algorithms from them, so that those algorithms could be executed, without programming them in the usual sense, and in such a way that their correctness would be guaranteed by the proofs from which they were extracted.

At about the same time, and as far as I know, completely independently, de Bruijn was developing the first computer system for representing and proving mathematical theorems in a systematic way. That system was called AUTOMATH. It was the grandfather of all modern proof-checking systems. The second generation of such systems was pioneered at Cornell by Robert Constable, and Constable explicitly acknowledges [20] the influence of Bishop on the design of the system, which was ultimately

solutions; to bound the size of p and q, and thus be able to compute the exact number of solutions, could earn another Fields medal.

known as NuPrl [12]. NuPrl was explicitly designed to "execute constructive proofs", i.e. to extract programs from proofs. Constable writes,

> Shortly after we had executed our first constructive proof, I wrote to Bishop informing him of what I took to be an historic event. I told him how much his writings and his encouragement had meant to us on the long road to this accomplishment. I was crushed to receive my letter back unopened, marked "recipient deceased."

In [26], NuPrl is said to be based on Martin-Löf's systems, which does not contradict Constable's statements in [20], as Bishop's influence on the design was at the "requirements" level: the system had to be adequate to formalize Bishop's work, and hence finite type theory would not do; a more elaborate system had to be developed, and Martin-Löf's theories met that requirement, in the judgment of Constable.

In turn, NuPrl and Bishop both influenced the design of later proof-checkers. By 2006 there were at least seventeen computer systems designed to check proofs; see [26] for descriptions and comparisons of seventeen proof checkers. But of those seventeen, only a few work with constructive logic and/or permit the extraction of algorithms from proofs. Besides NuPrl, we have Coq, Minlog, and Alfa/Agda.

The proof-checker that most nearly corresponds to a "computer oriented version" of constructive mathematics is Coq. The native logic of Coq is intuitionistic logic. Coq was developed in France, but in Nijmegen, there has been for some years an intensive project to develop the "Coq Repository Nijmegen" (CoRN). The design of CoRN has been heavily influenced by Bishop, and it has been used to formalize parts of real analysis in Bishop's style [13]. In 2008, Georges Gonthier used Coq to prove the four-color theorem [15]. Previous proofs made use of computer programs to carry out certain searches, but those programs were never proved correct, leaving some room for lingering doubt about the correctness of the overall proof. But Gonthier's proof in Coq formalized the entire proof, including the proof of correctness of the algorithms. One can extract a coloring algorithm from Gonthier's proof, but the word "extract" is misleading, or even wrong, since Gonthier had the algorithm in mind and works hard to make his algorithm as efficient as possible, given the Coq framework. Nevertheless, the application of Coq to a problem of such historical significance was certainly a milestone. If Bishop could have seen Coq and CoRN, and the work formalized with these tools, he would have been interested and pleased.

Minlog allows one to work in intuitionistic or in classical logic, and amazingly even allows one to extract algorithms from some classical proofs; e.g. an algorithm for the greatest common divisor of m and n can be extracted from the classical proof that the least positive linear combination of m and n is their greatest common divisor. The designer of Minlog is Helmut Schwichtenberg, who has been familiar with the work on formal systems for constructive mathematics ever since the time when Bishop's book was published, and was certainly influenced by Bishop and by the logical and mathematical work that followed. Alfa/Agda was created in 1990 and uses a logic based on Martin-Löf's theories, so one cannot point to a direct influence of Bishop.

5 Life of Errett Bishop

Errett Albert Bishop was born July 24, 1928, in Newton, Kansas. His father, Albert T. Bishop, graduated from the United States Military Academy at West Point. Wikipedia says that Albert ended his career as professor of mathematics at Wichita State University in Kansas. It also says that Errett grew up in Newton, Kansas, and that Albert died when Errett was five. Newton is farther from Wichita than anyone commuted in those days, which is consistent with the MacTutor web page on Bishop, which says (without a reference) that Albert was forced by illness to retire early. His father influenced Errett's eventual career by the textbooks he left behind, which is how Errett discovered mathematics. Bishop entered the University of Chicago at the age of 16, and in three years obtained both his B. S. and his

M .S. degrees in mathematics. Then, according to Wikipedia, he began studying for his doctorate; but he performed two years of service in the US Army, 1950–52, doing mathematical research at the National Bureau of Standards. He completed his Ph. D. in 1954 under Paul Halmos; his thesis was titled *Spectral Theory for Operations on Banach Spaces.* Halmos tells in [16] that Errett wrote *two* Ph. D. theses. After his first one was finished, it was discovered that the same results had been obtained earlier by a Russian. Halmos told Bishop that it was not a problem, because Errett's work was independent. But Errett said no; he would write another thesis, and he did. This story was also related to me by a fellow student of Bishop's, who said that Errett (even in his student days) was "a strong person; he knew what he wanted, and was respected by everyone."

Errett was 39 years old when this book was published in 1967. He died of cancer on April 14, 1983, at the age of only 55. According to Wikipedia, he "became interested in foundational issues" in the 1964-65 academic year, which he spent in Berkeley at the Miller Institute for Basic Research; but in the acknowledgements of this book, Bishop says his work on the book was supported by NSF grants for the summers of 1964, 1965, and 1966, as well as by the Miller Foundation, so it seems he was working on it for at least three years. It is natural to wonder whether there was some event or experience that turned his interest to constructivity. Metakides and Nerode asked him about that [20], and "he said that the fact of the matter was that he thought by nature constructively from the beginning of his mathematical life, and that the book was a natural outgrowth of his mathematical temperament." He also told them that "he had been influenced by Weyl's book, and had looked briefly at Brouwer, but had avoided detailed study of Brouwer for fear of being led away from his own natural lines of development." This reminds me of Feynman, who, when working on an unsolved problem, did not read the work of other physicists because "they didn't get the answer."[3]

[3]Related to some students over lunch in a Caltech cafeteria in about 1966. I was one of those students.

From 1964 on, constructivity seems to have occupied his attention, as he was engaged right up until his death in the preparations for a second edition of his book, as described above. In the middle of his work on his book (1965), he moved to San Diego, and remained professor at the University of California, San Diego, for the rest of his life. The decision to leave Berkeley preceded the decision to go to UCSD; faculty at UCSD heard of this decision and recruited Bishop, calling him while he was visiting Yale to urge him to not make a decision until he had seen UCSD [25]. Bishop was full professor at Berkeley–why did he decide to leave? I was told by someone who heard it directly from Bishop that it was because of the student unrest. The Free Speech Movement took place from October 1, 1964 through January of 1965, so the timing is consistent. If Errett wanted to avoid a campus climate that included unruly political activity, he was right to leave when he did, as Berkeley continued to be tumultuous until after the Vietnam war. He probably had other reasons as well; it is a major decision to make based on a few demonstrations.

He had three Ph. D. students at Berkeley, who wrote theses about uniform approximation and interpolation (in the time preceding his interest in constructive mathematics). At UCSD, he had eight more Ph. D. students (according to [25]); the Mathematics Genealogy Project lists six of these, five of whom wrote theses with the word "constructive" in the title, providing further evidence that constructivity continued to hold his attention. We have already mentioned a reason why Bishop stopped supervising such theses–it was not because he lost interest.

Errett's colleague Stefan Warschawski described him [25] in these words:

> Errett was a remarkable personality. Particularly outstanding traits were his independence and originality, apparent in everything he did, in his research, his teaching, in every aspect of daily life. He had strong principles by which he lived and a strong feeling for fairness in the treatment of other people—his colleagues, his students. He treated people with

> kindness and consideration. He was a very private person and did not talk much about himself.

Warschawski also tells us that Errett had a stamp collection and a collection of Indian and Mexican artifacts.

Errett was survived by his wife Jane and two sons, Edward and Thomas, and a daughter Rosemary. Edward earned a Ph. D. in mathematics from the University of California at San Diego in 1991.

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