

Tributes to Bill Arveson

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FIGURE 1. Source: Oberwolfach photo collection, copyright George M. Bergman, Berkeley

Introduction

“...I saw that the noncommutation was really the dominant characteristic of Heisenberg’s new theory. It was really more important than Heisenberg’s idea of building up the theory in terms of quantities closely connected with experimental results. So I was led to concentrate on the idea of noncommutation and to see how the ordinary dynamics which people had been using until then should be modified to include it.”

—P. A. M. Dirac, from *The Development of Quantum Theory* (J. Robert Oppenheimer Memorial Prize Acceptance Speech), Gordon and Breach Publishers, New York, 1971, pp. 20-24.

William Arveson’s work has been extraordinarily influential, and it is known to everyone in functional analysis and in operator algebras. Bill’s career spanned UCLA, Harvard, and (since 1968) UC Berkeley, where he had 29 PhD students and also mentored several postdocs. And as we prepared this tribute we were struck by the sheer number of spontaneous notes or comments from mathematicians who felt personally inspired by his papers in the beginning of their careers.

Functional analysis and operator algebras owe much to Hilbert’s and von Neumann’s pioneering visions for a rigorous mathematical foundation of quantum mechanics (Hilbert’s Sixth Problem [Wig76], and see also [Dir26]). Two other areas motivated these subjects from the start: ergodic theory, and the study of unitary representations of groups, especially the Lie groups arising in relativistic quantum theory. Bill Arveson told us that von Neumann and Norbert Wiener were his two mathematical heroes, and the non-commutativity that lies at the heart of quantum theory exerted great fascination for him.

Over decades, Bill pioneered making sense of deep questions regarding non-commutativity, and these developments became an integral part of great advances that propagated to other fields. Non-commutative harmonic analysis and non-commutative geometry [Con94] in particular have become flourishing and active research areas. The first has direct relevance to signal and image processing (inspiration from Wiener, for more details see [Jor03]), and the second offers insights to many generalizations of Atiyah-Singer type index theorems, to diverse applications, including in physics (e.g., sets from aperiodic tilings determining geometries of solid-state quasicrystals), in stochastic processes, and in engineering.

Bill's work often presented entirely new perspectives to problems, introducing new technical tools and breakthroughs which later proved essential for the solution of some of the deepest and most celebrated questions in the field. We only mention a few examples, among others which will be explored in more detail below by the individual contributors to this tribute article.

Let us remark on a connection with quantum theory. Fixing an algebra \mathcal{A} of operators, the selfadjoint elements in \mathcal{A} are candidates for quantum observables. In the case of unbounded observables, such as the energy H (usually semibounded), one considers bounded functions of H . One must show that these are in the von Neumann algebra of observables, see [Bor66, Haa96]. For decades, there was no rigorous formulation of dynamics in the theory that encompassed the essential physical requirements. Arveson attacked this problem by introducing a spectral theory for non-commutative dynamical systems [Arv74], which had numerous other applications. For example, the Arveson Spectrum inspired two new and powerful von Neumann algebra invariants (the S-invariant, and the T-invariant, see [Con94]) that served as key ingredients in the completion of the classification problem for hyperfinite factors (von Neumann algebras with trivial center).

Perhaps Bill's most famous result is his extension theorem, which plays an analogous role for operator-valued maps on subspaces of operator algebras as the Hahn-Banach theorem for the extension of linear functionals on subspaces of Banach spaces. Arveson was the first to recognize the necessity of considering matrix norms for the extension, in essence starting the theory of operator spaces and operator systems. This attracted the attention of many and laid the foundations for the study of injective von Neumann algebras and operator systems, and later nuclear C^* -algebras. Furthermore, in the very same paper Arveson started a program applying these new ideas from operator algebras to the study of important problems in operator theory. This started a significant far reaching trend. And it is worth noting that some open questions from the program started in [Arv69] were finally solved by Arveson himself, almost 40 years later, in a remarkable paper [Arv08]!

Bill's career was long and fruitful, and we refer the reader to two recent survey articles on some of his many contributions to mathematics [Dav12, Izu12]. Bill's work was often inspired by problems from physics,

but this was by no means the full story. No matter the source of his inspiration, however, Bill produced many “pure” theorems of unusual elegance and striking beauty.



FIGURE 2. Bill Arveson as a Benjamin Peirce Instructor at Harvard (1965-68) (source: Lee Kaskutas)

Kenneth R. Davidson¹

Bill Arveson completed his doctorate in 1964 at UCLA under the supervision of Henry Dye. After an instructorship at Harvard, Bill started a long career at the University of California, Berkeley. I was a student of his in the early to mid 1970s. Bill was still young, but already had had a strong influence on operator theory and operator algebras. The influence of this early work continued to grow in the following decades.

Arveson’s work was deep and insightful, and occasionally completely revolutionary. When he attacked a problem, he always set the problem in a general framework, and built all of the infrastructure needed to understand the workings. This perhaps is the reason that his influence has been so pervasive in many areas of operator theory and operator algebras.

In the introduction to a 1967 paper, Arveson wrote “Many problems in operator theory lead obstinately toward questions about algebras that are not necessarily self-adjoint.” This has been a common theme in his work, interweaving ideas from self-adjoint and nonself-adjoint operator algebras.

There is no space here to review all of the many contributions that Bill made to operator theory and operator algebras. I must mention his famous papers in *Acta Math.* on dilation theory. He developed a framework for

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dilation theory for an arbitrary operator algebra based on the Sz. Nagy dilation theory for a single operator. The key ideas were that the operator algebra \mathcal{A} was a subalgebra of a C*-algebra. Secondly, the right kind of representations were *completely contractive*, and yielded *completely positive* maps on $\overline{\mathcal{A} + \mathcal{A}^*}$. Thirdly, he claimed that every operator algebra \mathcal{A} lived inside a unique canonical smallest C*-algebra called the C*-envelope.

While these results are fundamental for nonself-adjoint operator algebras, they had immediate consequences for C*-algebras as well. His use of completely positive maps, and proving that $\mathcal{B}(\mathcal{H})$ was injective in this category, led to deep work on injective von Neumann by Connes, and nuclear C*-algebras by Lance, Effros and Choi. When Brown, Douglas and Fillmore did their groundbreaking work on essentially normal operators and the Ext functor for C*-algebras (a K-homology theory), Arveson pointed out how one gets inverses in the Ext group using completely positive maps. Later he wrote an important paper introducing quasicontral approximate units for C*-algebras, and used this to provide a unified and transparent approach to Voiculescu's celebrated generalized Weyl-von Neumann theorem, the Choi-Effros lifting theorem, and the structure of the Ext groups.

The idea of C*-envelope took longer to develop. It was a decade before Hamana established its existence in general. It took another decade before good tools for computing it were developed. But in the past two decades, it has been a central tool in studying nonself-adjoint operator algebras, as imagined by Arveson back in the late 60s. A new proof due to Dritschel and McCullough provided important new insights into the structure of this C*-algebra. Arveson revisited his early approach, and was able to establish a stronger form (analogous to the Choquet boundary as compared to the Shilov boundary), as he had conjectured in 1969.

Arveson tackled the problem of invariant subspaces from a completely different point of view. In a small paper, he showed that an algebra of operators containing a maximal abelian self-adjoint subalgebra of $\mathcal{B}(\mathcal{H})$ (a masa) with no invariant subspaces was weak-* dense. When Radjavi and Rosenthal extended this result to algebras whose lattice was a nest, Bill revisited the problem and in a 100 page paper in *Annals Math.*, he developed a spectral theory for reflexive operator algebras containing a masa. Using this, and making connections with spectral synthesis in harmonic analysis, he showed the limits of this kind of result. This became an important class of operator algebras (CSL algebras).

Even 'failed' attempts had profound positive impacts. Arveson tried to provide an operator theoretic proof of Carleson's famous corona theorem about the maximal ideal space of H^∞ of the disc. In his paper, he developed the distance formula for nest algebras, now called the Arveson distance formula. He also established a weaker version of the corona theorem. This became known as the operator corona theorem. It has proven to be an important stepping stone to the full result on other domains.

I will skip ahead in time, passing by many important results, to the current century. Arveson tackled the problem of multivariable commutative operator theory. The ideas of dilation theory, now well established, suggest that one should understand the universal operator algebra determined by appropriate algebraic and norm constraints. A number of authors, in the non-commutative setting, had observed that a row contractive condition was proving to be much more amenable than insisting that individual generators be norm one. Bill applied this to an n -tuple of commuting operators. The canonical model that he developed was the space of multipliers on symmetric Fock space. This space turned out to have other remarkable properties. Arveson showed that it was a reproducing kernel Hilbert space of functions. David Pitts and I showed that it was a complete Nevanlinna-Pick kernel, and Agler and McCarthy showed that it was the universal complete NP kernel. These three results came from different directions, but served to make operator theory on this space a rich venue for analysis and algebra.

Bill went on to write a long series of papers on this operator algebra. He introduced many ideas from commutative algebra into the program. He developed a notion of curvature as a key invariant for commuting row contractions, and many other ideas. He made an important conjecture which has generated a tremendous amount of work by many authors. As with his earlier work, he had the good taste, the vision and the mathematical power to establish a powerful new approach to an important problem.

This is his legacy—a deep and powerful vision of operator theory and operator algebras as an integrated whole. He brought ideas from function theory, harmonic analysis, commutative algebra, geometry and physics to bear on problems in operator theory and operator algebras (two areas I am sure that he considered as one), and produced works of art that have attracted almost every practitioner of this subject at some time. He has had a profound impact; and this impact will continue for a long time to come.

It has been my honor to have been a student of Bill's. His work influenced me more than most, since most of my work, traced to its roots, goes back to Bill in some way. I am glad that I had the privilege to know him.

Ronald G. Douglas²

In January, 1965, after giving a ten-minute talk at the annual American Mathematical Society meeting, held that year in Denver, a graduate student came up to me and asked a question. I don't recall what he asked but I do remember the event because it was the first time I met Bill and our mathematical careers became intertwined from that point on. We became, and remained, strong friends and colleagues over the next almost fifty years. Let me recall some of the highlights of that professional friendship as I remember them. I make no effort at any sense of completeness.

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FIGURE 3. Bill Arveson and Ronald Douglas at COSy 2006
(source: Marcelo Laca and Juliana Erlijman)

During the next couple of years, our paths didn't cross. While I spent the following year at the Institute for Advanced Study in Princeton, Bill completed his doctorate at UCLA. When I returned to Ann Arbor, Bill was off to Cambridge to become a Peirce Instructor at Harvard. Still, in the Fall of 1966, I received a preprint in the mail from him detailing some results he had obtained on the existence problem for invariant subspaces for bounded operators on Hilbert space. In his work he investigated the closability of certain partially-defined linear transformations. I was both intrigued and impressed and presented his results in Halmos' seminar at Michigan. The research had many of the hallmarks characteristic of Bill's approach to mathematics: deep, incisive, often unexpected results obtained by applying technical machinery which often he had built himself and, many times, apparently unrelated to the problem at hand. Moreover, it showed that Bill was not hesitant to strike out in new directions. His thesis had concerned the classification of algebras of operators defined using measurable transformations, but none of that was present in this new work on a very different problem.

In August, 1966, I attended the International Congress of Mathematicians held in Moscow, enabling me to meet the larger world of operator theorists including many from the Eastern bloc. Having gained a broader view of the

subject, I decided to try to bring many of the practitioners together, at least the Western ones, in Ann Arbor, for the month of July, 1967. Bill visited for the first week and we had the opportunity to share ideas and approaches, not only to mathematics but to life in general. Most nights were spent at the “Pretzel Bell,” drinking beer and listening to Dixieland jazz. The following summer, Bill arrived in Berkeley, where I spent a month at the AMS Summer Institute on Global Analysis. We continued our wide-ranging discussions, often in Bill’s office in T5, a “temporary” building which housed part of the department before Evans Hall was completed. Our children played together during many of the days.

Over the next few years, Bill moved on and was now considering a “non commutative” analogue of function algebras and obtained one of his iconic results, the dilation or extension theorem and made clear the importance of the notion of completely positive and contractive maps. After I moved to Stony Brook in 1969, we got together on both coasts and at conferences around the world including ones I recall in Dublin and Krakow. Many of my visits to Berkeley coincided with singular events such as Peoples Park. Bill’s beliefs and temperament resonated with that somewhat raucous period. In 1974 Bill published a paper in which he obtained another legacy result on reflexivity of weakly closed non selfadjoint subalgebras. Through a series of deep reductions based on machinery he constructed, he first extended positive results of Heydar Radjavi and Peter Rosenthal. Moreover, he then obtained a remarkable counterexample to the general question by reducing the question to a spectral synthesis problem. The failure of the latter for the two sphere in three space allowed him to show that weakly closed subalgebras containing a MASA are not always reflexive. In the same year he published a seminal paper on transformation groups returning to the theme of his thesis. During this period he became one of the pillars of the functional analysis group at Berkeley.

In the early seventies, I collaborated with Larry Brown and Peter Fillmore to produce the body of results usually known as BDF theory. Classes of operator algebra extensions were made into an abelian group which could be calculated resulting in some, then rather surprising, results in operator theory. I had many discussions with Bill in the middle seventies in which he wrestled with these ideas trying to fit them into his context. No surprise – he did! He saw the bigger picture relating the group structure to certain questions in operator algebras involving completely positive maps and nuclearity.

In Spring, 1980, Bill and I, and several other operator theorists, were invited to spend one to two months at the Mittag-Leffler Institute outside Stockholm, to work through Per Enflo’s paper on a Banach space operator without proper invariant subspaces. Although all of us spent some time on that project, more time was devoted to developing ideas involving operator algebras, related to index theory on my part and completely positive maps

and C^* -algebras for Bill. He also had the opportunity to explore the Swedish branch of his family tree. Finally, I recall riding between the cars on a train ride to a conference in Goteberg so Bill could enjoy perhaps his favorite vice.

The machinery connected with BDF theory Bill provided helped extend the ideas and provide the extension framework for Guennadi Kasparov's KK-theory. Further, revolutionary development of these ideas by Alain Connes, Kasparov and many others led to the Special Year at MSRI in Berkeley in 1984-85. By this point, Bill was well on his way to inviting an outstanding group of young mathematicians into the field and his seminar was a must for everyone interested in linear analysis, both as a speaker and an attendee. Bill also participated in the social life surrounding the program at MSRI. On one Friday evening, he offered to show a group of perhaps ten, "his San Francisco." After dinner and wandering through the North Beach area, we ended up at Carol Doda's club, where she invited Bill on stage to join her. Bill didn't disappoint.

Next Fall, back in Stony Brook, I got a call from Bill. He was in New York and invited my wife, Bunny, and me to come into Manhattan to meet Lee. On his return flight from Tokyo, following a visit to China, he had met her. Although she was seated in first class while he was in "steerage," he had managed to talk with her and the two of them had been talking and meeting since. Over the years my wife and I got together with Bill and Lee, now his wife, many times in Berkeley and at conferences around the world.

In 1988, Bill and I jointly led an AMS Summer Institute in Operator Algebras/Operator Theory. In part, we were making an effort to keep the two communities from fracturing since each of us had a foot in each camp. The program was held at the University of New Hampshire in July where we assumed the weather would be welcoming. It was the hottest summer in memory on the East Coast including Durham. With little air conditioning and fans in short supply, it was a hot month. But the mathematics was "hot" too and almost everyone involved in any aspect of the subjects, worldwide, participated. For the final week of the workshop, both couples moved into the air conditioned campus hotel.

Over the next decade or so, our research interests diverged, although we often shared ideas and kept up with what the other was doing, usually in Berkeley or at meetings. One I recall fondly is the NATO meeting in Istanbul in June, 1991.

Perhaps the most singular event of those years was the loss of the hillside house of Bill and Lee in the Oakland Hills to a wildfire. Rather than admit defeat, however, Bill and Lee plunged into the task of rebuilding and refurbishing an even better house. The energy and enthusiasm one would encounter upon one's arrival in Berkeley during those years was nothing short of amazing. Still the mathematics flowed since this was the period in which Bill took up and transformed the endomorphism problem of Bob Powers.

With my move to College Station in February, 1996, and my new administrative role at Texas A & M University, I saw less of Bill although he did deliver a series of lectures at A & M during the early years of my tenure as provost. I noted that he had gotten interested in an approach to a topic which I had been exploring – Hilbert modules. As was usually the case, Bill had his own ideas about the subject, had obtained some deep, unexpected results, and had formulated some challenging questions. His approach was based, in part, on his earlier work on subalgebras of C^* -algebras and published the third paper in the series.

While attending a conference at Berkeley in February, 2003, honoring Donald Sarason, Bill told me that he had come upon a problem he wanted to discuss with me. During the rest of that year and at conferences that December in Bangalore and Chennai, we discussed his problem. He was trying to get C^* - or “quantum” models for projective varieties in \mathbb{C}^n . He sought to show that the closure of a homogeneous polynomial ideal in the symmetric Fock space is essentially normal; that is, the cross-commutators of polynomial multipliers and their adjoints are compact. (Actually he conjectured that they are in the Schatten-von Neumann p -class for p greater than n .) He was able to show that this was the case for homogeneous ideals generated by monomials but not in general. I became intrigued and was able to extend his results modestly. Both Bill and I announced, at different times, proofs of the conjecture which turned out to be incomplete. In talks, Bill spoke of the “witch’s curse” on this problem and indeed at least one other incorrect proof has been announced since then. The question is deep and has attracted the attention of researchers around the world but the general case remains open.

The last time I saw Bill was in August, 2008, at SUMIRFAS, a conference held in College Station, where he talked on quantum entanglement. He had realized that some of his earlier work on completely positive maps was closely related to this phenomenon from physics, but he didn’t stop there. After establishing this relationship, he went on to uncover surprising applications of these ideas and raise questions about others.

As various emails and papers make clear, Bill was doing mathematics till the end. He will be, and is, missed but his mathematical legacy is strong and will live on.

Edward G. Effros³

The functional analysts at UCLA were devastated by the news that Bill had passed away. He was one of the key figures in the development of non-commutative functional analysis and its applications to a wide range of mathematical disciplines. I will largely restrict my remarks to several of Bill’s papers on linear spaces of operators.

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FIGURE 4. Bill Arveson with Edward Effros (source: Lee Kaskutas).

One of Bill's most influential discoveries was that one could develop a theory of boundaries for the operator algebraic analogues of function algebras [Arv69]. His key observation was that linear spaces of operators have a hidden matricial structure that must be incorporated into the theory. This rests upon the fact that a matrix of operators is again an operator, and thus the matrices over an operator space is again an operator space. The ordering and norms of such matrices is an essential part of the relevant structure, and must be acknowledged by the morphisms, i.e., by the completely positive and completely bounded operators.

Although complete positivity had been investigated earlier by Sz. Nagy, Stinespring, and Umegaki, Bill was the first to appreciate the power of these notions. The crowning achievement of his early theory were analogues of the Hahn-Banach theorem for completely bounded and for completely positive mappings (put in its final form by Wittstock [Wit81]). He used this theory to prove important results about matrix numerical ranges.

Soon the young operator/functional analysts jumped on the matrix ordered version of Bill's theory (operator systems), and before very long, the injective (or semidiscrete) von Neumann algebras were characterized as being the hyperfinite von Neumann algebras (work of Connes, Choi, Lance, and myself). Of course, there were many other directions to be pursued, and within a few years, the nuclear C^* -algebras were determined (Choi and myself, and some parallel work by Kirchberg), and lifting theorems were proved (relevant to KK theory).

Owing to Ruan's axiomatization of the operator spaces (the "quantized Banach spaces" [Rua88]), the full significance of Bill's approach to matrix

norms is now also understood. This has enabled researchers to find non-commutative analogues of many of the notions of Banach space theory (see [ER00, Pis03]). Very recently, the matrix ordered operator systems have seen an upswing of interest, due to the work of Vern Paulsen and his colleagues [P⁺]. Yet another application of these ideas may be found in the abstract characterization of the non-self adjoint unital operator algebras [BRS90]. This provides an elegant framework for Arveson's original investigations.

Bill's interests ranged over a wide range of subjects, and he influenced several generations of mathematicians. A particularly intriguing example of this work was his theory of continuous tensor products, which was also pursued by Bob Powers, and then by Boris Tsirelson. What was truly remarkable about Bill was that his productivity never declined throughout his mathematical career. He was always ready to tackle a completely new area. This is illustrated by some of his last papers, which are concerned with quantum information theory.

Although I have never worked on non-commutative boundary theory, I would be remiss if I did not recount one of Bill's most spectacular recent results. Nearly forty years before, he had posed the problem of determining if operator systems have sufficiently many boundary representations. Important contributions had been made by a number of individuals, including Dritchel and McCulloch, Muhly and Solel, as well as Ozawa. In [Arv08] he finally succeeded in proving the result for separable operator systems, by using delicate direct integral techniques. This is an "old-fashioned" technology (dear to my "Mackey heritage") that might not have been appreciated by his younger colleagues.

Upon the appearance of that work, I couldn't resist writing to him that he "had shown all those young whippersnappers a thing or two". He gleefully replied that he shared that opinion, and then he characteristically sent me a fascinating paper on operator systems on finite dimensional Hilbert spaces [Arv10]. I am only just beginning to realize its importance.

Having summarized so much of Bill's professional accomplishments, I would like to add a final personal memory of how non-mathematicians viewed Bill: I was with my family at Victoria Station in London, probably in the late 1980's, awaiting the train to a math conference somewhere in the UK, when we bumped into Lee and Bill, who were enroute to the same meeting. We all spoke for a while, then moved on so that we could get a bite to eat. Our teen-age daughter asked how we know those two people, and I mentioned that Bill was a mathematician. Having met many of my colleagues over the years, she looked totally shocked, and said "That guy seems much too cool to be a mathematician!".

Bill, you will be irreplaceable.

Richard V. Kadison⁴

Bill and I met during his graduate student days at UCLA. He reminded me of that, with a smile, on a few occasions, each time I said that we had met during the so-called “Baton Rouge Conference” (at LSU in March of 1967). After two or three corrections, much to Bill’s amusement each time, I finally got that straight (I’m a slow learner – but I, then, retain it tenaciously). As I was just noting, when I first met Bill, at that Baton Rouge conference, the year was 1967, the same year in which Bill’s great paper in *Amer. J. Math.* appeared. We’ll have more to say about that paper at a later point. It was clear to me that Bill was a very smart young mathematician. What I hadn’t known, until we had that time to talk to one another, was that Bill had a personality that was very congenial to my own way of doing and thinking about things. Bill was articulate and clear, with the kind of humor that I enjoy. He had a candor, at least when talking with me, that I appreciated. It wasn’t “kick-in-the-shins” candor, the kind that hurts people, without much extra purpose. When I listen to some people, who pride themselves on being “candid,” I feel that they are deriving at least as much pleasure from being cruel as from being “forthright.” I never detected one scintilla of cruelty in Bill’s interaction with people. What one could observe about Bill was that he had an abundance of what the young people, these days, call “cool.” At a conference in England (Durham, I think) that Bill, Ed Effros, and I were attending, I talked to Rita Effros during a lunch break. She reported that her son, then a youngster, had remarked to her, the preceding evening, that “Bill Arveson was the coolest mathematician he had met.” At the same moment in which she told me that, she realized that she might have offended me by not saying that her young son thought that I was “cool” as well. Now, Rita is as sweet and kind as they come, to which everyone who knows her will attest. But level of “cool” is not one of the axes in my personality description on which people are prepared to place a mark. Bill’s “cool credentials” are, however, unassailable.

I dwell on our meeting, the “Baton Rouge conference,” and Bill’s paper [Arv67], in which I browsed somewhat carefully, but not as carefully as I should have, as later years were to reveal because that paper is the basis of a tangled mathematical tale that occupies part of this vignette. At the same time, it provides a glimpse of Bill and of the relationship Bill and I had. I’m sorry that so much of *my* work and activities appears in this remembrance; I don’t know of another way of describing the interaction between the two of us.

Given our interests and general view of how mathematical development should proceed, Bill and I were certain to become good friends and to meet often at conferences. One such meeting took place at a conference at the

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University of Newcastle-on-Tyne in England near 2000. That conference, on Banach Algebras, was in honor of Barry Johnson, an heroic figure in that and allied subjects. There was a sadness about that meeting. Barry had terminal cancer and was near the end of his life. We all knew of Barry's mathematical heroism. On this occasion, we had the unhappy opportunity to note his physical courage as well. He attended a number of lectures, clearly with effort and in some pain, yet attentive and interested. Both he and Bill were at my lecture – paying close attention. I was speaking about some work I was doing related to the Pythagorean theorem — what is often referred to as the “converse” (if the numbers are right, there is a right triangle with sides of those lengths) [Kad02a]. I refer to that converse as “the Carpenter’s Theorem,” to distinguish it from the usual formula. (Carpenters use that converse to check right-angled constructions, as I learned from my wife’s youngest brother, a carpenter, via my son.) As one takes this (the converse, that is) to higher dimensions, the problem becomes a fascinating little matrix problem that looks very simple but is devilishly difficult. It can be formulated in several different ways, but the most primitive and innocent is the following. Given n non-negative real numbers for the diagonal entries of an $n \times n$ matrix can the remaining ($n(n - 1)$ off-diagonal) entries be prescribed so that the resulting matrix is a projection (a self-adjoint idempotent)? One realizes, quickly, that those prescribed real numbers must sum to a positive integer, the rank of the projection. As strange as it may sound, the affirmative answer to this little question is the converse to Pythagoras. (See [Kad02a].) I struggled with this for longer than I am happy to admit, approaching it as a “primitive” (fiddling with matrices and with the geometric form of the problem – yes, there is one, and it is tantalizing), until I had an important epiphany, to wit: being a Functional Analyst I should act like one. Shortly after that, something I was doing reminded me of a key lemma G. K. Pedersen and I produced in connection with some convexity result we had proved in the early eighties [KP85]. After that, the finite-dimensional results and some important segments of the infinite-dimensional case fell to the technique associated with that convexity lemma. There was a result “settling” the rest of the finite-dimensional case. The only problem was that I had both a proof and a counter-example to that result. I mentioned this during that Newcastle lecture in a joking way, “I seem to have a proof that the Earth is flat – or any other assertion you care to have proved.” If Barry Johnson had been healthy, this subject would look different, now, I feel. Bill was (or seemed) well at that time, and the subject *is* different as a result.

Bill and I met at the end of the following academic year, while I was visiting Berkeley for a week or two. We were having lunch together at the Berkeley faculty club (or whatever it is called these days). As we waited for our sandwiches to be made, Bill mentioned my Newcastle lecture and a feeling he had about some of the results I reported there. He had had, then,

and still had, as we spoke, a vague feeling that some old work of a former teacher of his, A. Horn, was related to what I was describing. Bill was, of course, probing for a response of the form, “Oh, yes. That is so-and-so, whereas, my work is \dots ,” and so forth. The other possibility was that I hadn’t thought about it or any relation to my work, and perhaps didn’t even know of Horn. As it happened, I had certainly not thought of Horn’s work, but I had received a reprint [Hor54] when it first appeared (in 1954). I glanced through it and determined that it was far from anything I was doing at that time. Moreover, it seemed scrambled and unclear (although, I was well aware that I was looking at it very superficially). I mentioned some of this to Bill. He responded that he had liked his teacher and planned on taking a closer look at this if I didn’t mind. Of course I didn’t mind. Bill had my two PNAS reprints [Kad02a, Kad02b]; so, we left it at Bill’s getting back to me when and if he found a connection.

A few days later, while I was still in the Berkeley area, I received a phone call from Bill. He said that he had looked at Horn’s work again and “built a small bridge” from something in that work to my result; but he could not understand the argument in that work, even after thinking seriously about it. At this point, I would like to include a copy of a few-page report I wrote to Is Singer, in a letter dated November 2, 2006. Is was acting as editor of an article, submitted to the Proceedings of the National Academy of Sciences (USA), [Arv07] growing out of these discussions. I was reviewing (“refereeing”) the article for Is. I hope that the readers will find this “nugget of memorabilia” interesting and informative. It contains sufficient description and mathematical background to allow me to begin afterward with the closing vignette. I feel, too, that it offers a very good picture of Bill’s style and his strength as a mathematician. It also gives a further view of that “contradiction” with which I struggled and how it resolved itself.

“Dear Is,

Here is the longer letter on the Arveson paper “Diagonals of Normal Operators With Finite Spectrum,” which I promised you in my e-mail report. Bill’s work grew out of my Pythagoras work. It was joint work with me. At the point where I felt that the extent to which I was delaying Bill was unconscionable, I cut myself adrift and told him to publish this part as his own. (There were, already a few joint items.) Bill had done so much and waited so long and patiently for me to add the things I might be able to do and wanted to think about that I felt he must be allowed to proceed without me dragging and bumping along behind him. It has also been a relief for me to shed the mountain of guilt that accompanied my interminable delays. Well, Bill

has come to a watershed, it's far from the end of the journey, but it's an interesting and important advance along that road. It certainly deserves to be published in PNAS.

Let me say some more about the work (Bill's and mine). It is deeply and inextricably related to things you probably know well. I'm referring to the Atiyah and the Guillemin-Sternberg results on convexity, moment map, and Hamiltonian dynamical systems. It's also related to the combinatorist's work on Schur bases and MacDonal'd polynomials (though less closely). So our project has plenty of mathematical "scope." All this stems from a (gorgeous!) 1923 paper by I. Schur [see [Sch23] –Ed.] and a later paper (1954) by Al Horn [see [Hor54] –Ed.], a (then) young Assistant Professor at UCLA (maybe a contemporary of yours?) and a teacher of Bill's. Schur undertakes to extend some 1893 results of Hadamard, a determinant inequality (for a positive hermitian matrix: the product of the eigenvalues does not exceed the product of the diagonal values). Schur's paper is deep and rich. He develops a string of inequalities going from the Hadamard determinant inequality to "trace" inequalities. These trace inequalities are what Bill and I have been calling the "Schur inequalities." Along the way, Schur studies functions that preserve operator ordering introducing his uniformly, multivariable convex functions, developing his Schur bases for symmetric functions, inventing doubly stochastic matrices. The "other end" of the line from the determinant inequalities, the Schur inequalities, states that if A is a self-adjoint matrix with eigenvalues $\lambda_1, \dots, \lambda_n$ listed in decreasing order and a_1, \dots, a_n are the diagonal entries of the matrix, then $a_1 + \dots + a_k \leq \lambda_1 + \dots + \lambda_k$ for k in $\{1, \dots, n-1\}$. Of course, $a_1 + \dots + a_n = \text{trace } A = \lambda_1 + \dots + \lambda_n$. If we examine the case where the spectrum of A consists of 0 and 1, that is, where A is a projection E , and make the (almost "automatic") assumptions that $\lambda_1 + \dots + \lambda_n = a_1 + \dots + a_n = \text{tr}(E)$ and $a_j (= \langle Ee_j, e_j \rangle)$, where $\{e_1, \dots, e_n\}$ is the orthonormal basis relative to which A has the given matrix) lies in $[0, 1]$, then the Schur inequalities are also automatic. So, Schur gives us necessary conditions on the diagonal for the general hermitian matrix which are not restrictive beyond the obvious when the hermitian is an orthogonal projection. That's the door thru which I entered all this: trying to find what the diagonal of a projection could be. (Don't ask why I was interested; I could explain, but it's too long.) When I began, I knew nothing of Schur or Atiyah and Guillemin-Sternberg

and “convexity theory” in symplectic geometry (my knowledge of this latter is still superficial.) I didn’t even remember Horn’s 1954 paper (he sent me a copy, but it looked like a mess). In effect, Horn sets out to find what the diagonal of an orthogonal transformation can be (relative to various orthonormal bases). Along the way, he is looking at the converse to Schur’s result (in the form of proving that Schur’s conditions are sufficient as well as necessary). I can’t say that he has proved it; he has a lot of “airy” allusions to the right sorts of things, but I don’t find it a proof. Bill couldn’t understand it at all. As I said, Bill became interested in this when he heard one of my “Pythagoras” lectures. He remembered one of his teachers at UCLA, Horn, and something Horn showed his class. Bill was able to build a small bridge from Horn’s proof; so he didn’t really have a proof of the finite-dimensional case. He felt really badly about that because he wanted to push on to the infinite-dimensional case. When I looked at what Horn wanted to do (remember, I hadn’t known of Schur and paid no attention to Horn’s paper when I got it – and had even forgotten it), I saw that what I had done in the projection ($\{0, 1\}$ spectrum) case applied almost without change to prove the Schur converse. Bill was so pleased to finally see a proof of the Schur converse in the finite-dimensional case (and one that was absolutely clear, correct, and complete, though not so easy) that he suggested we throw in together on the infinite-dimensional Schur-Horn. We did the Schur inequalities two ways in that case. One way was primarily mine, doing all sorts of delicate boundary cases by fancy conditional-expectation techniques in both the discrete and II_1 cases (it was complicated – necessarily, for accuracy – and heavy as a Russian tragedy), and the other way, primarily Bill’s, nice, lighter, without the boundary cases, easier reading but making use of other work (Weyl inequalities and such). I could see that Bill wasn’t too happy to meld my version into what we were writing; so I suggested publishing it (as part of joint work) as a section in my article on non-commutative conditional expectations in the Baltimore, von Neumann-Stone conference Proceedings Bob Doran and I were editing. It was clear that Bill was relieved and happy about that. At one point, Bill mentioned what we were doing to some young guys and the word got around. Soon, a number of people were clamoring to see what we had. Bill apologized to me for letting it slip out without asking me – of course, he didn’t have to apologize.

He suggested posting it on the internet as work in progress and I agreed. It generated quite a bit of interest. In particular, Palle Jorgensen, Dave Larson, and some others wanted it very badly (as is) for a “fancy” GPOTS volume they were getting together (just appeared – Cont. Math. 414). They’re good friends and we had attended that GPOTS meeting, so we agreed. Our article also contained Horn (the Schur converse) for trace class operators. That’s not just a limiting extension of the finite case. The technique I had found for filling out a projection matrix given a diagonal didn’t converge as you went infinite-dimensional. In addition the Schur inequalities kick in, in the general case, and have to be dealt with for the converse in this general L_1 case. I had done this in the projection case, which means finite-rank projection, but an infinite diagonal. As I noted, I didn’t really have the Schur inequalities to fight with (just had to make sure that what is on the infinite diagonal lies in $[0, 1]$ and that its sum is the rank of the projection).



FIGURE 5. Visible faces of Bill Arveson and Henry Dye in profile, at a conference at the University of Iowa in 1985 (see [JM87]) (source: Palle Jorgensen)

At that point, I went to infinite-rank projections; I was on a roll. It was “clear” that you could put anything in $[0, 1]$ on the diagonal that summed to infinity and my algorithm, slightly extended, would prove it. I was about to dust off my hands, wrap it up, publish it and walk away from the whole project. Fortunately, I realized that something was

wrong (after a week or so). If E is a projection, so is $I - E$. If a_1, a_2, \dots is the diagonal for the matrix of E , then $1 - a_1, 1 - a_2, \dots$ is the diagonal for $I - E$. Suppose $I - E$ is finite rank. Then $1 - a_1, 1 - a_2, \dots$ must sum to that rank. Of course, there is a sequence a_1, a_2, \dots in $[0, 1]$ that sums to infinity, while $1 - a_1, 1 - a_2, \dots$ sums to a real number that isn't an integer (e.g. $a_j = 1 - j^{-2}$). But I had proved that there was a projection without that integrality obstruction included; I couldn't find the mistake in the proof! For a month or more, I was walking around responding to people who asked me what I was doing, that I was trying to find out why the world isn't flat! (I may even have said that to you at some point during a phone conversation.) Finally, I realized that my algorithm didn't yield a convergent process at each matrix entry. I had been doing a "small" matrix computation mentally (dangerous!) and making the same mistake (neglecting a term) several times.

With integrality as an added condition, that infinite case with finite rank complement is a trivial consequence of the finite-rank result (on infinite-dimensional space – that is, with infinite diagonal) that I'd proved – but I'd made the same mistake in proving that. Back to the drawing board to repair that (not too easy). What about the case of an infinite-rank projection with infinite-rank complement (so, $a_1 + a_2 + \dots = \infty = 1 - a_1 + 1 - a_2 + \dots$). Now, surely, there was no integrality obstruction. Wrong! The final result is the following:

Let a_1, a_2, \dots be a sequence of numbers in $[0, 1]$ such that $\{a_j\}$ and $\{1 - a_j\}$ sum to ∞ . Let $\{a'_j\}$ be the set of those a_j that exceed $\frac{1}{2}$ and $\{a''_j\}$ be the set of those a_j that do not exceed $\frac{1}{2}$. Let a be the sum of the a''_j and b be the sum of the a'_j . If either a or b is ∞ , then a_1, a_2, \dots is the diagonal of a projection of infinite rank with complement of infinite rank. If both a and b are finite, then a_1, a_2, \dots is the diagonal of such a projection if and only if $a - b$ is an integer.

What does $\frac{1}{2}$ have to do with anything? Nothing! If a and b are finite and we use $\frac{1}{3}$ instead of $\frac{1}{2}$, then an a_j between $\frac{1}{3}$ and $\frac{1}{2}$ enters the difference $a - b$ with the prime and double prime interchanged and either acquires or drops a 1. In any event, the change in $a - b$ is the number of 1s involved. So, while $a - b$ changes value, it stays integral or non-integral, as the case may be. I worked long and hard to prove this, partly because I didn't realize the integrality condition until

the very end. That is Theorem 15 in my Pythagoras II paper [see [Kad02b] –Ed.].

Bill was very impressed by that result. It was the one thing I had done that was way out of the range of any of the earlier work (Horn, our own trace-class stuff, Kostant, etc.). So, we wanted to push on to the general spectrum case without the summability restriction (as in the case of my infinite-rank projection with infinite-rank complement). We met out in Berkeley at one point and spent a working day together. I suggested something to Bill that I had been thinking about, but hadn't had a chance to work on (all the chores and distractions!), shortly after Bill drew my attention to Horn's work. I had begun to suspect that the difference of the spectral values generate a subgroup of the (additive) group of reals that must play the role of the subgroup of integers in my projection case, and that the sequences that might be diagonals had to "cluster" around the points of the spectrum. The invariant would play a role when their differences from those spectral points could be summed to a finite number (as with my a and b). I hadn't given it real thought and certainly wasn't thinking of expanding the study to complex spectrum (normal operators). So, it was all quite loose, vague, and muddled in my mind. Nevertheless, I decided to tell it to Bill and did so over lunch. Bill clearly caught on and began to think about it seriously and to get somewhere (in contrast to me). Meanwhile, I continued in my mental, dream world, not responding, for long periods, to Bill's occasional "position" papers on the topic after I received them and, then, only after a few minutes thought. Bill had formulated the normal case precisely, cleaning up my wild, disorganized suggestions and producing a piece of genuine mathematics. He decided that the case where the finite complex spectrum X forms the vertices of a convex polygon in \mathbb{R}^2 was the right case to tackle first. Each point in X has infinite multiplicity (corresponding to the 0 and 1 in the crucial case of my projections). Each point on the diagonal has to lie in the convex polygon (corresponding to my $[0, 1]$ in the projection case, but as in this case, that's not sufficient). If a_1, a_2, \dots is the proposed diagonal for some operator with spectrum X (each point having infinite multiplicity), then he sets up the clustering around the points of X and restricts to the case of the distances from the points summing (which is the tough case of a and b finite). He forms the "obstruction" quotient space and shows that the obstruction must

be 0 for the diagonals of normal operators with spectrum X , and diagonal clustering around points of X with the differences summing. He has an example to show that those conditions don't guarantee being a diagonal (in this infinite case) even when X consists of just three points. To do the job in the two-point case, Bill uses a simple transformation to take the two points to 0 and 1, respectively, and then cites my "Pythagoras" result.

As I said in my e-mail review, it looks like a very nice article to me."

At this point, we've come full circle; we are back to Bill's 1967 Acta paper [Arv67]. In Section 4.3 of that paper, Bill speaks of "determinants" in a von Neumann algebra setting. He cites the "determinant" that Bent Fuglede and I introduced [FK51, FK52] and used to answer a question we had asked ourselves: Must a generalized nilpotent operator in a II_1 factor have trace 0? We proved, using that determinant that the trace of each operator lies in the closed convex hull of the spectrum of the operator, which, of course, provides a positive answer to the question about generalized nilpotents. Bill notes that our determinant applies to more general circumstances than the II_1 factor case without the least difficulty; statements, definitions, and proofs, remain virtually unchanged. One has, for example, the extension to general, finite von Neumann algebras, where a fixed tracial state is used in place of the trace itself in the factor. Using this determinant, Bill states and proves a generalization of the Hadamard determinant inequalities [Had93], mentioned in the report to Is Singer. It involves the tracial state and a conditional expectation that "lifts" it. This occurs in Section 4.3. In Section 4.4, the final section of Chapter 4, Bill formulates an extension of Jensen's inequality in terms of Bill's "subdiagonal algebras," the determinant, a conditional expectation onto the "diagonal" of the subdiagonal algebra, and a tracial state that lifts it. In some inexplicable way, my browsing in Bill's paper, back near the time it first appeared, had been, at most, superficial. Any memory of those later sections with our determinant had completely disappeared by the time Bill and I were thinking about the extension of the Carpenter's theorem. On the other hand, I had re-examined Schur's paper, which Schur had dedicated, with the greatest (and certainly, all due) respect to Hadamard and the Hadamard inequality. It seemed worthwhile to get hold of Hadamard's paper and examine it. I did so; it was interesting — just a few pages long — but, as nothing compared to Schur's paper in richness of results, depth, and scope. I had just seen how to use conditional expectation techniques to extend and prove the Schur inequalities for finite matrices to certain infinite-dimensional situations (*viz.* trace-class operators on Hilbert space and self-adjoint operators in factors of type II_1). (See [AK06] and [Kad04, Section 5]). Bill and I had spoken about this and his version of the same, as described earlier. At the same time, I had just been learning

about the Hadamard determinant inequality — either totally oblivious to all the fine work Bill had done with this inequality in his Acta article, or having completely forgotten glancing at it as I browsed in that article many years earlier. So, I began to think about proving an infinite-dimensional, Hadamard, determinant inequality in a factor of type II_1 , in terms of our determinant in a II_1 factor introduced some fifty years earlier, with my conditional expectation techniques. I stopped almost at once, after getting a rough, unwritten idea of how I might proceed, when the thought occurred to me that it would be great fun to do this with Bent. And how apt that would be; two young fellows, twenty-five years old, start this line of work and return to it, as old men, close to eighty, to prove another inequality with these tools. I phoned Bent shortly after that thought. I recall his response, when we had straightened out what the Hadamard result said. He conjectured that since I was proposing that we work on this, I may have a proof in mind. I answered that I had a rough idea along certain lines, but I hadn't tried to "push it through." We agreed to think about it and get back in touch. Two weeks later, I received a handwritten letter from Bent with a lovely, "primitive" proof of the inequality we wanted in a finite factor. (It may seem a little curious to conjoin 'lovely' and 'primitive' in a description of a proof, but that feels accurate to me.) I had begun to add my own ideas to what Bent had done, after a pause to complete some other work, when Bill and I contacted one another, by phone, to talk about our joint project. I mentioned what Bent and I were doing with the Hadamard inequality to Bill. He must have been shocked that I was totally unaware of the fact that this inequality was a key element in his Acta paper, but there was no "explosion" (of rage, disbelief, or anything). What I heard was, rather, a soft response, almost whispered, suggesting that I might want to take a look at Chapter 4 in his Acta paper. I did that and generated my own stunned disbelief at what my much-admired, younger friend had done with things "close to my heart" — and making crucial use of my work with Bent Fuglede, and my work with Is Singer [KS59, KS60], at that! What a good sport Bill was. That incident prompted my earlier remark, "I browsed somewhat carefully but not as carefully as I should have."

I phoned Bent shortly after that "illuminating" conversation with Bill, and told him of Bill's work with "our" question. Bent and I agreed not to try to analyze the connection of Bill's work to ours until we were together again. That meeting occurred not much later in Copenhagen in Bent's office. We had examined Section 4.3 of Bill's Acta paper but could not clarify the connection between what Bill had done and what we had done. Bill's article is not something that permits complete comprehension when it is entered randomly. It's an "organic whole" and requires being studied as such. In writing this "remembrance," I have taken the time to have that closer look at the last sections of Bill's Acta paper (the look I should have had forty-five years earlier). I now understand the connection between Bill's extension of

our determinant in a II_1 factor and what Bent and I did in that regard. Bill's point is to extend that determinant, with no essential changes, to a form in which he can use it for *important* purposes. Bill certainly does not "fuss" about that extension (and would have been embarrassed, not "gratified," by anyone who did so on his behalf). He uses that theory with his extension to formulate the generalized Hadamard inequality, which he proves with roughly the same conditional-expectation techniques I had in mind (forty years later!). Very likely, Bent and I will make our thoughts on the Hadamard result and some extensions available in published form at some not too distant future date. Bill will be very much in our thoughts at that time. It's not too daring to predict that he will be in the thoughts of many mathematicians for many years to come.



FIGURE 6. Bill Arveson and some of his students at COSy 2006. From the left: Marcelo Laca, Ilan Hirshberg, Michael Lamoureux, Kenneth R. Davidson and Donal P. O'Donovan (source: Marcelo Laca and Juliana Erlijman)

Marcelo Laca⁵

It is difficult for me to imagine the world without Bill Arveson, mathematician, mentor and friend. The recognition that he left behind an extremely rich mathematical legacy, both in print and in the minds of those he inspired, is only a partial consolation. He also left behind a wonderful network of friends and colleagues who will remember him fondly and miss him sorely. I had the privilege of doing my PhD with him, in a period that shaped the rest of my life, and I would like to take this opportunity to reminisce and give a glimpse of what it was like to have Bill as an advisor, and also to share some of his advice. Other aspects of Bill's professional life and his many contributions to mathematics are described elsewhere by others.

The 1980's were excellent years for functional analysis and operator algebras at Berkeley, and it was in the spring of 1984 that I first met Bill. At the time I was a graduate student in statistics, but old habits die hard and I had decided to take a course in operator algebras. The very first day Bill outlined the course for us, it was going to be an introduction to C^* -algebras and their K -theory. He then mentioned in passing that we could settle on A's for everyone who was still there at the end of the semester, unless we preferred actual grades, which *might* include some A^+ 's, or worse. Somewhat incredulously, we took him up on the safe bet by overwhelming (but not unanimous) vote. The irreverent way of settling that detail and the direct dive into the matter of the course that followed left a lasting impression on me. Later I was to find out that 'being there at the end' would not be as easy as it sounded, and that Bill's real assessment of performance and potential would be distilled in a few crucial words to be appended to student's files.

After taking a couple of his courses and hearing him speak on his research at seminars, I made up my mind that I wanted to work with him. His mathematics, his style and his personality were so compelling that it was an unavoidable choice. Never mind it was a long shot. At the time Bill had a large number of graduate students and rumour had it that he was not taking any more. But some of them graduated just in time and I got lucky. I instantly felt I was part of a society, or rather, of a family. In the mid eighties the family included Belisario Ventura, David Pitts, Jack Spielberg, Jack Shaio, Richard Baker, Michael Lamoureux, Chikaung Pai, Hung Dinh and myself. It was a great privilege to work with Bill, and, even better, it was fun, great fun. Among several of us, he had the status of rock star or movie star, somewhere between a shining Elvis Presley and a tough John Wayne. The simple fact was that Bill was amazing, as a mathematician and as a person. And he was cool too. He even wore a black leather jacket, which prompted several of us to follow suit. The rumours about his past

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were plenty and colourful: racing dragsters down the streets of LA, chased by police, or being thrown out of Las Vegas casinos for a little too much success at blackjack. And the list went on. More revealing stories kept surfacing as time went by (many told by Bill himself -he was definitely not one to take himself pompously), adding to the legend.

Fun as it was, studying with Bill was serious business. With the benefit of experience I understand even less now than then how Bill managed to keep us all moving along our separate tracks, yet close enough to be highly interactive with each other. With up to six students at a time, this was no small feat. We all met and gave talks at Bill's weekly seminar, usually following one or two major topics in a semester. Bill would meet with each of us separately for an hour or more every two weeks, to talk about our respective projects. At the beginning I often had nothing, not even questions, so Bill would try to get me going again, or he would simply launch into a fascinating impromptu lecture on whatever was in his mind at the time. It could be a dilation principle viewed across different categories, the principles of nonlinear filtering, ideas on noncommutative scattering, the construction of E_0 -semigroups or a number of other topics. The range was astonishing. Invariably, I came out of our meetings upbeat, feeling privileged for the window to Bill's deep insight on a wealth of ideas and also for his unbounded enthusiasm and encouragement. Initially, because of my background, I chose to work on Osterwalder-Schrader positivity, and its relation to Markov processes. After some time and effort I came up with a disappointing answer to the key question. Bill showed some interest in the example that trivialized the question, but acknowledged that it had not been such a great problem after all. I know now that many advisors would show more than a bit of concern seeing a PhD problem come to such a dead end. Not Bill. True to his trademark, not only did he not have any regrets, he was not even taken aback. I got the idea that things like that sometimes happened and moved along.

At the time Bill was deeply into his effort to sort out the index theory for E_0 -semigroups via product systems, and in the weekly seminar several of us had been going through Bob Powers' seminal papers on E_0 -semigroups. After a while I felt I had a shot at something related to semigroups of endomorphisms for my thesis so I inquired about changing topics. Bill agreed, but said to get some results first and only then announce the change. Once I asked what to read for background; his answer was a dry "We'll worry about that if you start spinning your wheels. If you read too much you'll never write anything." He obviously knew the kind of background I would need, and pointed me in the direction of the Powers-Størmer inequality. But even then, he did it by asking me to present some of their results in the seminar, in relation to something else, or at least so I thought. Later I was to prove a modified inequality that was quite useful in classifying certain endomorphisms of $B(H)$ up to conjugacy.

The semester I was supposed to graduate Bill was planning to be on leave at the Mittag-Leffler Institute, so he said to give him my manuscript before he left. I took this literally so the day before he left I gave him my neatly *handwritten* notes, together with the typeset title page. I remember him looking at the notes, then at me, then the title page, then the notes again. A few weeks later I got the notes carefully annotated in red *and* the signed title page. He did eventually comment that I should not always take words so literally.

Bill had a gift for being genuinely friendly and personable while keeping his distance in a smooth but firm way. Initially I did not quite know what to make of this, but then I appreciated it and often wished I had more of that capacity myself. After the seminar we all used to go out for pizza and beer, usually to La Val's where he would insist on buying the first couple of pitchers. Bill had many foreign students over the years, and he and Lee were honour guests at our 'international' potluck dinners. He was gregarious and easygoing, and was always keen to learn more about people, customs, food and other things foreign. But his larger-than-life mathematical persona still loomed over me, ominously, even many years after graduation, and I suppose I am not alone at that. He knew this, of course, and sometimes got a kick out of playing the role. I will never forget a rather jolting email I got from Bill a few years after I left Berkeley. It came out of the blue but was so much in time and to the point that I got the impression he was actually watching me. The message said that it was time to stop the fooling around and to get back to work proving theorems. What really got me was the signature: "*Bill (the voice of your conscience) Arveson*". Needless to say, his "voice of my conscience" still resounds in my ears.

I remember asking once about administrative duties. He pointed at a pile of papers close to the edge of his desk: "Look at that pile. It keeps growing, but at some point it will go over the edge and into the basket. Less than 5% of it is important. It will come back." As editor, he declined to take a paper of mine, adding with a grin "it would look like greasing the skids for my former student, and we wouldn't want *that*".

I am convinced that when Bill did mathematics, he just thought differently from everyone else I know; his was a very intimate thought process that was not complete and could not be shared until he had the perfect way of presenting the big picture, frame and all. Many of his papers became hugely influential in the field, and for all I know, many others are just awaiting re-discovery to achieve similar fate, for Bill's ideas are deep and timeless. His work is all the more impressive considering that he worked almost exclusively by himself, and that he only published what met his high standard. His research touched upon many areas. One common thread was that he preferred to deal with challenging problems, another was the principle that to be properly understood, problems should be put in operator algebraic terms. Only once or twice I got the feeling that something I was

saying was news to him but, in any case, the interval between “that cannot be true” and “I see” was never long enough to enjoy. He was very generous with credit and with his ideas. Embedded in his explanations there were often priceless jewels of his original insight, which he simply gave away as part of his approach to the subject. These keep cropping up once and again in my mathematical life, evoking no small amount of admiration, gratitude, and nostalgia.



FIGURE 7. From the left: Daniel Kastler, Marcelo Laca, Paul Muhly, Hans Borchers and Bill Arveson at a conference at the University of Iowa in 1985. (Source: Palle Jorgensen)

Paul S. Muhly⁶

I had the wonderful good fortune to spend the 1977-78 academic year on sabbatical at the University of California at Berkeley. It was an extraordinarily stimulating experience, but my most vivid memories are from the times I spent talking with Bill Arveson. Among the many things we discussed were his papers, *Subalgebras of C^* -algebras I & II* [Arv69, Arv72]. I was already very familiar with them. Indeed, I had spent a lot of time studying them. I found them full of inspiration and, after more than 40 years, I still do.

So I was taken aback, early in our discussions, when Bill expressed disappointment that *Subalgebras I* had not received more recognition. It was Bill’s most heavily cited paper and it continues to be number 1, with almost

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100 more citations on MathSciNet than the runner up. One might think, therefore, that Bill was being greedy. He was not, and I would like to take this opportunity to explain why.

Bill wrote [Arv69, Arv72] in order to set the stage for studying general, not-necessarily-self-adjoint operator algebras. He drew inspiration from several sources. First there was the seminal work of Kadison and Singer [KS59]. This was the first paper dedicated to studying non-self-adjoint operator algebras. Their objective had been to classify algebras of operators which are infinite dimensional analogues of the algebra of upper triangular $n \times n$ matrices. Bill also gained inspiration from the dilation theory that was due in large part to Sz.-Nagy [SN53]. Owing to the contributions of many function theoretically oriented functional analysts, dilation theory had grown into a model theory for arbitrary operators on Hilbert space. And he was inspired by developments in the theory of function algebras. This theory had arisen, in large part, to provide a functional analytic treatment of spaces of analytic functions that arise in harmonic analysis and in approximation theory. There were already very close ties between the theory of function algebras and the model theory stemming from the dilation theory of Sz.-Nagy.

For Bill, an operator algebra was simply a norm-closed subalgebra of the algebra of operators on Hilbert space. His Hilbert spaces were always complex and almost always separable. He also assumed his algebras contained the identity operator. Thus he was set up to study a category whose *objects* are general norm-closed unital operator algebras. But what are the *morphisms* in this category? The answer is not so clear. At least it wasn't at the time Bill started his study. What Bill settled upon was the notion of a *completely bounded* algebra homomorphism. The point is that if A is a subalgebra of $B(H)$, the algebra of bounded linear operators on a Hilbert space H , then not only does A have a norm structure, but for each n , the $n \times n$ matrices over A , $M_n(A)$, carries an intimately related norm structure arising from the natural $*$ -isomorphism between $M_n(B(H))$ and $B(H \oplus H \oplus \dots \oplus H)$. If $\varphi : A \rightarrow B$ is a homomorphism between two operator algebras, then one may promote φ in the obvious way to a homomorphism $\varphi_n : M_n(A) \rightarrow M_n(B) : \varphi_n((a_{ij})) := (\varphi(a_{ij}))$. One says that φ is *completely bounded* in case each φ_n is bounded and $\sup_n \|\varphi_n\| < \infty$. This supremum is called the *cb-norm* of φ , $\|\varphi\|_{cb}$. There are, of course, variations on this notion. Thus, for example, φ is called *completely contractive* if $\|\varphi\|_{cb} \leq 1$ and φ is called *completely isometric* if each φ_n is isometric.

Bill was led to the notion of a completely bounded homomorphism by the little note of Stinespring [Sti55]. Stinespring, in turn, was inspired by Naimark's theorem about dilating positive operator-valued measures to spectral measures. Stinespring's theorem asserts that if B is a unital C^* -algebra and if $\varphi : B \rightarrow B(H)$ is a linear map, then there is a C^* -representation $\pi : B \rightarrow B(K)$, for some, possibly different, Hilbert space K and a bounded

linear map V from K to H such that

$$\varphi(b) = V^* \pi(b) V$$

for all $b \in B$ if and only if φ is completely positive in the sense that each $\varphi_n : M_n(B) \rightarrow M_n(B(H))$ is positive for every n . Although this theorem received some notice in the literature between 1955, when it appeared, and the appearance of [Arv69], the literature about completely positive maps, and their relatives defined by Bill, exploded afterwards and grew, ultimately, into the subject now known as the theory of operator spaces.

Bill modestly asserts at the outset of [Arv69]: “In a broad sense, the objective of this paper is to call attention to certain relations that exist between non self-adjoint operator algebras on Hilbert space and the C^* -algebras they generate.” His goal was to show that each operator algebra A has a naturally associated C^* -algebra, $C^*(A)$, that contains it and is invariant under complete isometric isomorphisms in a very strong sense. Further, he wanted to show that the completely contractive representations of A can be expressed in terms of C^* -representations of $C^*(A)$. One’s first reaction to these assertions might be to ask: What’s wrong with simply taking the C^* -subalgebra of $B(H)$ generated by A , where H is the Hilbert space on which A lives? One answer is that A can be represented in a completely isometric fashion on Hilbert space in *lots* of different ways and it is not at all clear how the different representations affect the theory. To put the same thing somewhat more emphatically: When one defines an operator algebra as a subalgebra of $B(H)$, one gets more than an operator algebra, one has an operator algebra *plus a preferred module*. The problem is to separate the intrinsic properties of the algebra from those that are artifacts of the way in which it acts on the Hilbert space.

Bill’s solution to this problem involved the notion of a boundary representation. Suppose B is a C^* -algebra and suppose B is generated by a copy of the operator algebra A . A bit more formally and completely, suppose $\varphi : A \rightarrow B$ is a unital, completely isometric homomorphism from A into B whose image generates B . An irreducible representation π of B on the Hilbert space H_π is called a *boundary representation of B for $\varphi(A)$* in case the only completely positive map from B to $B(H_\pi)$ that agrees with π on $\varphi(A)$ is π .

As an illustrative example, let \mathbb{D} denote the open unit disc in the complex plane and let $A(\mathbb{D})$ denote the *disc algebra*, i.e., $A(\mathbb{D})$ is the space of all continuous functions on $\overline{\mathbb{D}}$ that are analytic on \mathbb{D} . Then $A(\mathbb{D})$ is a subalgebra of the C^* -algebra $C(\overline{\mathbb{D}})$. The irreducible representations of $C(\overline{\mathbb{D}})$ are given by all the evaluations at the points of $\overline{\mathbb{D}}$. The ones that are boundary representations for $A(\mathbb{D})$ are the ones coming from the points on the topological boundary of \mathbb{D} , namely the unit circle.

Bill called $\varphi(A)$ an *admissible* subalgebra of the C^* -algebra B in case the intersection of the kernels of the boundary representations of B for $\varphi(A)$

is a boundary ideal in B in the following sense: A two sided ideal J of B is a *boundary ideal* for $\varphi(A)$ in case the restriction of the quotient map $q : B \rightarrow B/J$ to $\varphi(A)$ maps $\varphi(A)$ completely isometrically into B/J . In the example of the disc algebra, every open subset U of $\overline{\mathbb{D}}$ determines an ideal in $C(\overline{\mathbb{D}})$, namely the space of all functions that vanish on the complement of U , $C_0(U)$. By the maximum modulus theorem, $C_0(U)$ is a boundary ideal for $A(\mathbb{D})$ if and only if U contains no points of the unit circle. Bill proved that when $\varphi(A)$ is admissible, then the intersection of the kernels of the boundary representations of B for $\varphi(A)$ is the largest boundary ideal in B for $\varphi(A)$. He called this ideal the *Shilov boundary ideal* for $\varphi(A)$.

Bill proved that the quotient of B by the Shilov boundary ideal for $\varphi(A)$ is unique in this sense: Suppose that for $i = 1, 2$ $\varphi_i : A \rightarrow B_i$ is a completely isometric homomorphic embedding of A into a C^* -algebra B_i in such a way that B_i is generated as a C^* -algebra by $\varphi_i(A)$ and suppose that the Shilov boundary ideal of B_i for $\varphi_i(A)$ vanishes. Then there is a C^* -isomorphism $\pi : B_1 \rightarrow B_2$ with the property that $\varphi_2 = \pi \circ \varphi_1$. Thus, there is an essentially unique C^* -algebra containing a completely isometric copy of A such that the Shilov boundary ideal vanishes. This is the C^* -algebra Bill wanted to associate to A and he called it the C^* -envelope of A , denoting it $C^*(A)$. $C^*(A)$ codifies the operator algebra structure of A , i.e., its complete isometric isomorphism class, and it does so without reference to any Hilbert space on which A might act. Further, Bill showed how the completely contractive representations of A could be studied in terms of the C^* -representations of $C^*(A)$ in a fashion that naturally generalizes Sz.-Nagy's dilation theorem.

There was a problem, however. It was the source of Bill's disappointment. He really had no way to decide when $\varphi(A)$ is an admissible subalgebra of B . More accurately, he had no universal way to ensure that there were any boundary representations at all! Some might argue that in a sense, his theory was stillborn. To be sure, *Subalgebras of C^* -algebras I & II* attracted a lot of attention, but not really for the reasons he had hoped. Bill told me that he had put the subject matter of those papers aside and gone on to other things.

That was in 1977. Then, in 1979, Masamichi Hamana made an important discovery: He showed that the C^* -envelope of an operator algebra always exists [Ham79]. He did so, however, without showing that any boundary representations exist. Rather, he showed that there always is a maximal boundary ideal. As Bill had shown, when the maximal boundary ideal is zero, the C^* -algebra has the uniqueness property described above. However, without the boundary representations one is missing a lot of the fine structure of $C^*(A)$ that Bill had anticipated. I asked Bill for his reaction to Hamana's paper. He was very gracious and complimentary, but he felt nevertheless, that Hamana's was the "wrong" solution to existence of $C^*(A)$: The existence of boundary representations remained a big problem. Bill was well into other things at that point, and one might believe that he had

given up entirely on this existence problem. So it seemed – at least for another 28 years or so. In February of 2007, however, he posted [Arv08] on the arXiv. It was written in Bill’s characteristic, low-key, matter-of-fact style. Nevertheless, as I read it and reflected back on our conversations in Berkeley, his exultation was palpable. He had settled the existence problem. Indeed, he showed that every norm-separable operator algebra is an admissible subalgebra of any C^* -algebra that contains it.

I was delighted that Bill solved the problem and that finally, the central thesis of [Arv69] had been fully vindicated. Since the appearance of [Arv08], there has been an uptick in the interest in [Arv69], and I suspect, indeed, I fervently hope that the program that Bill initiated in it will flourish for years to come. In addition to being the source of great mathematics, “Subalgebras of C^* -algebras” will serve as a monument to Bill’s unswerving perseverance, from which we may all draw inspiration.



FIGURE 8. Bill Arveson in New York (source: Lee Kaskutas)

David R. Pitts⁷

Most chance meetings are of little consequence. But a few are life-changing. I first met William Arveson in a laundromat in Berkeley in the early 1980’s, and in the ensuing conversation, I learned he was a mathematics professor, and he learned I was a mathematics graduate student. The circumstances amused us both. Shortly afterward, I remember thinking that would be a remarkable way to meet a thesis advisor. To my great fortune, Bill became my Ph.D. supervisor a year or so later.

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As readers of Arveson's work know, Bill had an extraordinary ability to expose mathematics clearly and efficiently. I saw the process first-hand while a graduate student. I remember once asking Bill a question regarding the Weyl-von Neumann-Berg Theorem. Bill's response was, "Let me think." A few moments later, he began to write. What flowed from his pen were several paragraphs clearly outlining the key ideas for the proof. This was far from unique: in my file cabinet are several of Bill's extemporaneous writings, which I still find useful.

During other conversations, Bill taught that a good way to do mathematics is to find some interesting literature, and then "seek to understand it *very deeply*." Bill was masterful at this. I gained some sense of what he meant by reading a portion of the book, "Theory and applications of Volterra operators in Hilbert space," by I. C. Gohberg and M. G. Krein. In the book, the authors give a characterization of the bounded invertible operators T on a Hilbert space \mathcal{H} which are universally factorizable along every nest \mathfrak{P} of projections on \mathcal{H} in the following sense: given a nest \mathfrak{P} , there exists a bounded invertible operator A on \mathcal{H} such that A and A^{-1} leave invariant every projection $P \in \mathfrak{P}$ and $T^*T = A^*A$. I thought I understood the proof, so I showed what I'd learned to Bill. His simple response left me bewildered. He said, "I don't understand. Think about it more and show me again later." This occurred several times. At last, I observed that it is possible to modify the LDU decomposition of a positive invertible matrix $X \in M_n(\mathbb{C})$ to obtain a formula for U which is an idempotent linear operator on $M_n(\mathbb{C})$; a modification of the formula yields a tool for factoring certain operators relative to a nest. This time, when I showed what I found to Bill, his words were, "Ah, now I understand."

Arveson once told me that he published "when I have something to say." It wasn't until after completing my graduate studies that I began to appreciate the remarkable scope and impact of Arveson's work. I took Bill's advice and went to as many conferences as I could. At these meetings, I'd hear Arveson's name attached to an astonishing number of deep and pioneering results, some related to, but many others far removed, from what I'd studied as a graduate student. Bill truly had a lot to say!

I, along with many others, have benefited much from Bill's mathematics, mathematical leadership, guidance, and generosity. He is greatly missed.

Robert T. Powers⁸

I have known Bill Arveson all of my mathematical life as I first met him at the large Baton Rouge Conference in March of 1967 while I was still a graduate student in Physics, a student of Arthur Wightman working in quantum field theory. I remember his enthusiasm as we talked of factors, von Neumann algebras with a trivial center. At that time I was under the

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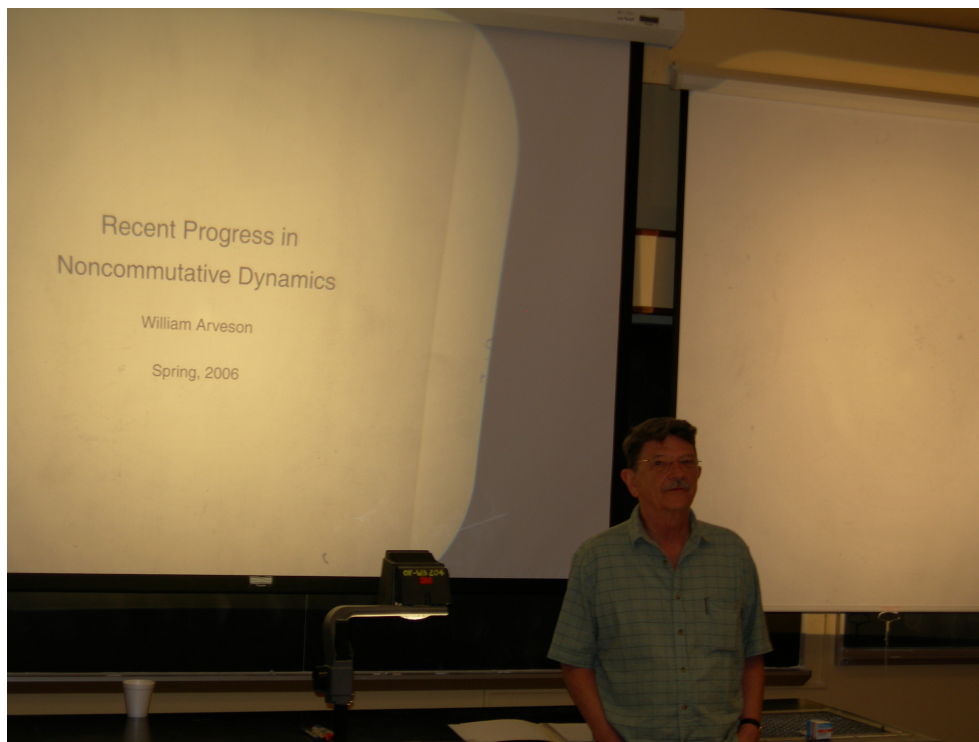


FIGURE 9. Bill Arveson at COSy 2006 (source: Marcelo Laca and Juliana Erlijman)

illusion that problems of quantum field theory would be settled by applying the techniques developing in C^* -algebras and von Neumann algebras. Over the years we saw each other many times at Berkeley, Philadelphia and at conferences all over the world.

I should say at the start that I do not enjoy reading other people's papers. I often spend weeks trying to prove a result rather than looking it up and I tend to ignore work that does not have a direct bearing on what I am currently working on. For that reason I am not qualified to assess the impact of Bill's work on mathematics. But as much as I avoided reading other people's papers I could not avoid reading many of Bill's papers which I not only read but studied them to the point that Bill's ideas became incorporated in my own research. I was frankly jealous of one of his earlier papers on one parameter automorphism groups that can be implemented by unitary group with positive spectrum, an idea from Physics expounded in an early paper by Hans Borchers, in that I was well aware of the ideas leading up to it but kicked myself for not seeing Bill's brilliant ideas for turning these ideas into gold.

Around 1986 I began studying E_0 -semigroups of $B(H)$, one parameter semigroups of $*$ -endomorphism of $B(H)$. Soon I lost my NSF support partly

because my work was “uninteresting” but a few years later Bill came to my rescue. He developed the theory of product systems and proved the great result that each product system is associated with an E_0 -semigroup of $B(H)$. He later wrote a book, “Noncommutative Dynamics and E-Semigroups,” Springer-Verlag (2003). Besides proving a number of pivotal results and providing a mathematical framework for the study of E-semigroups, Bill’s work made the subject accessible so that other mathematicians such as Boris Tsirelson, working in Markov processes, could provide new examples of E_0 -semigroups. Bill and I never published together. We agreed it would be more likely for us to be consulted as an impartial referee regarding each other’s work but I have published repeatedly with two of his students, Alexis Alevras and Daniel Markiewicz. Since 1990 Bill and I met on almost a yearly basis to discuss ideas. One of my papers was simply a reply to a question of Bill’s. At a small informal gathering at the U. S. Naval Academy with Geoffrey Price and Alexis Alevras, Bill presented an example of a semigroup of completely positive contractions of the two by two matrices and wondered what E_0 -semigroups they produced by Bhat’s dilation result. The race was on and I went back to Penn and Bill to Berkeley and we produced the same result (only Bill’s was more general, his for $B(H)$ and mine for $B(H)$ with H finite dimensional) and the papers were published side by side in the International Journal of Mathematics.

Intellectually I know Bill died, but I still don’t believe it. I know next Spring I will think about visiting Bill and Lee in Berkeley or look forward to hearing that laugh of his regarding some recent development till I remember the hole that he has left. I only interacted with Bill in a fraction of his mathematical work and I am sure others can tell similar stories about his significant influence in different areas of mathematics.

Geoffrey L. Price⁹

In the late 1970s it was my good fortune to be a graduate student at the University of Pennsylvania, where Dick Kadison had assembled a stellar cast of operator algebraists, including Bob Powers, my thesis advisor, and where Bill and other big names in operator algebras would come to spend their sabbatical year. Although I was too shy to speak with him it was clear from a distance that Bill was a different sort of mathematician altogether. He was the operator algebraist’s answer to Jack Kerouac, or Belmondo, complete with great hair, bomber jacket, sneakers, cool demeanor and cigarette always in hand. He had a style of lecturing in the Tuesday functional analysis seminars that was more conversation than lecture, and the ease with which he brought so many ideas to bear in his presentations was breathtaking and more than a little intimidating to a graduate student.

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FIGURE 10. Bill Arveson at Mendocino, circa 1986 (source: Lee Kaskutas)

I did not have an opportunity to see Bill again until the late 80s, when he became interested in the work that Bob Powers was doing on E_0 -semigroups. E_0 -semigroups can be thought of as dynamical systems which can evolve in one time direction only. As he has often mentioned in his talks, Bob's first thought about E_0 -semigroups was that he could probably knock off their classification in an afternoon, but he and others have been working on them ever since. Bill also got hooked on this subject. Part of what was so exciting was that Bill's approach differed so significantly from Bob's. The Powers approach used the machinery of unbounded derivations, whereas Bill noticed that it would be useful to think about E_0 -semigroups using continuous tensor products of Hilbert spaces. In his AMS Memoirs paper, *Continuous analogues of Fock space*, Bill showed that all of the so-called completely spatial E_0 -semigroups are equivalent to the canonical flows on the CAR algebra, the most basic of all examples. Another of Bill's important contributions to the subject came soon after. In his first paper Powers introduced a notion of a numerical index for E_0 -semigroups which can be a positive integer or infinity. He was able to show that the index was subadditive under tensor products, and that it was additive for the CAR-flows. Using product systems Bill introduced his own notion of index which agreed with the Powers index for the CAR flows. Bill established that his index was actually additive under tensor products: $d_{\alpha \otimes \beta} = d_\alpha + d_\beta$.

Bill visited the Naval Academy many times, the first being in 1990 when Bob Powers held a visiting position here. The main draw for Bill was Bob, who loved to visit Annapolis during his spring breaks with his wife, Mary. At one of these gatherings Bill and Bob were really intrigued by a result of Rajarama Bhat that all continuous semigroups of completely positive unital maps on a type I factor can be dilated to an E_0 -semigroup on a larger type I factor. Arveson and Powers were both puzzled about the minimal E_0 -semigroup dilation of the simplest nontrivial CP-semigroup that one can imagine. Within days they came up independently with the answer. The minimal dilations α had to be CAR flows of index 1. Their results became the basis of a pair of papers they wrote independently and which appeared back to back in 1999. It was pleasing to know that these results had their origin in conversations that took place at the Naval Academy.

Ten years later I had the good fortune to work long distance with Bill on a couple of projects, one of which involved the behavior of infinite tensor products of CP-semigroups of the type above. It was a thrill to work with one of my mathematical heroes. Bill would write to me what he'd thought about on a given day and would close by saying that it was time for a glass of wine and, according to him, further inspiration from his Dachshund: "Last night I dreamed that Schnitzel said 'Think *symmetry* stupid!'. So I followed his advice and I think I found a more manageable invariant". Bill's wife, Lee, has given me his handwritten notes on that paper and I am happy to have them here beside me.

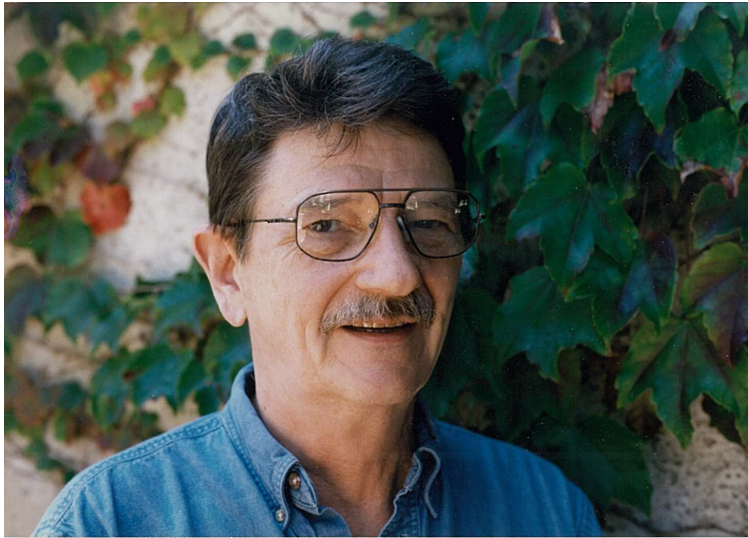


FIGURE 11. Bill Arveson in Berkeley, 1997 (source: George Bergman/Oberwolfach collection.)

Donald Sarason¹⁰

Bill Arveson and I were colleagues and friends in Berkeley for 44 years. If memory serves, I first met Bill in person in Ann Arbor when he was a Benjamin Peirce Instructor at Harvard. The occasion was one of Paul Halmos's summer operator theory get-togethers.

After Bill's Harvard position ended, he was hired by Berkeley in 1968 as a Lecturer in Mathematics, a temporary position. We were lucky he decided to come to Berkeley, because he had a couple of more substantial offers. It took some persuasion, mainly by Henry Helson, to get our colleague in charge of hiring at the time to offer Bill a position at all. Bill in his early research was focused on creating a theory of non-self-adjoint operator algebras, and our hiring czar regarded some of Bill's recommendations to be somewhat tentative. (It took a while for other operator algebra theorists to appreciate what Bill was doing.) At any rate, Bill was promoted to Associate Professor, a tenured position, in 1969. Otherwise we would have lost him, because the offers he turned down to come to Berkeley as a lecturer were still in force.

Bill and I had many common mathematical interests, but our *modus operandi* were different. My attraction was to concrete problems. Bill, in contrast, always had the global picture in view. Beyond possessing an intimate grasp of the technical aspects of his specialty, he had an uncanny insight that led him to intriguing uncharted territory, coupled with the boldness to launch an exploration.

Those who knew Bill are aware that he had a stubborn streak, a beneficial trait for anyone engaged in research. Bill's stubbornness extended beyond mathematics. As anyone of a certain age will recall, the 1960s and 1970s were tumultuous times, especially on many college campuses, including Berkeley's. One day not long after he came to Berkeley Bill entered Sproul Plaza, the main campus entrance, while a demonstration of some kind was in progress. The police were trying to clear demonstrators out, and kept telling people to move on, move on. When Bill received this order he replied "I have a perfect right to be here." He held his ground until he was suddenly seized from behind by a very large cop and hustled off to the local jail. He did not carry enough cash to post bail, but he managed to contact our chair at the time, John Addison, who got him released. I believe no charges were pressed. Bill never backed down when he thought he was in the right.

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FIGURE 12. From the left: at the back Erling Størmer, and in the front Marc Rieffel, unidentified person, Bill Arveson and Astri Rusten (source: Lee Kaskutas).

Erling Størmer¹¹

Among operator algebraists, now in their seventies or eighties, the most memorable conference they ever attended, was the one in Baton Rouge in Louisiana in 1967. Then many of us met for the first time and initiated lifelong friendships. Bill and I were no exceptions. Our friendship grew over the years, as we regularly met at conferences and their like, and culminated with my three one semester visits to Berkeley after 1998, when I enjoyed his hospitality and saw him regularly on and off campus.

While Bill was very social when he was with people, he was basically more of a loner. He worked very much by himself and mostly at home. Last time I was in Berkeley, we wrote a little paper together. It was a rather special collaboration. Our discussions mostly lasted for a few minutes when he took a little time away from his home, where his charming dachshunds were waiting for him. I remember, as the highlight, when we spent a full half hour at one of the coffee shops at Berkeley campus discussing our paper. But it was really an enjoyable and pleasant collaboration.

Much of his mathematics can be viewed from this point of view. It is often very original and based on his deep understanding of some topic, often in an abelian setting, which he extended to the non-abelian case, where they could be better understood. Thereby he initiated important new research,

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especially in C^* -algebras, where he started new directions. But he also developed a theory for non-self-adjoint operator algebras. The latter study arose from his interest in single operator theory and in particular in the theory of non-normal operators. He also considered the problem of computing spectra of self-adjoint operators from a general point of view. In his own words, “numerical problems involving infinite dimensional operators require a reformulation in terms of C^* -algebras.”

Very close to his heart were completely positive maps. They are the most important and nicest positive maps, which in the finite dimensional case are sums of maps of the form $a \mapsto V^*aV$ for a given operator V . The famous Arveson’s Extension Theorem is a Hahn-Banach type theorem for such maps. He also showed how they relate to other problems, among them E_0 -semigroups, i.e. some semigroups of endomorphisms of C^* -algebras indexed by the positive reals, a subject on which he made major contributions.

Much more can be said about his huge mathematical production, for example his work related to mathematical physics, in particular on entanglement in quantum information theory. But I stop here, hoping that the above gives the reader a feeling for the width and depth of his mathematical contributions.

Masamichi Takesaki¹²

I first met Bill at the famous first international conference on operator algebras at LSU, Baton Rouge, in March of 1967. Although I was unable to speak English well at that time, he patiently discussed mathematics and other things with me. He was an instructor at Harvard at that time. Bill was exactly one year younger than me, but started his mathematical career much later than myself because of his navy service. At any rate, the mathematical bond between us started as a result of his lecture at the first Spring Quarter Functional Analysis Wednesday Seminar at UCLA arranged by our common friend, and Bill’s PhD supervisor, Henry A. Dye in the Spring of 1972. He lectured on his constructive proof of Borchers’ result on the quasi local measurability of an energy operator with positive spectrum in quantum field theory. His proof was based on his new theory of spectral analysis of a one parameter automorphism group of a von Neumann algebra, which is called today the Arveson Spectrum. His theory was surprisingly elegant and simple despite the bad behavior of an operator algebra as a Banach space on which the usual spectral analysis of operators fails miserably. After his lecture, I could not resist the urge to apply his new theory to the modular automorphism group of a von Neumann algebra. His method was so powerful that, even though Bill’s talk had been in the first week of the Spring Quarter, by late April the structure of a von Neumann algebra of type III was beginning to emerge. In May, I was already able to report

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FIGURE 13. Peter Rosenthal, Bill Arveson, James Mingo (standing), Man-Duen Choi and unidentified person at COSy 2006 (source: Marcelo Laca and Juliana Erlijman)

on the discrete decomposition of a von Neumann algebra of type III with a periodic state. The story didn't stop there, as in the Fall of 1972, I was able to prove the so-called Takesaki duality theorem on the crossed product of a von Neumann algebra by a locally compact abelian group, and hence establish the structure theorem for von Neumann algebras of type III. Thus the whole structure analysis of von Neumann algebras of type III can be seen as a consequence of Arveson's spectral analysis.

Bill's work on the spectral analysis heavily influenced Alain Connes's thesis work, which is still quoted often today. In particular, he adapted and extended Bill's work to formulate what is now known as the Connes Spectrum of an action.

Bill's life was full of unusual incidents. One I remember is the Oakland Hillside Fire in the fall of 1991 in which he lost his lovely hillside home along with his personal mathematical library. He mentioned to me that the loss of his mathematical collection was quite a blow to him. With his permission, I asked the operator algebra community around the globe to send him their own available reprints. People responded beautifully, rebuilding his math library.

Bill's working style is very unique, quite different from mine. He, somewhat like Ed Effros, likes to explore a new area of mathematics where nobody has worked before and create a new field; Bill's successful implementation of this approach has lead him to be a very influential and even seminal figure in many areas of modern analysis. Remarkably, Bill's publication rate increased significantly after his retirement. When I expressed my surprise at this to him, his reply was that life after retirement was wonderful. I assumed after my retirement and move back to my home country Japan that he would continue his surprisingly creative mathematical life, so I was very saddened to hear of his declining health and now his death. We have lost an inspirational mathematical innovator, and a wonderful friend and colleague.

Lee Ann Kaskutas¹³



FIGURE 14. Bill holding a kitten at his house on Trask Street in Oakland (source: Lee Kaskutas)

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Bill's Youth. Bill was born in Oakland. His parents divorced when he was just a year old. In his earliest years, Bill was raised by his grandmother, who had come here from England. She was the first forewoman at the Levi Strauss factory in San Francisco, so was gone during the day. They lived in a house on Trask Street, and his grandmother had a lady come in to be there when Bill got home from school. Bill would walk to and from the local public grade school with a neighbor boy, who had a miniature dachshund that Bill loved to play with. Bill deeply loved his grandmother, and as a child did not see much of his parents who had, separately, moved to southern California.

When Bill was in maybe 3rd grade, his father decided that he was being overly influenced by women, and he arranged for Bill to go to a military school. He hated the military school and after a year or so, he and a friend ran away. They went to the circus that was in town. After dark, and after they had spent all their money, and were hungry, they snuck back into the military school and were caught. The next day, his mother arrived, and Bill begged her to please take him out of the school, which she did. He returned to Trask Street to live with his grandmother, until his mother remarried at about the time Bill was high-school age.

Bill went to high school at Alhambra High in San Gabriel, in southern California. He had a part-time job in a gas station, and liked to work on cars, which he raced, illegally. He did not turn in homework at school, but got A's on his final exams, which infuriated many of his teachers. They would say to him, "Just think how much you could have learned, if you would have worked harder throughout the year." Bill would say, "But I aced the final exam, what is the problem, I obviously learned the material."

Bill's parents did not especially encourage him in academic pursuits. Bill's high school guidance counselor told him he should become a TV and radio repairman. Bill would later joke with me about how he would love to have a little talk now, with that guidance counselor!

From the Navy to CalTech. After graduating from high school, Bill joined the navy, took a placement test, and was told he could have pretty much any job he wanted to train for. He chose to study radar technology, and spent many months at Navy schools, on Treasure Island and also in Washington State. After his training, he served in the Pacific on an aircraft carrier in the "CCC," the Command and Control Center, where he was their ace repairman. When not repairing the radar equipment, he played bridge, read, and taught himself how to play the jazz saxophone. (Bill played jazz piano, too, in his younger days.)

At the end of his 3 years of naval service, Bill took another placement test at the Navy. Apparently he got the highest score that anyone had ever gotten. They asked him to stay in the Navy, and enter their training program for jet fighter pilots. He told me that, had he done that, he would



FIGURE 15. Bill Arveson in Yosemite with his beloved daschunds, Pretzel and Schnitzel (source: Lee Kaskutas).

probably have become an astronaut (and now, he joked, would be an airline pilot!). He decided instead to leave the Navy, and try to go to college.

Bill went to Pasadena City College for 2 years, then took still another test, this one to compete for the two slots that are made available each year for transfers to Cal Tech. Again, his performance on the test was a big surprise to everyone, including the math professor who had to grade the math question. It seems the professor did not expect anyone to actually be able to solve the problem; he just wanted to see how they approached it. Bill solved the problem, was admitted to Cal Tech, and majored in mathematics, of course.

I said “of course.” Did he consider other majors? He once said that he had thought briefly about becoming an engineer, and also had considered physics.

From talking with Bill over the dinner table all these years, I realized what a truly first-rate education he had gotten at Cal Tech. Not just in chemistry, physics, the hard sciences. But also in literature. He had read all the classics, Shakespeare's plays, and remembered characters and recognized quotations from them. He left Cal Tech understanding how the world worked, and with a deep respect for scholarly endeavors, which defined him.

Occupations. During summers in college, Bill had various jobs. He was a meter reader for the gas company; a draftsman for an engineering firm. The Navy paid his tuition. After college, in 1960, he worked at the Naval Undersea Research and Development Center in Pasadena, first as a full-time worker, then part-time while he was in graduate school at UCLA. After he received his doctorate in 1964, he returned to the NavyLab. One of his contributions there was a report in which he used stochastic processes and harmonic analysis to model strategies that a destroyer commander could use to evade a hostile submarine, after sonar contact had been established, but before any weapons had been deployed!

One day at the Naval Undersea Research Center, Bill got a call from his thesis advisor, Henry Dye, who told him to apply for the Benjamin Peirce Instructor job at Harvard. Bill was shocked. "Me?" he said. "Yes, you should apply, Bill," insisted Henry Dye. But Bill was ambivalent, in large part because the Navy had paid for his education, and he felt that he owed them. He took the issue to his boss, also a PhD-level mathematician, who told him that it would be payback enough if Bill were to thrive as an academic mathematician, training other mathematicians, and doing original mathematics research. Bill's eyes would tear-up when he told me that story, because he was so grateful, and impressed, by the generosity expressed by the man.

There is a picture of Bill at Harvard, looking so gloriously young, in this journal.

Bill joined the Berkeley Mathematics Department in 1968 as a lecturer, became an Associate Professor in 1969, and a Full Professor 5 years later. He retired in 2003, and continued doing mathematics research until his death last November [November 2011–Ed].

Bill Meets Lee. Bill and I met at the Tokyo airport in August 1985, on a bus that took passengers onto the tarmac to board their planes. There were two buses, and I almost took the less full one, because I was carrying a huge stuffed rabbit from Korea. "Did you have to buy a seat for that rabbit?", Bill flirted. Later, on the plane, he asked the stewardess to come and get me, since she wouldn't let him come up to first class where I was sitting. "He wants to buy you a drink, and I told him your drinks up here are free. But he came back twice, asking me to get you for him."

Bill was sitting way, way in the back of the dark plane. It was more than half empty. He was coming back from lecturing about mathematics in China



FIGURE 16. Bill and Lee at Cliff House, San Francisco, 1985, Thanksgiving Day (source: Lee Kaskutas)

for 6 weeks, he told me. I later joked with him that he decided to marry me when, hearing that, I responded that I had majored in math in college. Bill spread out this suitcase for me – it was full of beautiful trinkets from China, which he had bought with the Chinese money they had given him for his lectures. He had a lovely white cloisonné vase; a blue cloisonné dish; a pair of intricately carved red lacquer vases; a black cloisonné bangle, which he later gave me as a present—or maybe he gave it to me right then, on the plane; I forget. There was a little screen that unfolded, with Chinese-style drawings and writing on the panels; a series of eight blue cloisonné bells, hanging from a wooden frame made to look like a Chinese gate, which came with a little wooden pallet, to make the bells sing. All these things, and more, he showed me on that airplane.

Bill was so handsome, so interesting to talk to, so genuine, so easy to be with during that long flight to SFO. Getting off the plane, he asked for my number, and I told him I would be on another trip so not to call for a week. He didn't quite believe me, as I was just returning myself from 6 weeks in the Far East. But, luckily, he did call when I got back. We bought our house 7 months later, and married a year after that.

Life With Bill. My favorite thing about Bill, is how smart he is. How smart he *was*. I was so lucky, so happy, hearing him talk about ideas, and beliefs, and about how the natural world works. He was the smartest person I ever have known and ever want to know.

I loved and appreciated his complete generosity. I liked it that he let me be myself – uncritically. I liked it that he wanted to be with *me* for dinner;



FIGURE 17. Bill Arveson with Schnitzel (source: Lee Kaskutas)

that we didn't do things separately at night, with other people. Nor did we go out with other couples. I liked having him to talk to, to tell about my day. I loved our rituals around our dogs (miniature dachshunds!); the routines of our daily lives together; going to Sunday matinees. . . His voice, as he joked when I said something stupid like using a word wrong or pronouncing it incorrectly: he would then call me Lee Ann instead of Lee, with a tender playful tone.

Living with Bill was like having Google across from you at the dining room table. He was just so deep! He did not fool around inside that head of his. He was absolutely the clearest thinker one could ever know.

But he wasn't just smart; he also was wise, and warm, and witty. And he loved our dogs – always miniature dachshunds.

Bill's Routine. Every day, after breakfast and reading the paper, he would get out his clipboard and work. He used blue lined paper, and a uniball pen. He always sat at our dining room table, which is in a room full of light and has a view of the bay in one direction, and a canyon in the other. When it was time for lunch, he liked to have a sandwich, and to read a book for an hour. The book might be "D-Day" by Stephen Ambrose, David Halberstam's "The Fifties," "Blackwater" by Jeremy Scahill; biographies of Kennedy by Sorensen, Manchester; or something new by Bob Woodward. . . He liked reading books in hardback, and he used a leather book-weight so the book would stay open as he read. He worked until dinnertime.

When Bill did his mathematics research, he got excited about his discoveries and might write “WOW” in big letters, plus three exclamation points, and put a box around it. I only discovered this about Bill – that he was alone but not lonely doing his work – when I went through his papers to choose something handwritten for one of the collages for his memorial service.

I think mathematicians are a mysterious phenomenon to many. People often said, when Bill would tell them what he did for a living, “I hate math. It was my worse subject in school.” That is why I joked with Bill that the reason he married me was that I did not say that, when we met on that airplane and he told me what he had been doing in China.



FIGURE 18. Bill Arveson and some of his Mathematical Family at the Fields Institute Workshop on Noncommutative Dynamics and Applications, July 2007 (source: Fields Institute)

Family_{Bill} = Mathematical Family. Bill didn’t want to spend time with hardly anybody but other mathematicians. And me. And the dogs. One of his greatest pleasures was going to his math conferences, and being invited to give talks, be it in Chennai, College Station, Urbana, Ireland, Norway, the US Naval Academy, Banff, Haifa, Samos, GPOTS... the list goes on. (We spent the first part of our honeymoon at a math conference in Durham,

where men slept in one dorm, women in another. Naturally, Bill protested, and got us a room *together!*)

Bill considered the mathematics community his real family. *He said that all the time: “We are a family.”*

I remember vividly the first time I met some of Bill’s students: it was at LaVal’s, and Marcelo Laca, David Pitts, and Mike Lamoreaux were there. They told me that they all knew something was up with Bill when he started dressing better; that he must have met someone. A list of his students is included in this memorial issue.

Bill was enormously proud of his students. He shared with me, how he chose his students. He had two rules. They had to have demonstrated that they would be able to do deep, original mathematical research. But that is an obvious criterion. The second rule was that he had to *like* them; that they had to be nice, good people. He did not take on a new student lightly or thoughtlessly, and felt it to be a lifelong commitment. Whenever possible, we would have the student over to dinner at our house when they were graduating. Bill looked forward to these dinners very much, and afterwards he loved hearing what I thought of the person, as it was often the first time we had met. I liked them all, and loved some, as did he.

Beating Cancer. Now I will tell you a bit about Bill’s illness. Bill noticed a lump in his neck in November 2009, which they removed; a biopsy indicated it was nothing. Months later, when he still had a lump in his neck, it was determined that the surgeon had removed the wrong lymph node! An x-ray of his neck then detected something at the base of his brain that, quoting our doctor, “shouldn’t be there.” She ordered a brain scan. While Bill was still in the scanning machine, the technician brought in the radiologist, who asked Bill if he was feeling ok; had he driven to the clinic; had he been having headaches? No, he felt fine. The radiologist told him to walk, *not drive*, to his doctor’s office right away.

Of course, Bill drove there. She delivered the news that he had a dozen brain tumors, some quite large. She put him on steroids to reduce the swelling in his brain from the tumors, and was amazed he could still speak, still walk, still do mathematics! Two hours later, we were both at the oncology radiation department. I remember seeing the sign, “radiation oncology,” and thinking wow, *oncology*; we are going to an oncologist. This was March 15, 2010.

They started radiation on Bill’s precious brain the next day. After 10 days of full- brain radiation, Bill started chemotherapy. After several rounds of that, he was scanned again. When we met with the radiation oncologist to get the results, the doctor sat at his computer screen to look at the scan, and stopped immediately. He left the room, saying something about how it must be the wrong patient’s scan that had been loaded onto his computer. He came back: “No, it *is* your scan. There is zero evidence of any brain

tumors, plus no evidence of scarring, no evidence that you ever had those brain tumors.” He had never before seen that.

Several more rounds of chemo later, Bill was re-scanned, and they found one small tumor behind his esophagus. His case was twice taken to the cancer board, because doing radiation was dangerous and probably wouldn’t work anyway. They decided to try the radiation, 6-weeks worth. And again came the scary day to get the results of the treatment. The tumor was gone.

The kind of cancer that Bill had is famous for coming back as soon as chemotherapy is stopped, so they continued the chemo, 21 rounds in all. He also had many blood transfusions, which would take 7 hours. During chemo, and during the transfusions, Bill had his MacAir, his clipboard, his pen. And me; I always wanted to be there with him, at every treatment, every test, every doctor’s visit.

On September 16, 2011, Bill wrote this email to several colleagues:

You’ve probably noticed that I’ve been keeping a low profile - because of medical treatments - namely 20 rounds of chemo five days each, plus 10 days of full brain radiation and six full weeks of daily radiation focused on my thorax. All this invasive stuff has been going on for more than a year. Sounds awful, I know, which is why I haven’t been advertising it - I didn’t want to have to deal with explanations.

But now the results are in, and they are truly amazing - as good as could have been hoped for. In fact, the results couldn’t be better. I started this with stage 4 cancer more than a year ago and now, it is gone! Explanation: The radiation oncologist tells me that he has NEVER seen such a positive response to treatment - same from the chemo oncologist. The unbelievable part is that, I am cancer-free (no cancer cells detectable anywhere in my body by way of an MRI scan). Which means, my cancer is in remission!

So Lee and I are celebrating. We aren’t back up to normal socializing yet, I am on chemo right now and my energy level is low. I apologize for that but will make up for it. We are now planning the rest of our lives - now that we know that we have a life!

Falling. On Sunday, October 23, five weeks after writing that email, and on still another round of chemo, he fell at the movie theater, and broke his hip. It was a minor fracture that could be fixed in 40 minutes with a metal pin, the surgeon said. He was admitted; it was the first time he would ever spend the night in a hospital. The next day, when asked whether he wanted a local or general anesthetic, Bill’s choice was influenced by a bad experience with local anesthetic that had happened when his appendix was removed - he had felt them cutting into him. Plus, given his painful broken hip, the local anesthetic would have been difficult to administer. So Bill chose

general anesthetic. He had fasted more than the required 24 hours, and had only had a very light breakfast and half a peanut butter cookie at the movie the day before, so it seemed straightforward.

But it was day five of chemo – the kind of chemo that kills your small cells, the very cells that cause your food to digest. According to Bill’s oncologist, no one had ever had as much chemo of this type as had Bill. When they started the anesthesia procedure, stomach contents entered his lungs. They pumped out his lungs, put him on a ventilator... This began a downward spiral, which we were not able to stop.

Bill always said to me, “*Lee Ann*, life is not fair... *Lee Ann*, you cannot control everything.”

A Life Well-Lived and Remembered. A memorial service honoring Bill was held on February 19, 2012 at the Berkeley Faculty Club. Over 90 people were there. Thirteen of Bill’s 29 students from his mathematical family came to the service from afar, including one flying in from Montevideo, some driving up from southern California, and others flying from Alabama, Canada, Arizona, New York, Nebraska, Texas and Washington. Several of Bill’s closest colleagues made the trip, from UCLA, UC San Diego, University of Iowa, the US Naval Academy. Dozens of fellow professors from UC Berkeley attended.

At the service, we enjoyed French wine from Bill’s wine cellar, and listened to a local jazz band play some of Bill’s favorite tunes: All Blues, Corcovado, Desafinado, Django, Laura, Misty, My Funny Valentine, ‘Round Midnight, Samba Triste, Smile, Stardust, Tender is the Night, Tenderly, The Nearness of You, Third Gymnopedie, Time after Time.

During the service, Bill’s students and colleagues shared their memories and stories about Bill. Colleagues from Europe, Asia and the US who were unable to make the long trip sent statements to be read during the service. The stories we heard spoke to the depth and originality and elegance of Bill’s mathematics, to Bill’s influence on their own mathematics, and to the impact of Bill’s work in multiple fields of mathematics. His students talked about Bill’s generosity and his unfailing commitment to them as a mentor, and about how deeply he had inspired them. And Bill’s warmth and his sense of humor were emphasized by many of those who shared their memories of him. Here are some quotations from their stories:

Not just a great mathematician: “I used to think it unfair that someone could be so supremely talented both technically and socially.” ... “So many of us really loved and admired that man – he was one of the people who convinced me that mathematicians could be brilliant while being wonderful human being.” ... “Bill was a legendary figure to people my age, but you’d never know it from the way he treated people. Indeed, I could never decide which part of Bill I respected the most – amazing math talent or down-to-Earth genuinely

nice person. But now that I think about it, I respect the fact that he was both at the same time. What a rare and beautiful human being.”

Valued by fellow faculty: “What I knew of his field I had learned in graduate school years ago, and there were inevitably times when I needed to know more or to reassemble what I had forgotten. When this was the case, it was Bill I would ask. I never had to feel ashamed of my ignorance, and I could tell that he genuinely loved explaining even the simple things I needed to know.”

Cherished by students: “It was really great when you hit on something that Bill really thought was interesting. He would not be able to stay still, he would ask you to wait a second and try to get the answer himself, and scribble his ideas on the board. And if what he guessed was what you did, or even better, if you surprised him, the reaction was great. You could tell he wanted to jump in but contained himself to give you some space, because he respected your work on your problem.” ... “It is not possible for me to overestimate the influence Bill had on my training and professional development.”

Treasured by his field: “His work on subalgebras of C^* -algebras was probably the single most influential source of inspiration I have had in my career. I have been studying it ever since receiving it as a preprint. I often remark that, one of these days, I hope to understand it. What was so compelling about it, and equally compelling about all of Bill’s work, was the clarity of his exposition and the depth of the questions he addressed. He began with very simple ideas and worked through to deep conclusions. Bill was such a profound thinker. But he was also a wonderfully warm human being.” ... “He was a giant in his field, and will have a lasting impact for generations.” ... “The mathematics community has lost a giant.”

Something I realized only after he was gone, is that Bill had been a very happy, and always optimistic, person. He never once complained about getting cancer, about the chemo, the radiation, the fatigue, the lack of appetite. He wanted to get better; he was looking forward to attending GPOTS this year. I hope that they are able to do the GPOTS meeting in summer 2013 in his honor [Indeed GPOTS 2013 took place in Berkeley in Arveson’s honor. –Ed].

Bill’s wonderful attitude about life is echoed in the statement that his radiation oncologist wrote for me to read at the memorial service:

I met Bill at a time of great stress and fear in his life, as he began a battle for his life. I'd never known him before, but into my office he walked, and from the moment I met him, I thought "what an incredible, wonderful, witty and wise man! - the world can't afford to lose someone like him!" He was looking in the face of great tragedy, and he managed to pull off a smile and a laugh that made me want to pull out all the stops to try and help him. In the months to come when I would see him, he was never without a kind word, an optimistic outlook, and a real zest for life. I spend my working hours in small rooms trying to help people with life-threatening situations, and I've come to realize we all meet adversity with different faces, strengths and weaknesses. Bill never seemed to weaken - he took what was sent his way, and dealt with it serenely and rationally. I looked forward to those meetings with him and Lee in that small room, and I always walked away feeling better about the world and our role in it. That was a gift that Bill left with me, and I hope that I can pass a little bit of Bill onto everyone whom I meet going forward.

In closing, we should all remember that one of the many remarkable things about Bill is that he never expressed any regrets. He loved his life.

Arveson's Ph.D. Students.

- (1) Richard I. Loebel (1973)
- (2) Donal P. O'Donovan (1973)
- (3) Kenneth Davidson (1976)
- (4) Cecelia Laurie Bleecker (1976)
- (5) David Larson (1976)
- (6) Jon Kraus (1977)
- (7) Thomas Fall (1977)
- (8) Niels Toft Andersen (1977)
- (9) Earl Eugene Kymala (1980 - UC Davis)
- (10) Bruce Wagner (1980)
- (11) Belisario Ventura (1984)
- (12) John Spielberg (1985)
- (13) Jack Shaio (1985)
- (14) David Pitts (1986)
- (15) Richard Baker (1987)
- (16) Michael Lamoureux (1988)
- (17) Chikaung Pai (1988)
- (18) Hung Dinh (1989)
- (19) Marcelo Laca (1989)
- (20) Keith Manson (1989/informal)

- (21) Neal Fowler (1993)
- (22) Frederick Semwogerere (1994)
- (23) Alexander Alevras (1995)
- (24) Masayasu Aotani (1996)
- (25) Dylan SeLegue (1997)
- (26) Devin Greene (2001)
- (27) Daniel Markiewicz (2002)
- (28) Ilan Hirshberg (2003)
- (29) Dennis Courtney (2008)

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