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The Genesis of Geometric Algebra: A Personal Retrospective

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Abstract. Even today mathematicians typically typecast Clifford Algebra as the "algebra of a quadratic form," with no awareness of its grander role in unifying geometry and algebra as envisaged by Clifford himself when he named it Geometric Algebra. It has been my privilege to pick up where Clifford left off—to serve, so to speak, as principal architect of Geometric Algebra and Calculus as a comprehensive mathematical language for physics, engineering and computer science. This is an account of my personal journey in discovering, revitalizing and extending Geometric Algebra, with emphasis on the origin and influence of my book Space-Time Algebra. I discuss guiding ideas, significant results and where they came from—with recollection of important events and people along the way. Lastly, I offer some lessons learned about life and science.

1. Salutations

I am delighted and honored to join so many old friends and new faces in celebrating the 50th anniversary of my book Space-Time Algebra (STA). That book launched my career as a theoretical physicist and the journey that brought us here today. Let me use this opportunity to recall some highlights of my personal journey and offer my take on lessons to be learned. The first lesson follows:

2. Clifford Algebra Versus Geometric Algebra

A central theme in the history of mathematics is an intricate dance between geometry and algebra, with each playing solo on occasion. Drawing on contributions by Grassmann and Hamilton, the two were fused into a comprehensive Geometric Algebra by W. K. Clifford [8] in the middle of the nineteenth century. Ironically, in honoring Clifford by calling his system Clifford Algebra

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mathematicians devalued it by separation from its geometric roots. Consequently, for the better part of a century it was treated as just one more algebra among many without recognizing it as a universal algebra containing all others. Then, in the 1950s, following up on insights of Marcel Riesz [54], I picked up where I am sure Clifford was headed when his life was so tragically cut short and proceeded to develop Geometric Algebra into a unified mathematical language for all the sciences. Today it covers a broader range of physics, engineering and mathematics than any other system. Its scope is reviewed in [41].

In the following I review how the Geometric Algebra of today came to be. But first a product warning:

Do not confuse Geometric Algebra (GA) with Clifford Algebra (CA)!

Though they stand on common ground, they differ profoundly in culture, content and praxis. You may have noticed that mathematicians employ the term CA exclusively; whereas the term GA is employed only by physicists and engineers.

Some well-meaning individuals aim to bridge the gap and honor Clifford with the name "Clifford's Geometric Algebra." But that overlooks Clifford's express choice for a name that is not attached to a single individual, while he avers that Grassmann's contribution to GA is greater than his own. Indeed, the idea for GA was so much in the air that Rudolph Lipschitz discovered it independently only a few years later [47], though he deferred to Clifford's priority when he learned about it. More to the point, as I document in this paper, recent developments of GA beginning with Marcel Riesz vastly extend its original conception. Accordingly, I submit that GA is as much a collective product of human culture as the Real Number system. Both belong equally to us all, while both are enriched by stellar contributions from many individuals.

A more egregious mistake is attributing the fundamental formula

$$\mathbf{ab} = \mathbf{a} \cdot \mathbf{b} + \mathbf{a} \wedge \mathbf{b}, \tag{1}$$

to Clifford. Mathematicians who work with CA do not make that mistake, because, after a century of ossified tradition, they have still not recognized its deep significance in integrating Grassmann's inner and outer products. Clifford missed it himself. That is absolutely clear in his popular book *The Common Sense of the Exact Sciences*, so good that it is still in print today. There he introduced Grassmann's outer product and a version of the inner product, but he did not combine them to form his geometric product, nor did he relate them to complex numbers and rotations.

I can count on the fingers of one hand the number of mathematicians I know who recognize the role of Geometric Algebra in unifying mathematics. Why so slow? It has been said: "The husband is always the last to know!" Surely, one factor is the great chasm between mathematics and physics that opened up in the last century. As the distinguished mathematician Arnold opined [1]:

"Mathematics is a part of physics. Physics is an experimental science, a part of natural science. Mathematics is the part of physics where experiments are cheap. In the middle of the twentieth century it was attempted to divide physics and mathematics. The consequences turned out to be catastrophic. Whole generations of mathematicians grew up without knowing half of their science and, of course in total ignorance of other sciences."

Physics was traditionally a required minor for math students until it was dropped after World War II. Consequently, ninety percent of math professors today have studied no physics at all! They remain ignorant of advances in mathematics outside their narrow specialties.

3. Growing a Coherent World View

My story is unusual in that I only got started on serious study of physics at age 23, when many physicists are reputed to have already done their best work. One wonders if this late start was a serious handicap or if it gave me an opportunity to approach physics with a fresh perspective. In search for an answer, let me review some relevant facts about my life experience.

My father was a distinguished mathematician, with major contributions to Optimal Control Theory [21] and Numerical Analysis [28]. He was proud and self-assured, but modest; kind and tolerant, but firm and exacting. He never tried to interest me in his own work or teach me mathematics. Rather he left me to my own devices. Truth is, I was an unproblematic child. My grades were good, and I took all the available science and math courses in high school, though I didn't put much effort into them. Early on I made it a game to finish homework as fast as possible, often as fast as the teacher assigned it to the class.

My development as a teenager was driven by two major forces: games and religion. I avidly played all kinds of games, including sports. I usually won, mainly, I think, because I deliberately focused on analyzing tactics and strategy. In trying to invent new games, I learned how difficult it is to define a good set of rules.

The single most exciting and influential period of my life was the summer of 1947, when I spent every day and night in the company of my cousin Marguerite Eastvold, a vivacious and beautiful woman some 4 years older than me. She thoroughly inculcated in me a coherent Christian world view. She took me to spectacular revivals by the famous evangelist Billy Graham. She got me in the habit of daily Bible study and memorizing salient verses. She convinced me that my talents and interest in science suited me for a career of service to the Lord and humanity as a missionary doctor. All this persisted through high school into college, where I fell into roles of Christian leadership myself.

In college I began to find cracks in the Christian world view that led me toward critical philosophy and science. Indeed, the evolution of my teenage world view amounted quite literally to a recapitulation of the Reformation and Enlightenment in my personal life. It was driven, in part, by intense study of the Bible, which sensitized me to deep subtleties in language structure and meaning. That has served me well in my subsequent scholarly career. I was especially struck by wide divergences in different translations of certain

Bible passages. From this I concluded that there is no such thing as a truly literal translation, and I wondered how language gets its meaning. This led me eventually to wonder how mathematics gets precise meaning and greater truth value than ordinary language. It became a driving force in my research on Geometric Algebra and my *Modeling Theory* of learning and cognition [39].

Among other things, it led me (largely under the influence of Jaynes [42, 45]) to conclude that Information Theory is grounded in a sharp distinction between message and meaning. Messages conveyed in human perception and communication as well as scientific measurement have information content measured by Shannon's entropy, but they have no meaning. Meaning of a given message is supplied by the human receiver using a codebook derived from evolution and experience. That insight lies at the foundation of *Modeling Theory*.

I entered UCLA as a pre-medical major in preparation to be a missionary doctor, but I had dispensed with that plan by the end of my sophomore year when I failed the first course in calculus. I was actually given a passing grade because my father was chair of the mathematics department. However, I knew that the real reason for failure was because I spent most of my sophomore year playing chess. That turned out to be time well spent when later I studied what chess can tell us about cognitive processes.

To break from my past and start anew, I started my junior year as a speech major at Pacific Lutheran College, a thousand miles from UCLA. There I discovered the beautiful Nancy Shinkoethe, who became my wife before I graduated. Also in the first semester I got hooked on philosophy and extended it to a major by the time I graduated.

After my junior year I started reading philosophy of science, including Hans Reichenbach and Bertrand Russell. I was dismayed to find that every argument about the physical world was supported by quotes from physicists like Einstein, Bohr and Schroedinger. I concluded "This can't be philosophy, this is the 'revealed word.' The real philosophers must be the physicists." So I decided to become a physicist, and I took the first course in freshman physics with another stab at calculus in the last semester of my senior year. I had the same professor for both courses, and when he gave me barely passing grades at the end, he said memorably: "You'll never make it as a physicist, Hestenes!" That didn't faze me a bit, because I knew my father was very good at math, and I didn't think he was so smart! After all, I could crush him at chess.

To support my wife and budding child while I studied physics in graduate school, I got the GI bill after serving a 2-year stint as a private in the U.S. Army. That stint taught me great lessons about the hard life of the underprivileged and the workings of government institutions. It convinced me that all young people should be required to devote 2 years to some kind of public service such as Peace Corps, Teach-for-America or the military.

Then, in 1956 at age 23 I started graduate school in physics at UCLA with one semester of freshman physics behind me.

4. Accelerating into Physics

When I entered the UCLA graduate school as an unclassified student, I was assigned an experimental physicist as academic advisor, and he told me it would take 4 years to make up the deficiencies in my physics background. Alarmed at the prospect, I ran to my father who said, "Go talk to Dave Saxon!"

David Saxon was an Assistant Professor of Theoretical Physics. Ultimately, he turned out to be the savviest of academic politicians, rising to be President of the entire University of California System, the most powerful academic position in the United States if not the world. Years later I learned that he was deeply obligated to my father. Caught up in the paranoia of McCarthy hearings on communist infiltration into American institutions, the University of California instituted a Loyalty Oath for all employees. When Saxon refused to sign in protest, he was fired from his professorship at UCLA. But he did not have to leave campus, because my father rehired him at once for the Institute for Numerical Analysis (INA) where my father was co-director. After the Loyalty frenzy was over a few years later, Saxon returned to his position in the UCLA Physics Department.

Saxon set up a program of study that enabled me to complete the essential undergraduate physics courses in 1 year. Since I aimed to become a theoretical physicist, he let me omit all the lab courses. I skipped some courses because they were prerequisite to more advanced courses that I did take. Of course, this left some holes in my background, but I had no trouble filling them in later when needed. There is a great lesson in this for all academics. Students spend far too much time meeting academic prerequisites tailored to each specialty, be it in physics, engineering or mathematics. Better to let them move on where fancy takes them and pick up what they need along the way. Students need flexibility to choose a path that suits them best.

With undergraduate physics under my belt after my first year in graduate school, in the second year I completed the required graduate physics courses, passed the graduate Ph.D. qualifying exams and received a Master of Science degree in Physics. My grades were very good and my teachers were impressed, so I was one of only two students in the whole graduate school to be awarded a University Fellowship to support me for the 1958–1959 school year. Good thing too, because my GI Bill support had run out.

I was riding high, eager to start doctoral research. Then disaster struck! I failed my doctoral oral exam, the gateway to research. The committee asked me to work out a standard bit of quantum mechanics which happened to be one of the holes in my background. While I struggled to make progress, the committee, including Saxon, harassed and cajoled me all the way. Though I managed to solve the problem in half an hour, the committee was unimpressed and required that I study for a whole year before repeating the exam. I was devastated! But they were right, of course. Years later, after I had become a professor myself, I realized that I had actually performed better than most students do, and they could readily have passed me. But the committee was looking out for my interests. They thought I was moving along the academic path too fast and decided to slow me down. When I returned for the exam a year later, I was not asked a single question I had prepared for. Fortunately, I just happened to have fluent answers to every question that they did ask, including one about asymptotic properties of spherical Bessel functions. After only 20 min, Saxon jumped out of his chair and said He passes! as he stomped dramatically out of the room. The rest of the committee folded soon thereafter. I was finally ready to start research! And Saxon saw to it that I was awarded a full time Research Assistantship for financial support.

The 3 years of academic preparation just described was only half my graduate education. The other half came at the INA (Institute for Numerical Analysis) where my father set me up in an office for quiet study. The INA was established shortly after WWII for research on the first electronic computer in western U.S., the so-called South Western Automatic Computer (SWAC). Driven by exceptional engineers and mathematicians that flocked from around the world, the SWAC was an incubator for the first generation of Computer Scientists. There I was, right in the middle! Of course I had to try my hand at computer programming. But programming was done in machine language in those days. I soon concluded that life is too short for that, so I steered clear of programming for the rest of my life, though I imbibed the culture of computer science.

The INA had another attraction for me. When Saxon was there he was given a budget to create a library for the INA. There I found more than a hundred of the best physics books and exciting new books on artificial intelligence like W. Ross Ashby's "Design for a Brain," which hooked me on artificial intelligence. I had the library all to myself, with no other physics students around. How exciting!

You could say that I benefited from "special privilege," owing to my father's position. But the truth is, any student could have much the same. I know several math and engineering students who opted to hang out at the INA, and that morphed into Computer Science careers. University students should know that if they just seek out what research professors are doing they are likely to find something that interests them. And if they express that interest to the professor, for example by hanging around, they are likely to be taken on to help. Like anyone else, professors are gratified by interest in their work.

One special privilege I did get from my father in the summer of 1959 was the opportunity to attend an expensive 2-week course on "Information Theory and Statistical Mechanics" by E. T. Jaynes, intended exclusively for industry executives and researchers. I was the only student who got to attend—for free! That was the most profound and gripping series of lectures I have ever heard, including a complete course on quantum electrodynamics from the spectacular Richard Feynman which I attended a couple of years later. It introduced me to a rich theme for research and teaching throughout my career: interplay between Shannon's statistical concept of information and the Clausius concept of entropy!

5. Research Begins: Discovering a Theme

In the fall of 1959, with graduate exams behind me and Research Assistantship in hand, I disappeared from the Physics Department for an entire year to study in the Math Department, where my father arranged for me to use an empty office reserved for visiting scholars.

This gave me complete freedom to pursue my own research agenda, which was already heavily influenced by reading the philosopher Bertrand Russell. I was quite consciously focused on foundations of mathematics and its role in physics and epistemology. Two courses I had taken the previous year proved to be especially relevant: First, a course in Feynman's approach to Quantum Electrodynamics (QED); Second, a unique course on General Relativity that introduced me to Schroedinger's elegant little book *Spacetime Structure* [56]. In the Math Department I got to take a new graduate course in Differential Geometry by Barret O'Neill [50]. This may have been the first course in the United States to include a thorough introduction to differential forms with applications. These three courses provided ideal preparation for a serendipitous event that changed my life and ultimately brought all of you here today.

One day when I dropped into the Mathematics Library, as I often did, I noticed on the "New Book Shelf" a set of lecture notes entitled *Clifford Numbers and Spinors* by mathematician Marcel Riesz [54]. They were based on lectures given by Riesz at the Institute for Fluid Dynamics and the University of Maryland. They were not formally published, and I don't know why a copy was sent to UCLA, perhaps because Riesz had been a guest of my father a few years before.

I was excited by what I read from the get go. The lectures began by factoring the well-known "Laplacian" into the square of a first order differential operator with coefficients he called "Clifford numbers." Though Dirac was not mentioned, I recognized this at once as a variant of Dirac's famous factorization of the "d'Alembertian" to get the Dirac operator and the Dirac equation. But Dirac's coefficients were matrices. I knew from my QED course that physical predictions are independent of matrix representation, so I immediately concluded that Dirac matrices should be replaced by Clifford numbers in the equations of physics, and I suspected that Clifford numbers could be identified with vectors. These convictions were supported by lecture IV, where I saw the Dirac operator written as $\nabla = e^k \partial_k$ and Maxwell's equations written in the now familiar form

$$\nabla F = \nabla^2 A = J. \tag{2}$$

Before that stunning afternoon was over I had my doctoral research objective clearly in mind: To develop Clifford algebra into a unified, coordinate-free calculus for all of physics. The project was not so straightforward as it might look from hindsight today.

I set about devouring the Riesz lectures immediately, and anyone familiar with STA today will recognize important notions that I picked up there and have used extensively since. But what may not be so obvious are the crucial insights that I introduced to elevate Clifford algebra to a comprehensive Geometric Algebra. For example, Riesz demonstrated how Grassmann's exterior product can be incorporated into Clifford algebra, but he did not note its geometric interpretation, which is crucial to applications. I adapted the interpretation of differential k-forms that I learned from O'Neill to interpret k-vectors as directed k-dimensional volumes, though it was many years before I was clear on the difference between k-forms and k-vectors. Also, I recognized many relations of Riesz's expansion formulas to Feynman trace calculus and the theory of determinants. Eventually I learned that Grassmann had captured it all a century earlier in his Regressive Product. He derived a rich system of formulas that have been repeatedly rediscovered by other mathematicians including myself. As I was eventually able to show [44], his progressive (exterior) and regressive (interior) products provide a foundation for invariant theory including the "theory of determinants."

Within a couple of months I made my first scientific discovery: The geometric connection of Grassmann algebra to Clifford algebra, and their subtle relations to the Pauli matrix algebra. I was so excited that I prepared a private mini lecture to tell my father about it. This story has been told before [31], but it was so crucial to my career that a brief recounting is in order here. Even today, the symbols $\boldsymbol{\sigma}_1, \boldsymbol{\sigma}_2, \boldsymbol{\sigma}_3$ are referred to as Pauli Spin Matrices in books on quantum mechanics, where it is noted that they anticommute:

$$\boldsymbol{\sigma}_1\boldsymbol{\sigma}_2 = -\boldsymbol{\sigma}_2\boldsymbol{\sigma}_1. \tag{3}$$

and satisfy the striking formula:

$$\boldsymbol{\sigma}_1\boldsymbol{\sigma}_2\boldsymbol{\sigma}_3 = i. \tag{4}$$

Quantum physicists claim that anticommutivity expresses incompatibility of spin measurements in orthogonal directions. On the contrary, I proposed that $\{\boldsymbol{\sigma}_1, \boldsymbol{\sigma}_2, \boldsymbol{\sigma}_3\}$ is simply a frame of three orthonormal vectors; anticommutivity expresses orthogonality, and $\boldsymbol{\sigma}_1\boldsymbol{\sigma}_2$ represents a directed area, while $\boldsymbol{\sigma}_1\boldsymbol{\sigma}_2\boldsymbol{\sigma}_3 = i$ is a pseudoscalar representing an oriented volume. How is it, I asked my father, that the great theoretical physicists and mathematicians Pauli, Dirac, Weyl, von Neumann—all failed to recognize the geometric import of the Pauli algebra. My father's response is the greatest compliment I ever received: "You have learned the difference between a mathematical concept and its representation by symbols—many mathematicians never learn that!"

Of course, the geometric interpretation of the Pauli algebra raised many new questions about the interpretation of quantum mechanics, such as, whether the pseudoscalar i is related to the imaginary unit in Schroedinger's equation. However, I was unable to get my fellow graduate students excited about it. "Why i?" they teased. I soon learned that it is even harder to interest professors, including my thesis advisor. I needed more ammunition.

6. Genesis of Spacetime Algebra

In fall 1960 I returned to the physics department to be close to my thesis advisor, Robert Finkelstein, and complete my thesis. For the next 3 years my efforts oscillated back and forth between investigating Finkelstein's (brilliant but ultimately unsuccessful) ideas on Unified Field Theory and my own interest in developing geometric algebra and its implications in physics.

Having identified the 2×2 Pauli matrices with basis vectors in Euclidean 3-space, it was obvious to identify the 4×4 Dirac matrices with basis vectors in spacetime. The relation between them was not so obvious though, because the matrices are of different dimension. Moreover, spinor wave functions in the Pauli and Dirac theories are column matrices, so their relation to vectors was not obvious either. Perfect resolution of these issues turned out to be keys to creating the spacetime algebra in my book [19,20], but I had to complete an imperfect doctoral thesis before I could see how to do it.

It was undoubtedly good for me that the chapter on spinors promised in Riesz's lecture notes never got written, because I had to work it out for myself. He did drop a hint though, namely, that spinors can be represented as elements of minimal ideals in a Clifford algebra. They are easily constructed by recognizing that the columns of a matrix are minimal left ideals of the matrix, that is, columns are invariant under matrix multiplication from the left (an important fact that is seldom mentioned in books on linear algebra). I will explain how that played out in my doctoral thesis.

As the title *Geometric Calculus and Elementary Particles* [18] suggests, my thesis was composed of two parts. I was most excited about the first part on Geometric Calculus, but my thesis advisor expressed no interest in it. He liked the second part on Elementary Particles, about which I was privately highly dubious.

In reviewing Part I, let me change notation in my thesis to current GA notation that did not exist at the time. Accordingly, the geometric algebra generated by a vector space $\mathcal{V}^{r,s}$ of signature (r,s) is denoted by $\mathcal{G}_{r,s} = \mathcal{G}(\mathcal{V}^{r,s})$, or better by $\mathbb{R}_{r,s} = \mathcal{G}(\mathbb{R}^{r,s})$ to emphasize that the scalars are assumed to be real numbers $\mathbb{R} = \mathbb{R}_{0,0}$ exclusively. Indeed, GA can be regarded as an extension of the real numbers to include the concepts of magnitude, direction and signature.

The geometric algebra of Euclidean space $\mathbb{R}_3 = \mathbb{R}_{3,0}$ is generated by orthonormal vectors $\{\boldsymbol{\sigma}_k\,;\, k = 1,2,3\}$. This is, as we have noted, isomorphic to the Pauli matrix algebra. I used $\mathcal{D}_4$ to denote the (real) Clifford algebra generated by an orthonormal frame of vectors $\{\gamma_\mu\,;\,\mu = 0,1,2,3\}$, which are isomorphic to the Dirac matrices. This is the geometric algebra $\mathbb{R}_{1,3}$ now known as *spacetime algebra* (STA). The standard relation of the Pauli algebra to the Dirac algebra can be expressed by the tensor product

$$\mathcal{D}_4 = \mathbb{R}_{1,3} = \mathbb{R}_3 \otimes \mathbb{R}_2. \tag{5}$$

This factorization of the "Real Dirac algebra" $\mathcal{D}_4$ is an instance of Clifford's original construction of geometric algebras as commuting products of quaternion algebras [8]. To express it in terms of generating basis vectors, let

$\{\boldsymbol{\tau}_1, \boldsymbol{\tau}_2\}$ be an orthonormal basis for $\mathbb{R}_2$. Then the vectors and pseudoscalars are related by

$$\gamma_0 = \boldsymbol{\tau}_2\boldsymbol{\tau}_1, \quad \gamma_k = \boldsymbol{\sigma}_k\boldsymbol{\tau}_2, \quad \gamma_5 \equiv \gamma_0\gamma_1\gamma_2\gamma_3 = -i\boldsymbol{\tau}_1. \tag{6}$$

Dirac's alpha matrices are then isomorphic to the Clifford numbers

$$\alpha_k \equiv \gamma_k\gamma_0 = \boldsymbol{\sigma}_k\boldsymbol{\tau}_1, \quad \alpha_1\alpha_2\alpha_3 = \gamma_5 = -i\boldsymbol{\tau}_1. \tag{7}$$

The significance of the $\boldsymbol{\tau}_1, \boldsymbol{\tau}_2$ algebra remained a mystery to me until after my thesis was completed.

The most profound consequence of this representation for real spacetime algebra is that it enables a completely coordinate-free geometric formulation of the Dirac equation, which implies a geometric meaning for complex numbers in quantum mechanics. Unraveling its implications is still underway.

Part I employs STA to create a *gauge invariant* geometric calculus for spinor fields on spacetime. It should be recognized that, at the time, gauge invariance was just beginning to be recognized as a fundamental principle for elementary particle theory.

The gauge transformation of a spinor field $\Psi = \Psi(x)$ is defined by

$$\Psi \to R\Psi S, \tag{8}$$

where $R = R(x)$ and $S = S(x)$ are rotor fields normalized by $R\widetilde{R} = S\widetilde{S} = 1$. The gauge invariant derivative is given by

$$\nabla_k\Psi = (\partial_k + C_k)\Psi + \Psi B_k, \tag{9}$$

where $C_k = C_k(x)$ is the gravitational field connection, and the $B_k = B_k(x)$ are "gauge fields" for non-gravitational interactions.

Part II in my thesis studied models of fermions represented as ideals in $\mathcal{D}_4$. Just as columns are left ideals in matrix algebra, minimal left ideals in $\mathcal{D}_4$ can be defined by idempotents (serving as projection operators); thus

$$\Psi_{\pm\pm} = \Psi\tfrac{1}{2}(1 \pm \boldsymbol{\sigma}_3)\tfrac{1}{2}(1 \pm \boldsymbol{\tau}_3), \quad \boldsymbol{\tau}_3 = i\boldsymbol{\tau}_2\boldsymbol{\tau}_1, \tag{10}$$

with

$$\Psi_{\pm\pm} = (\Psi_{++}\Psi_{+-}|\Psi_{-+}\Psi_{--}), \tag{11}$$

These ideals were identified with positive, neutral and negative charge states for known Fermion families at the time:

$$\begin{aligned} &\text{Leptons:} \quad L = (\mu^+\nu_1|\nu_2 e^-) \\ &\text{Nucleons:} \quad N = (pn|\Xi^0\Xi^-) \\ &\text{Hyperons:} \quad Y = (\Sigma^+ Y^0|\Sigma^0\Sigma^-) \end{aligned}$$

I employed them in models of Hyperon decays, which seemed to impress Finklestein. However, I decided not to publish any of that because I thought it was no better than other models already in the literature.

If my idea of associating elementary particles with minimal ideals in the Dirac algebra has physical significance, it must come from geometric meaning of the algebra. Years later, when the Standard Model had finally settled on the Electroweak gauge group $SU(2)\times U(1)$, I pointed out that it fits perfectly into Dirac theory [30, 40]. Whether that has new physical implications is still uncertain.

After I completed my doctorate in 1963, Saxon again intervened to offer me a 1 year research position. That was just what I needed to support my family while I looked for a postdoctoral position. My first child, Kristina, was born while I was in the Army; my second child, Karen, was born on my first day of graduate school in 1956; two more, Helen and Glen were born in 1958 and 1960, respectively. By the end of the year I was awarded a 2 year NSF Postdoctoral Fellowship to study with John Wheeler at Princeton. The fellowship included a generous supplementary allowance for dependents. Consequently, my yearly income exceeded that of an Assistant Professor at Princeton. That may be some kind of a record.

How the mores of family and education had changed since my birth! Before WW II it was virtually unheard of for undergraduate or graduate students to be married. For example, my father put off marriage to his college sweetheart until after his Ph.D. in 1932. I was born eleven months later. When the GI Bill was offered to veterans after WW II, they flocked to the colleges, but most of them were married. To manage the influx of families, UCLA imported a raft of surplus Army barracks for “married student housing.” This on-campus housing was still available to me at a nominal price after the Korean war. Support for married students has declined ever since.

7. Crystallization of Spacetime Algebra

Shortly after completing my Ph.D., I recognized that the $\boldsymbol{\tau}_1, \boldsymbol{\tau}_2$ algebra in (6) has no physical significance and serves only to complicate the equations, especially in electrodynamics. It is a superfluous vestige of matrix algebra. The physical content of the algebra lies in relations of the Pauli and Dirac algebras to the geometry of space and spacetime. I saw that, in contrast to (6) these algebras are most efficiently expressed by defining the *spacetime split*

$$\boldsymbol{\sigma}_k \equiv \gamma_k\gamma_0, \quad \boldsymbol{\sigma}_1\boldsymbol{\sigma}_2\boldsymbol{\sigma}_3 = i = \gamma_5. \tag{12}$$

Immediately I set about incorporating this simplification in major portions of my thesis and in three months completed a paper entitled “Space-Time Algebra,” which I submitted for publication in *Reviews of Modern Physics*. The referees recommended that it be published as a book, and the editor, Elliott Montroll, offered to publish it in an international series of books by distinguished physicists for which he was also the editor. Indeed, shortly after publication, when my father was visiting a mathematician in India, he saw a copy of my book on the professor’s desk. A bit of luck for me! Such wide distribution turned out to be very helpful, for the book STA hit some important targets that would have been missed by a journal article.

The *spacetime split* offers an important lesson about a difference in thinking between physicists and mathematicians, or, if you will, between geometers and algebraists. The algebraist would never think of introducing the spacetime split, because it is geometrically motivated. Indeed, both mathematicians and physicists committed to the matrix formalism take no notice of it in my published works.

My postdoc at Princeton 1964–1966 may have come at the busiest time of John Wheeler's career. He had just coined the term "black hole" and was leading an international renaissance in General Relativity. He was so busy traveling that he missed half the classes in his relativity course, though they were admirably filled-in by his graduate student Kip Thorne. After my first year, Wheeler wrote me a long, rambling letter from his vacation home in Martha's Vineyard, apologizing for spending so little time with me. He framed it like a mystery story about another postdoc that he had seriously neglected, with the punch line at the end: "And that man was Roger Penrose!"

Actually, I was not much bothered by Wheeler's lack of attention. Princeton was teeming with clever postdocs in physics and mathematics. There were many stimulating colloquia, and excellent courses by Wigner, Wightman, Goldberger and others. Besides, I had a burning research question of my own to investigate.

GA has many square roots of minus one. In particular, spacelike vectors, bivectors and pseudoscalars in $\mathbb{R}_{1,3}$ all square to minus one. Which one of these plays the role of unit imaginary in quantum mechanics, and what is its geometric justification? I had my suspicions. But how can one prove it? The answer is given in my paper "Real Spinor Fields" [22]. It is the capstone of my reformulation of physics with STA. Because of its importance, I summarize its essential points here. See [36] for more details.

Although expressed differently in my paper, I began with a spinor ideal of the form (10)

$$\Psi = \psi \tfrac{1}{2}(1 + \boldsymbol{\sigma}_3)\tfrac{1}{2}(1 + \boldsymbol{\tau}_3). \tag{13}$$

Then I showed that the coefficient $\psi = \psi(x)$ on the right can be uniquely expressed as an even multivector with the *Lorentz invariant* form

$$\psi = \rho^{\frac{1}{2}} e^{i\beta/2} R, \tag{14}$$

where $\rho = \rho(x)$ and $\beta = \beta(x)$ are scalar-valued functions, and "*rotor*" $R = R(x)$ is normalized to $R\widetilde{R} = \widetilde{R}R = 1$. We can now dispense with the idempotent projections in (13) and regard $\psi = \psi(x)$ as the spinor wave function. This version of the wave function has a more direct physical interpretation than the ideal form. In particular, the Dirac current and the spin are given, respectively, by

$$\psi\gamma_0\widetilde{\psi} = \rho R\gamma_0\widetilde{R} \equiv \rho v \quad \text{and} \quad s = \frac{\hbar}{2}R\gamma_3\widetilde{R}. \tag{15}$$

This shows that $R = R(x)$ specifies a position dependent Lorentz transformation determining directions of the Dirac current and spin at each spacetime point. The Lorentz invariant "β-factor" in the general form (14) for a "*Real Dirac spinor*" is so deeply buried in matrix representations for spinors that its existence is not generally recognized by physicists, and its physical interpretation has remained problematic to this day.

In terms of the "*real Dirac wave function*" (14), I found that the Dirac equation can be written in the form

$$\gamma^\mu \left(\partial_\mu \Psi \mathbf{i}\hbar - \frac{e}{c} A_\mu \Psi\right) = m_e c \Psi \gamma_0, \tag{16}$$

where m_e is electron mass and $e = \pm|e|$ is the charge coupling constant, while the $A_\mu = A \cdot \gamma_\mu$ are components of the electromagnetic vector potential. It should be emphasized that this version of the Dirac equation is isomorphic to the standard matrix version, so it involves no new assumptions, and by itself it can have no new physical consequences. However, it does reveal geometric structure that is buried in the matrix version, so it has implications for interpretation of Dirac theory. In particular, it nails down a geometric interpretation for imaginary numbers in quantum mechanics, thus resolving the ambiguity that troubled me when writing my STA book.

The symbol $\mathbf{i}$ in (16) denotes a unit bivector, which can be written in the following equivalent forms:

$$\mathbf{i} \equiv \gamma_2\gamma_1 = i\gamma_3\gamma_0 = i\boldsymbol{\sigma}_3 = \boldsymbol{\sigma}_1\boldsymbol{\sigma}_2. \tag{17}$$

The notation $\mathbf{i}$ emphasizes that it plays the role of the unit imaginary that appears explicitly in matrix versions of the Dirac equation, and in Schroedinger's equation when derived as a nonrelativistic limit of the Dirac equation [16, 27]. Moreover, through angular momentum conservation [26] the Dirac equation implies that the direction of that plane is related to the spin (15) by the *spin bivector*

$$S \equiv isv = \frac{\hbar}{2} R\,\mathbf{i}\widetilde{R}. \tag{18}$$

Thus, we have a connection between spin and phase, revealing phase as an angle of rotation in the "*spin plane*." In view of the unparalleled success of the Dirac equation in physical explanation, this iron clad connection between spin and phase must be explained by any satisfactory interpretation of quantum mechanics. The widely accepted "Copenhagen interpretation" does not appear to meet that standard [27].

Defining a vector differential operator $\nabla = \gamma^\mu \partial_\mu$ puts the real Dirac equation (16) in the coordinate-free form

$$\nabla \Psi \mathbf{i}\hbar - \frac{e}{c} A\Psi = m_e c \Psi \gamma_0\,, \tag{19}$$

where $A = A_\mu \gamma^\mu$ is the electromagnetic vector potential. The operator ∇ is often called the "Dirac operator," and physicists typically regard it as uniquely tied to the Dirac equation and the mysterious origin of spin. But the astute reader will have noted that, when formulated in terms of STA, the same operator appears in Maxwell's equation (2), where it has nothing to do with spin. Indeed, we have seen that spin was (you might say) surreptitiously introduced into the Dirac equation with the "imaginary factor" $\mathbf{i}\hbar$. That leaves us free to regard $\nabla = \partial_x$ as the derivative with respect to the spacetime point x—a key to *Geometric Calculus*, as we see below.

As far as I know, the only person to pick up on these results from my 1967 paper [22] was the French mathematician Roget Boudet. He was so impressed that he spent the rest of his career investigating their implications [3,4], and he spread the word about STA to every Frenchman who would listen. One of the first to listen was Gaston Casanova, who then devoted his Ph.D. thesis and many short papers afterward to my "real Dirac equation" (16). Circa 1970, Casanova sent me a paper attributing that equation to my 1966 book instead of my 1967 paper. To my utter surprise when I looked in the book, there it was! I had indeed derived the equation there, but hadn't fully understood what I had achieved. Understanding was not complete until I derived the form for the wave function (14) along with the observables (15) and (18). What a stiking lesson in the subtlety of mathematical discovery!

8. From Spacetime Algebra to Geometric Calculus

Near end of my 2-year postdoc in 1968 I began to look for a permanent academic position. The search was happily short. Wheeler had just been approached by the chair of a recently created graduate physics program at Arizona State University (ASU), and he recommended me for a faculty position. I was immediately invited to give a colloquium at ASU and offered a position as Assistant Professor on the spot. Before accepting, I talked it over with my father. He told me that many new graduate programs had recently been created across the United States, and he had served on an NSF committee to evaluate them. He allowed that ASU was among the most promising and sure to grow rapidly, so it would offer great opportunity to "write your own ticket." Boy, was he right on all counts! ASU was the most rapidly growing university in the country during my entire tenure there, and it is now the largest university in the United States. More important to me, I was indeed able to write my own ticket from the get–go. The department chair agreed to let me teach only graduate courses of my own choosing and design for the first 5 years. That included electrodynamics and relativity using GA and statistical mechanics with the information theory approach of E. T. Jaynes. Of course, it was a great help in my program to work out details in applying GA as a unifying mathematical system for all of physics.

My graduate courses attracted and prepared a lot of good students to work with me. I mention two dissertation projects most relevant to the main thrust of my own research at the time.

Richard Gurtler had already been working as an industrial physicist at Motorola when he started with me, so he was well prepared and ripped through his thesis in a year without the least hesitation. I had him analyze solutions of the Dirac equation for hydrogen using STA, especially to describe the peculiar behavior of the parameter β in the wave function (14). The results have been valuable to me, as they clearly showed how problematic it is to find a viable physical interpretation for β. I still regard it as an important clue toward improving Dirac electron theory. On completing his Ph.D., Gurtler went right back to his job in industrial research on photovoltaics. I listed his name on a couple of papers I wrote after he left [16,25], because we had substantial discussions on some of the material included.

Garret Sobczyk took my physics courses while he was a graduate student in mathematics, so I was pleased when he asked me to supervise his doctoral dissertation. As discussed below, I had recently published two papers on Geometric Calculus in a mathematics journal, so I had many ideas on how to follow that up. For a year we had regular meetings when I gave him one good idea after another, and he came up empty each time. Then he seemed to undergo some kind of conceptual phase transformation, suddenly extracting interesting results from most of my suggestions. He quickly produced an elegant thesis, which I still regard as the best I have seen at ASU.

After Sobczyk had finished his dissertation, in fall 1971 I arranged to teach a graduate mathematics seminar on geometric calculus with extensive problem sets graded by Sobczyk. This gave me time to integrate Sobczyk's thesis with my own ideas and write it all up in three long papers, which I submitted for joint publication in a math journal. In the meantime, Sobczyk left for adventures as a postdoc in Poland behind the Iron Curtain, and I did not see him again for more than a decade. After a year, the papers were rejected with the recommendation that they be published as a book. I then submitted the papers to a different math journal with the same outcome after another year. Before continuing this story, let me backtrack and pick up the thread from the beginning.

In my dissertation I championed Geometric Algebra as a unique fusion of geometry and algebra. But when the time came to write up my ideas for journal publication, I had second thoughts. Was it a conceit of mine to think that there is but one Geometric Algebra that embraces all others? After all, I knew that other mathematicians laid claim to the name [2]. This question bothered me so much that my first two papers on the subject are conservatively entitled Multivector Calculus [23] and Multivector Functions [24]. I realized that more work was needed before I could be absolutely confident in asserting universality and uniqueness for *Geometric Algebra and Calculus.* These papers started that work. They are straightforward generalizations of ideas in my STA book.

The first paper argued that the differential operator $\nabla = \partial_x$ in "multivector algebra" (alias GA) should be regarded as no more and no less than the derivative with respect to a vector variable x. This has the huge advantages of reducing gradient, divergence and curl to a single operator in spaces of any dimension, and reducing the various integral theorems of Green, Gauss and Stokes to a single theorem. I knew that all this was related to differential forms, but the connection was unclear. That was a topic I discussed at length with Sobczyk. It took years to clear up to my satisfaction, largely because it became intertwined with issues of differential geometry. Indeed, it served as a major theme driving development of Geometric Calculus.

In the second paper I demonstrated that there is a natural "Green's function" associated with the vector derivative that generalizes Cauchy's integral theorem in two dimensions and extends it to arbitrary dimension. Since Cauchy's theorem is central to complex variable theory, I thought that this should be regarded as a major result. I wondered why I had not seen anything like it in the mathematics and physics literature. It was more than 15 years before I found an answer. We shall come back to that later. In the meantime, of course, my "spectacular theorem" was completely overlooked.

In 1970 I submitted an article on the "Origins of Geometric Algebra" to the *American Mathematical Monthly* to call attention to the geometric significance of fusing Grassmann algebra with Clifford Algebra. The editor was Harley Flanders, who was the first to publish (in 1964) a book in English on differential forms and their applications. I was surprised and dismayed when he rejected my article, saying that it contained nothing new. I concluded that it was not worth my time to try publishing in mathematics journals. My article was not wasted, however. Fifteen years later it became the first chapter in my book on classical mechanics.

In 1973 I learned that philosopher-physicist Mario Bunge was editor for a series of advanced monographs in mathematical physics published by D. Reidel. When I submitted my three papers with Sobczyk with a proposal for a book, he responded so enthusiastically that I told him I had much more along the same lines, and he gave me a strong green light to proceed. Thus began the long process of producing the book *Clifford Algebra to Geometric Calculus* [44]. I had no idea that it would take more than 10 years. Let me mention a few landmarks of its involuted odyssey.

Most of the book was written between 1973 and the end of 1976. Among other things, I introduced the concept *vector manifold* as a new foundation for geometric calculus and coordinate-free differential geometry, integrated differential forms into a more comprehensive theory of *directed integrals*, and developed a new approach to *differentials* and *codifferentials* for mappings and fields.

I submitted the completed manuscript directly to the publisher ready to be typeset and published. I was shocked when the manuscript was rejected several months later after being sent out to an independent reviewer. Bunge explained that all would have gone smoothly if I had just submitted the manuscript through him. I was annoyed but not discouraged by this outcome, as I was absolutely confident in the quality of my work. Besides I had just decided to add a chapter on incorporating Lie groups and Lie algebras into GA, thus greatly increasing its scope. I had developed the material in trying to help a student with a dissertation on the subject. But he was a man in a hurry and switched to another advisor to get his Ph.D. more quickly. At least I was pleased with the work he induced me to do myself. A decade later we got together again to collaborate in a different field—neural networks.

My problem of finding another publisher for the book was soon solved when I was contacted by the eminent mathematician Gian Carlo Rota about procuring a copy of my STA book. It happened that I was attending the first of many MaxEnt conferences at MIT in 1978, so I dropped in to see Rota in person. When I asked why he was so interested in STA he handed me some of his own papers on determinants and invariant theory. As I read them the next day, I saw immediately that the ideas and results articulated perfectly with GC, so within a week I had made the final improvements in the manuscript.

Rota also agreed to consider the book for publication in the Addison-Wesley *Encyclopedia of Mathematics* series for which he was editor. I expected another long delay when he asked for six copies of the manuscript. After 2 years I started asking for a yes or no decision. I kept sending requests to Rota for another year without response. Finally, one morning I got the most gratifying phone call of my life. It was Rota, who apologized profusely and explained that he had been recuperating from serious eye operations for a year, but he had recommended the book for publication before that. Then he praised the book in great detail for the better part of an hour, and finished with an offer to write a foreword for it.

Alas! Addison-Wesley had hired a new editor for their Advanced Books program, and, unaccountably, she sent my book out for another round of review, despite the strong endorsement from Rota. She was replaced before she finished, but the next editor introduced a new obstacle. He refused to communicate with me altogether, even to report on the status of my book. After 3 years with this new series of delays, I contacted Rota once again in frustration. He inquired and reported that the editor seemed to have some grudge against me. When I learned his name, I understood. He was the same man who had rejected my book years before when he was editor for Reidel. Immediately, I went back to Reidel, and the new editor there, Alwyn Van der Merwe, got the book reviewed and published within six months [44]!

By that time I had another book to publish, and Van der Merwe accepted it immediately. Here is its history. Teaching the two semester Electrodynamics course for many years in succession afforded me the opportunity and the impetus to rework the entire subject in with spacetime algebra and geometric calculus. I was so pleased with the details that I planned to write a book on the subject. But before I could get started, the course was passed on to a colleague who was eager to teach it. With some reluctance, I then agreed to teach the graduate Classical Mechanics course, because no one else was interested. Soon after I got started I realized, to my surprise, that reformulation with GA had as much to offer nonrelativistic mechanics as it did for relativistic electrodynamics. Indeed, GA enabled a new spinor approach to rotations and rigid body dynamics that eliminates the need for scalar coordinates and matrices, and substantially reduces the conceptual gap between classical and quantum mechanics. I became convinced that reformulation of physics with GA should begin with mechanics.

From 1974–1978 I taught a year-long course in classical particle and continuum mechanics that enabled me to work out the details. Then, during my 1980–1981 sabbatical as a NASA Faculty Fellow at JPL I extended the treatment to space physics and Celestial Mechanics. That was greatly enhanced when, stimulated by a visit from Pertti Lounesto in 1983, I worked out a powerful new spinor approach to the two body problem and orbital perturbations [43]. This has since been applied to complex perturbations in the solar system by Jan Vrbik [59], with impressive success. Every physicist in the field should know about that work, but, sadly, few do. Finally, as if on cue, Van der Merwe appeared to facilitate publication as a book *New Foundations for Classical Mechanics* [32].

I must take this opportunity to once again praise the service of my wife Nancy in preparing illustrations for the book, which I believe are among the most meticulous and extensive in the literature. Every diagram and label had to assembled by hand. In this day of easy desktop publishing, young people have no idea what painstaking effort that required.

Though relativistic quantum mechanics with STA was the main line of my research during my first two decades as ASU, I never asked to teach quantum mechanics, because that was a bastion of physics orthodoxy among my colleagues. Many, I believe, were contemptuous of my work behind my back, though none ever asked me a question about it. At least they respected the fact that I had no trouble getting published or attracting good students. When I went up for promotion to full professor in 1976, the department chair concluded that no colleagues were qualified to assess my work, so he contacted the editor of the *Journal of Mathematical Physics* for the name of a referee of my published papers. As he confided to me later in confidence, he received the most glowing report he had ever seen, so my promotion was approved with ease. Oh, how much our lives are influenced by people we never know!

9. Spreading the Word

Interest in STA began to pick up about 1980, when Dirac invited me to give a colloquium on *Real Dirac Theory* at Florida State University where he was living out his retirement. His associate, Leopold Halpern, confided that Dirac had given me his greatest compliment: mine was the first colloquium in 10 years when he stayed awake the whole time. Afterwards, in my conversation with him, Dirac agreed to write a letter of support for an NSF research proposal of mine. Of course, that letter was so rare and precious that I sequestered it for safe keeping. Consequently, I am unable to locate it right now, so I must paraphrase. It was characteristically pithy—one sentence long: “I think there is something to this Real Dirac Theory.” Are you surprised that the reviewers dismissed his opinion as inconsequential when they rejected my proposal? What gives me pause is that Dirac was 4 years younger than I am today.

During the decade after 1980, interest in spacetime algebra and Clifford algebra grew rapidly and shared research programs in physics and mathematics. I attribute this largely to efforts of Jaime Keller, Roy Chisholm and Pertti Lounesto. Each played the critical social role of *connector* (as described by Malcolm Gladwell [15]) serving to promote the spread of ideas.

Keller and Chisholm organized the very first conferences and conference proceedings on applications of Clifford Algebras, and both were motivated to invite me as Keynote Speaker by my STA book. Due recognition of both has been accorded in a recent Memorial to Keller [51].

Chisholm’s conference [7] was held in 1985 at the beautiful University of Kent campus, high on a hill overlooking the magnificent Canterbury cathedral. It was exceptionally long (12 days), well-funded and well-organized with high quality. Chisholm had scoured the literature in mathematics and physics to identify leading experts on Clifford Algebra and convince them with personal invitations to attend.

With my new book to support strong claims [44], here at last was the perfect opportunity for me to trumpet *universality of Geometric Algebra* as a unified language for mathematics and physics. I was surprised by the weak response to my address. I like to think that some listeners were just stunned by what they heard. More likely, though, they needed time to contemplate details in the written version a year later [33], though I suppose not many looked back. I cannot make a better case today. So I am pleased to report that it did have the intended effect on sophisticated readers a few years later. See below.

Chisholm's conference served as a paradigm for many more conferences to follow. Indeed, before it was over the next conference was already scheduled for Montpellier, France in 1989. That was followed by conferences near Gent in 1993 and at Aachen in 1996. More conferences followed at regular intervals to this day along with many offshoots on special topics.

Keller's conference *Matemáticas del epacio-tiempo* was held at the University of Mexico in 1981, and Chisholm must have known about it because he invited Keller to his. It did not have the impact of Chisholm's, except on me, as it was my first invitation as invited speaker. So I prepared a major paper on spinor calculation of scattering amplitudes in "Real Dirac Theory." Understandably, the response was disappointing, but Keller must have sympathized, because he took steps to rectify it 10 years later.

In 1991 Keller founded the journal *Advances in Applied Clifford Algebras* and republished my paper [29] in the first issue. Keller served as editor until his death in 2011, and successfully shepherded the journal to achieve international recognition, taking care to maintain its interdisciplinary character, especially to foster interaction between physics and mathematics. The Journal provided a crucial venue for publishing work in emerging disciplines involving Clifford algebra. Together with the regular conferences it helped define those disciplines and give them cohesion.

Pertti Lounesto's service as a *connector* was quite different. He studied significant publications on Clifford algebra with great care, then invited himself to visit many of the authors to discuss their work in person. On two occasions he traveled all the way from Finland to stay with me in Arizona for up to a month. He came prepared to discuss and argue about details in my books and papers. He pointed out many deficiencies and even mistakes in my arguments, though nothing, I am happy to say, that was not easily corrected. He persisted in this throughout the years with a kind of brutal honesty. It was difficult not to feel some resentment at being singled out for such persistent nitpicking. Eventually I learned that I was not alone in this. He did it to everyone. He even set up a web site with a huge list of mistakes by many authors, some very prominent. I confess some relief in seeing that my offences did not stand out as particularly egregious. I recognized all this as an expression of Pertti's uncompromising search for mathematical truth and respect for the work of others. Indeed, Pertti acquired a well-deserved reputation as the (*conscience*) *policeman of the Clifford algebra community!*

Pertti first contacted me about STA and Marcel Riesz in 1979, when he was finishing up his doctoral thesis. His first extended visit to Arizona in 1982 was especially productive. From my studies in celestial mechanics I knew about the celebrated Kustaanheimo–Stiefel regularization of the Newtonian two body problem, and I suspected it could be significantly simplified with GA. As Kustaanheimo was Finnish and Pertti knew him personally, I suggested that we work together on that. It turned out to be surprisingly easy. I set us up in separate offices at ASU so we could work independently and then compare results. I had the problem completely solved and written up in just two days while he had nothing. Of course, I had a huge advantage with all my background in classical mechanics. He spent a whole day meticulously studying the manuscript and proposing changes, which we then argued about at length. The net result was that he convinced me to change only a single sentence. I submitted the paper for publication immediately, and it was in print within a year [43]. It was a paper we could be proud of. I was comfortable including Pertti as coauthor, because I would never have written it without the stimulus of his visit. But I suspect Pertti was privately embarrassed by the whole incident, as he never spoke of it again, and I don't believe he ever listed the paper among his publications.

10. Influence of Marcel Riesz (1886–1969)

Lounesto provided a great service to mathematicians by informing them about Marcel Riesz's great 1958 lecture notes on Clifford Algebra, which could easily have been lost because they were not formally published. Though he learned about the notes from my STA book where I had incorporated and extended the main ideas, Lounesto, as always, sought access to original sources. He then became the main messenger informing other mathematicians about Riesz's lectures, which had, in fact, been dismissed with little interest in *Mathematical Reviews* [58]. In 1985 at Kent he met E. Folke Bolinder, who had heard Riesz's lectures in person, and together they got the notes published [54].

Actually, Riesz's most ambitious work with Clifford algebra is in a massive paper on integral operators and the Cauchy problem [52], which has been republished in his collected papers [53]. It should be classified, today, as a contribution to "Clifford Analysis," though it predates that field by decades. Indeed, I rank it as one of the most significant contributions to field, though, as far as I know, no one in the field has even referenced it. I hope this remark will induce someone to take a look. All that aside, I believe that Riesz's main impact on mathematics will come implicitly through his contribution to the evolution of geometric algebra.

Eventually, Lounesto followed publication of Riesz's lectures with his own book on *Clifford Algebras and Spinors* [48]. He thoroughly develops the geometric approach to Clifford Algebras, spinors, idempotents and ideals with a good survey of the literature and many examples. Among many other good ideas, he does me the honor of implicitly adopting and explicitly recounting many details from my work. In particular, he gives an accurate account of my operator approach to spinors, which, as we have seen, dispenses with ideals in Dirac theory. Unaccountably, he blunders in attributing the source of my idea to Paul Kustaanheimo, whom I had never heard of until I studied celestial mechanics. I should add that he does not appreciate the "spacetime split," so his treatment of electrodynamics is unnecessarily clumsy, which left him unprepared for the coming revolution brought on by the "Conformal Split." Moreover, like the rest of the mathematics community, he fails to integrate CA with differential forms. Lounesto's book has not had the impact it deserves in the mathematics community. Though often cited, it is seldom appreciated.

11. Culmination of Spacetime Algebra

I spent the fall of my 1987–1988 sabbatical year at Boston University pursuing my new interest in neural network modeling [34] at Steven Grossberg's *Center for Neural Systems*. My spring was spent at Kent University hosted by Chisholm. This set initial conditions for a momentous confluence of world lines in the summer.

I got to know Cambridge Astronomer Steve Gull from many encounters at summer conferences on *Maximum Entropy and Bayesian Methods*, which, led by E. T. Jaynes, attracted outstanding researchers on information theory and statistics in disciplines ranging from physics to economics and artificial intelligence. Steve himself had already started a revolution in astronomy data analysis that continues to this day, and he was a dominant voice in all discussions and disputes at the MaxEnt conferences. He had arranged to hold the conference at Cambridge that summer.

Steve happened to be sitting next to me at the conference banquet when a mysterious note was delivered to me on a plate. When Steve asked me what it was about I rebuffed him saying he wouldn't be interested. He kept pestering me as we walked out until I revealed that the note came from Anthony Lasenby. That just piqued his interest even more, but I kept the note confidential.

Before I left on my sabbatical, an astronomer colleague, David Burstein, gave me Lasenby's name as someone at Cambridge interested in my spacetime algebra. So I looked up Lasenby when I arrived at Cambridge that summer. He told me the full story about his encounter with Burstein when we met.

Lasenby was intrigued by my book *Space Time Algebra*, after a copy was given to him by a retiring colleague. Burstein was visiting the famous Cambridge Institute of Astronomy when they met at tea, and the conversation went something like this: "I see you are from Arizona State University, then you must know David Hestenes." Surprised that anyone at Cambridge would know my name, Burstein responded "Yes, why do you ask?" Lasenby answered, "Because he wrote the great little book *Space Time Algebra*." Whereupon Burstein exclaimed, "You mean it's good!"

When I met Lasenby I told him about what I had been doing since the STA was published and gave him my two papers from the Chisholm conference, which surveyed the aims and means of my two pronged research program in geometric calculus [33] and quantum mechanics [31]. My generalization of Cauchy's integral formula grabbed Lasenby's attention immediately, because he was teaching complex variable theory at the time.

Soon after I returned to Arizona I got a postcard from Steve Gull with just one word inscribed: WOW!

Steve had buttonholed Anthony the next day and they dug into my papers immediately. They were both astronomers and close colleagues. But they were not like my astronomer colleagues, who couldn't care less about the niceties of mathematical physics. They were radio astronomers. And I learned from them that radio astronomers are physicists first and astronomers second. Indeed, they were theoretical physicists—"bloody theoreticians," as Martin Ryle, the father of radio astronomy respectfully called them.

Steve and Anthony enlisted graduate student Chris Doran, and the three of them dug into Geometric Algebra with unprecedented fervor that generated scores of papers and a steady stream of dissertations by graduate students at Cavendish Laboratory—I call that period 1988–1998 "the Cambridge decade." It was capped by publication of their epochal *Gauge Theory Gravity* [46], to my mind, one of the most profound contributions to *General Relativity* ever.

That brought the *STA phase* in GA development full circle. I have reviewed the key ideas and mathematical details for the whole phase in a coherent series of three papers: [36–38] Doran and Lasenby give a more complete account in their book *Geometric Algebra for Physicists* [12].

A spectacular new phase in GA development began suddenly with the introduction of *Conformal Geometric Algebra* (CGA) [35] at the GA conference in Ixtapa Mexico. Call it the *CGA phase*. CGA enables direct (coordinate-free) algebraic manipulation of geometric objects like lines, spheres and planes. It has many applications in engineering and computer science, especially in robotics and computer graphics. From the beginning, development of such applications has been led by Hongbo Li, Leo Dorst, Joan Lasenby and Eduardo Bayro-Corrochano.

This is a good place for me to sign off my historical remarks with reference to a broader survey [41]. Up to the new millenium I believe I knew about every published paper on GA. Since that time, GA and CGA have so proliferated that I often encounter names of authors I have never heard of. I take that as evidence that GA will continue to flourish without my input. Satisfying closure on a lifelong endeavor!

Let me conclude with some observations on the sociology of geometric algebra.

Developments in CGA have been completely overlooked by mathematicians. An explanation for this can be found in the conceptual difference that underlies the superficially synonymous terms 'Geometric Algebra' and 'Clifford Algebra.'

Algebraists these days, especially in France, mostly follow Chevalley [6] in defining CA using an ideal in tensor algebra associated with a quadratic form. While that may have certain technical advantages, it neglects geometric meaning that provides a foundation for rich mathematical structures. To strict algebraists, CA is just "the algebra of a quadratic form,"—nothing special, just one more algebra among many others.

In contrast, the geometric approach assigns meaning to the notion of vector as a directed number by defining an associative product

$$\mathbf{ab} = \mathbf{a} \cdot \mathbf{b} + \mathbf{a} \wedge \mathbf{b}, \tag{20}$$

where

$$\mathbf{a} \cdot \mathbf{b} = \tfrac{1}{2}(\mathbf{ab} + \mathbf{ba}) \quad \text{and} \quad \mathbf{a} \wedge \mathbf{b} = \tfrac{1}{2}(\mathbf{ab} - \mathbf{ba}). \tag{21}$$

These equations (actually axioms) provide the essential grammar on which the language of Geometric Algebra is grounded. Contrary to a mistaken assertion that has become all too common recently, *Clifford never wrote down these equations!* Marcel Riesz was the first to do that, though with a different notation [54]. And Riesz did not emphasize their geometric interpretation. That emerged with the development of *Geometric Algebra* as a language for physics, as we have seen.

"By their fruits ye shall know them." Mathematicians adhering to the strict algebraic conception of CA have completely missed the rich geometric implications of GA, in other words, algebra that flows from geometry. Besides CGA, they missed the incorporation of Lie groups and Lie algebras into GA, including versor representations of the classical groups [13] and the exceptional groups [9].

In contrast to algebraists, specialists in functional analysis, most notably Marcel Riesz [52], have been led naturally (but tacitly) to the geometric interpretation of CA. Accordingly, it is in analysis, specifically in the emerging field of *Clifford Analysis*, where the unique power of CA has been most evident and the most mathematical progress has been made.

12. Geometric Algebra for Mathematicians

I have bemoaned the gap between mathematics and physics that grew up in the last century. As antidote I recommend a strong dose of *Geometric Algebra* or, if you will, *Geometric Calculus*, depending on which aspects of the subject you wish to emphasize. One great benefit is that GA unifies and simplifies all aspects of fundamental physics, thereby facilitating access by mathematicians. A complementary benefit is recognition that it has already unified much of mathematics and has potential for much more.

A good place to start treatment is with my friends in *Clifford Analysis*, as they are closer to physics than most other branches of mathematics. Over the years (decades, really) I have tried to convince them that *their field needs a makeover* in the way it is advertised to physicists and to other mathematicians. They must strengthen and repackage their claims, if they are to elevate Clifford Analysis from a struggling, minor sub-specialty to a major branch of mainstream mathematics. Here, I argue the case for the last time. First, we need to be clear on what is at stake.

As we have seen, interest in both Geometric Calculus and Clifford Analysis expanded rapidly in the 1980s through common workshops and publication channels. Still, during a coffee break at one CA workshop in the 1990s, I heard one participant refer to the workshop as a kind of “ghetto.” That was not necessarily a pejorative comment about the quality of participants and their work, but instead a remark on their isolation from mainstream mathematics. The notion of an “*ecological niche*” is a better metaphor, I think. In academic circles niches are essential for incubation of new ideas, for protection against skepticism and competing ideas until they are mature enough to stand on their own. Workshops on specialized subjects serve as one kind of niche, but they cannot support sustained incubation.

Richard Delange is rightfully regarded as the father of Clifford Analysis [5]. He has done much more than create foundations and prove the central theorem for the subject. At the Belgium University of Gent, he has created and sustained a warm academic niche to cultivate the subject and train new talent. Led by his talented students, most notably Frank Sommen, “Delange’s Belgium school” has produced a steady stream of quality mathematics for decades.

The scope and history of Clifford Analysis is comprehensively reviewed in [10] and, more recently, by Delange himself in [11]. Delange modestly notes that his seminal discovery was more or less duplicated independently by others, but that does not detract from his accomplishment. Multiple discoveries are more common than singletons, especially when they are important [49]. Moreover, the research program he established to explore implications of the discovery counts for at least as much. As John Wheeler responded when asked if publishing first is sufficient to establish scientific priority, “No, you have to convince your colleagues!”

It is worth noting that a self-contained special case of Clifford Analysis called “Quaternion Analysis” was created independently [57] and is still presented as an independent subject today [14]. The fine reviews of the subject just cited make the subtle mistake, common to most quaternion applications, of failing to discriminate between vectors and bivectors. However, that is easily remedied with GA [37] in a way that makes the results and techniques immediately useful to physicists and engineers.

I submit that Clifford Analysis should be advertised as *the* most natural and straightforward generalization of classical functional analysis, including and generalizing *all* special functions so crucial to practical applications in physics and engineering. Practitioners may believe this, but the case must be argued to convince others. Unfortunately, typical presentations of the subject, indeed, its very name suggests a minor specialty with esoteric concerns of dubious practical value. I see little effort to connect with the great classics like Whittaker and Watson [60] that have been bread and butter for physicists. I suppose that the name *Clifford Analysis* is an established tradition by now, but let me suggest the descriptive *Geometric Function Theory* (GFT) as a synonym, at least. The name GFT is already in use in the AMS mathematical classification system to designate a branch of complex analysis, but Clifford Analysis is legitimately regarded as a generalization of that branch, indeed, a generalization by incorporating more geometry, as is abundantly evident when regarding it as a branch of Geometric Calculus. The makeover of Clifford Analysis necessary to identify it with GFT as a branch of GC is fairly simple to implement.

The essential first step in the makeover is recognizing that the so-called "Dirac operator" is nothing other than the *vector derivative*, so the name is a terrible misnomer and must be dispensed with. Indeed, all the fundamental concepts in mathematics should be designated by descriptive names, and there is no more fundamental concept in analysis than the "*derivative!*" We don't call the *scalar derivative* d/dx the "Leibniz operator!" The special vector derivatives *gradient, divergence and curl* are not named after Hamilton. So the general notion of a vector derivative should not be named after Dirac, who had even less to do with its invention than Hamilton. Of course, the vector derivative presupposes the algebraic concept of 'vector' as defined by GA, just as the scalar derivative presupposes an algebraic concept of scalar.

Once the essential role of GA in defining the concept of vector and vector derivative is recognized, the embedding of Clifford Analysis as GFT in GC is straightforward. Here are some of the advantages:

Integration with differential forms: The entire calculus of differential forms in GC is built around the vector derivative, so no distinct concept of "exterior derivative" is needed, and the integral theorems are manifestly complementary to the vector derivative.

Connection to physics and engineering: Facilitated by the smooth embedding of STA in GC.

Connection to differential geometry: Facilitated by constructing differential operators from the vector derivative.

Integration of complex analysis with real analysis: Facilitated by reduction to real variables (vectors and multivectors as well as scalars). Integrating analytic function theory of several complex variables into Clifford Analysis still faces problems [55]. GC can help.

New possibilities: For example, GA enables unique (possibly optimal) representation of the mixed quantum states that are fundamental to quantum computation [17]. Therein lie new opportunities for functional analysis.

The ultimate: Of course, the ultimate advantage (or goal, if you will) of integrating Clifford Analysis into Geometric Calculus is contributing to unification of mathematics.

13. Geometric Algebra for the Future

Geometric Algebra simplifies and unifies mathematics and physics at every level from the most elementary to the most advanced [37]. These facts are undeniable.

"But, if GA is so good," I have often been asked, "why is it not more widely used?" I can only reply, "Its time will come!"

The published GA literature is more than sufficient to support instruction with GA at intermediate and advanced levels in physics, mathematics, engineering and computer science. Though few faculty are conversant with GA now, most could easily learn what they need while teaching.

At the introductory level GA textbooks and teacher training will be necessary before GA can be widely taught in the schools. There is steady progress in this direction, but funding is needed to accelerate it.

Malcolm Gladwell [15] has discussed social conditions for a "tipping point" when the spread of an idea suddenly goes viral.

Place your bets now on a Tipping Point for Geometric Algebra!

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