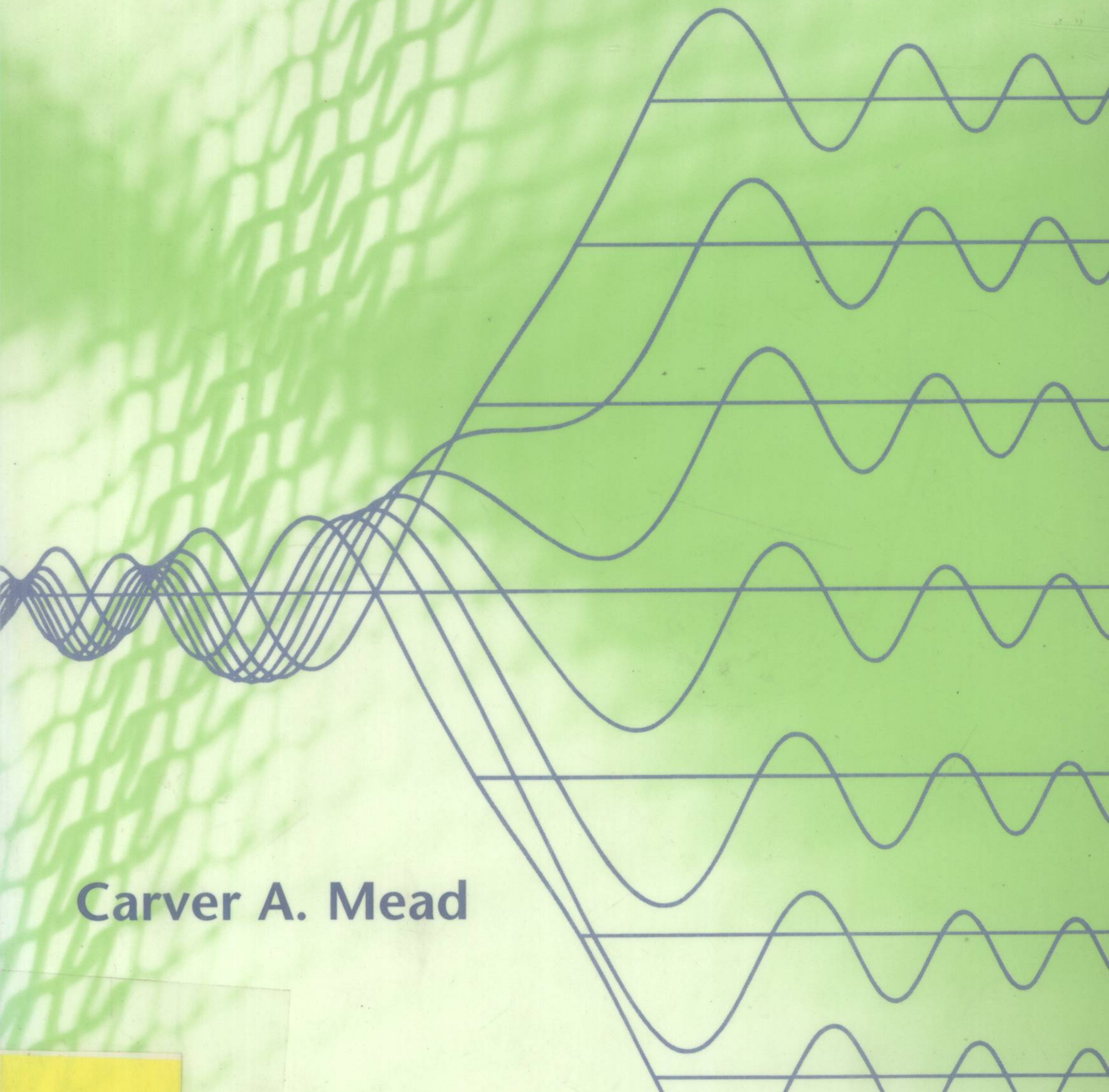
Collective Electrodynamics

Quantum Foundations of Electromagnetism



Carver A. Mead

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Quantum Foundations of Electromagnetism

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华南理工大学
图书馆藏书

E200301274

The MIT Press
Cambridge, Massachusetts
London, England

First MIT Press paperback edition, 2002
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This book was set in Computer Modern typeface (PostScript Type 1) by the TEX composition system.
Printed and bound in the United States of America.

Library of Congress Cataloging-in-Publication Data

Mead, Carver A.
Collective electrodynamics : quantum foundations of electromagnetism / Carver A. Mead.
p. cm.
Includes bibliographical references and index.
ISBN 0-262-13378-4 (hc : alk. paper), 0-262-63260-8 (pb)
1. Quantum electrodynamics. I. Title.

QC680.M43 2000
530.14′33—dc21

00-028180

Foreword

> The formulation of a problem is
> often more essential than its solution,
> which may be merely a matter of
> mathematical or experimental skill.
> To raise new questions, new possibilities,
> to regard old problems from a new angle,
> requires creative imagination and marks real advance in science.
>
> —A. Einstein and L. Infeld[1]

When I was a student, it was commonly understood that one would study a subject until one became an expert; then, one would go out into the world and apply that expertise in one's profession. It went without saying that the expertise itself, as updated through one's experience, would allow the practice of that profession until retirement. The tacit assumption involved in that world view was that the knowledge base evolves slowly, an assumption then already losing validity. Today, we face an explosive growth of knowledge; by any measure, our knowledge base is doubling every few years. How do we, as a human culture, prepare ourselves and our children for this world in which the knowledge base turns over many times within a single human lifetime?

One answer to this dilemma is specialization: One can become an expert in a specialty that is narrow enough to permit one to keep up with the changes as they come along. This is the default solution. In this manner, we can, as it has been said, learn more and more about less and less, until eventually, we know everything about nothing! Specialization, as we all know, has its merits; however, if specialization were to be our only response to rapidly evolving knowledge, I would view our prospects as a culture with deep concern, even with alarm.

In his wonderful book, *The Act of Creation*, Arthur Koestler (3) defines the creative process as starting with the juxtaposition

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[1]This quotation appears on page 95 of the popular book, *The Evolution of Physics* (1). This book has recently been reprinted (2); the quotation appears on page 92 of the new version.

of two concepts from separate conceptual spaces. Such a conjunction creates not merely a new idea but an enlargement of the space of ideas, a cross-fertilization that is the very stuff of which innovation is made. If we, by education, by scientific practices, by social norms, restrict the development of individual talents to narrow specializations, we will thereby lose the ability to innovate.

Fortunately, there is, within our culture, an evolution of knowledge over and above the addition of facts and the specialized understanding of those facts. Many phenomena that in the past were seen as separate are now understood to be the same: Fire is a chemical reaction, not a separate element; temperature is energy; light is electromagnetic radiation; molecules are aggregations of atoms; mechanical forces are electromagnetic in origin; ... Each of these equivalences represents a major *unification* and *simplification* of the knowledge base. Ideas formerly occupying separate conceptual spaces now occupy the same conceptual space. Each unification was made possible by a deeper understanding of existing facts, often triggered by the discovery of a crucial new fact.

It is this unification and simplification of knowledge that gives us hope for the future of our culture. To the extent that we encourage future generations to understand deeply, to see previously unseen connections, and to follow their conviction that such endeavors are noble undertakings of the human spirit, we will have contributed to a brighter future.

Remarks upon acceptance of the
1999 Lemelson–MIT Prize
April 22, 1999
San Francisco, California

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Personal Preface

> As for the search for truth,
> I know from my own painful
> searching, with its many blind alleys,
> how hard it is to take a reliable step, be it ever so small,
> toward the understanding of that which is truly significant.
>
> —Albert Einstein[1]

The material in this little volume has been for me a personal quest that I began nearly fifty years ago. It came about as a direct result of my interactions with Richard Feynman. He and I both arrived at Caltech in 1952—he as a new professor of physics, and I as a freshman undergraduate. My passionate interest was electronics, and I avidly consumed any material I could find on the subject: courses, seminars, books, etc. As a consequence, I was dragged through several versions of standard electromagnetic theory: $\vec{E}$ and $\vec{B}$, $\vec{D}$ and $\vec{H}$, curls of curls, the whole nine yards. The only bright light in the subject was the vector potential, to which I was always attracted because, somehow, it made sense to me. It seemed a shame that the courses I attended didn't make more use of it. In my junior year, I took a course in mathematical physics from Feynman—What a *treat*! This man could *think conceptually* about physics, not just regurgitate dry formalism. After one quarter of Feynman, the class was spoiled for any other professor. But when we looked at the registration form for the next quarter, we found Feynman listed as teaching high-energy physics, instead of our course. Bad luck! When our first class met, however, here came Feynman. "So you're not teaching high-energy physics?" I asked. "No," he replied, "low-energy mathematics." Feynman liked the vector potential, too; for him it was the link between electromagnetism and quantum mechanics. As he put it (5),

> In the general theory of quantum electrodynamics, one takes

[1]This quotation was taken from a letter written by Einstein in the year I was born. It appears on page 38 of the wonderful picture book *Essential Einstein* (4). This reference contains many historic photographs of Einstein, each accompanied by a quotation.

> the vector and scalar potentials as fundamental quantities in a set of equations that replace the Maxwell equations.

I learned enough about it from him to know that, some day, I wanted to do all of electromagnetic theory that way.

By 1960, I had completed a thesis on transistor physics and had become a brand-new faculty member in my own right. Fascinated by Leo Esaki's work on tunnel diodes, I started my own research on electron tunneling through thin insulating films. Tunneling is interesting because it is a purely quantum phenomenon. Electrons below the zero energy level in a vacuum, or in the forbidden gap of a semiconductor or insulator, have wave functions that die out exponentially with distance. I was working with insulators sufficiently thin that the wave function of electrons on one side had significant amplitude on the opposite side. The result was a current that decreased exponentially with the thickness of the insulator. From the results, I could work out how the exponential depended on energy. My results didn't fit with the conventional theory, which treated the insulator as though it were a vacuum. But the insulator was *not* a vacuum, and the calculations were giving us important information about how the wave function behaved in the forbidden gap. Feynman was enthusiastic about this tunneling work. We shared a graduate student, Karvel Thornber, who used Feynman's path integral methods to work out a more detailed model of the insulator.

In 1961, Feynman undertook the monumental task of developing a completely new two-year introductory physics course. The first year covered mechanics; although that topic wasn't of much interest to me, it would come up occasionally in our meetings on the tunneling project. When I heard that Feynman was going to do electromagnetic theory in the second year, I got very excited—finally, someone would get it right! Unfortunately, it was not to be. The following quotation from the forward to *Feynman Lectures on Gravitation* (6) tells the story:

> It is remarkable that concurrently with this course on gravitation, Feynman was also creating and teaching an innovative course in sophomore (second-year undergraduate) physics, a course that would become immortalized as the second and third volumes of *The Feynman Lectures on Physics*. Each Monday Feynman would give his sophomore lecture in the morning and the lecture on gravitation after lunch. Later in

> the week would follow a second sophomore lecture and a lecture for scientists at Hughes Research Laboratories in Malibu. Besides this teaching load and his own research, Feynman was also serving on a panel to review textbooks for the California State Board of Education, itself a consuming task, as is vividly recounted in *Surely You're Joking, Mr. Feynman*. Steven Frautschi, who attended the lectures as a young Caltech assistant professor, remembers Feynman later saying that he was "utterly exhausted" by the end of the 1962–63 academic year.

I was another young Caltech assistant professor who attended the gravitation lectures, and I remember them vividly. Bill Wagner (with whom I still communicate over collective electrodynamics material) took notes, and later worked out the mathematical presentation in the written version of the lectures. I also attended many of the sophomore lectures, to which I had mixed reactions. If you read Vol. II of *The Feynman Lectures on Physics* (5), you will find two distinct threads: The first is a perfectly standard treatment, like that in any introductory book on the subject. In his preface, Feynman says of this material:

> In the second year I was not so satisfied. In the first part of the course, dealing with electricity and magnetism, I couldn't think of any really unique or different way of doing it.

There is a second thread, however, of true vintage Feynman—the occasional lectures where he waxed eloquent about the vector potential. Section 15-5 contains a delightful discussion about what a field is and what makes one field more "real" than another.

> What we mean here by a "real" field is this: a real field is a mathematical function we use for avoiding the idea of action at a distance... A "real" field is then a set of numbers we specify in such a way that what happens *at a point* depends only on the numbers *at that point*... In our sense then, the **A**-field is "real" ... **E** and **B** are slowly disappearing from the modern expression of physical laws; they are being replaced by **A** and ϕ.

In Chapter 25, he develops the equations of electrodynamics in four-vector form—the approach that I have adopted in this monograph. I can remember feeling very angry with Feynman when I sat in on this particular lecture. Why hadn't he started this way in the first place, and saved us all the mess of a **B** field, which, as he told us himself, was not real anyway? When I asked him about it, he said something vague, like:

> There are a bunch of classical interactions that you can't get at in any simple way without Maxwell's equations. You need the $v \times \mathbf{B}$ term.

I don't remember his exact words here, only the gist of the discussion. Sure enough, when Vol. II of the lectures was published, the equation $F = q(\mathbf{E} + v \times \mathbf{B})$ in table 15-1 appears in the column labeled "True Always." The equation *is* true for the toy electric motor he shows in Fig. 16-1. It is *not* true in general. For a real electric motor, the **B** field is concentrated in the iron, rather than in the copper in which the current is flowing, and the equation gives the wrong answer by a factor of more than 100! That factor is due to the failure of **B** to be "real," precisely in Feynman's sense. Somehow he had separated science into two worlds: quantum and classical. For him, the vector potential was primary in the quantum world, whereas **E** and **B** were necessary for the classical world. These two worlds had not yet come together.

I was an active researcher in solid-state physics at that time, and I used the quantum nature of electrons in solids every day. Electrodynamics deals with how electrons interact with other electrons. The classical interactions Feynman was talking about were between electrons in metals, in which the density of electrons is so high that quantum interaction is by far the dominant effect. If we know how the vector potential comes into the phase of the electron wave function, and if the electron wave function dominates the behavior of metals, then why can't we do all of electromagnetic theory that way? Why didn't he use his knowledge of quantum electrodynamics to "take the vector and scalar potentials as fundamental quantities in a set of equations that replace the Maxwell equations," as he himself had said? I was mystified; his cryptic answer prodded me to start working on the problem. But every time I thought I had an approach, I got stuck.

Bill Fairbank from Stanford had given a seminar on quantized flux in superconducting rings that impressed me very much. The solid-state physics club was much smaller in those days, and, because I was working in electron tunneling, I was close to the people working on tunneling between superconductors. Their results were breaking in just this same time frame, and Feynman gave a lecture about this topic to the sophomores; it appears as Chapter 21 in Vol. III of *The Feynman Lectures on Physics* (7). As I listened to that lecture, my thoughts finally clicked: This is how we can make the connection! A superconductor is a quantum system on a classical scale, and that fact allows us to carry out Feynman's grand scheme. But I couldn't get this approach to go all the way through at that time, so it just sat in the back of my mind all these years, vaguely tickling me.

Meanwhile my work on tunneling was being recognized, and Gordon Moore (then at Fairchild) asked me whether tunneling would be a major limitation on how small we could make transistors in an integrated circuit. That question took me on a detour that was to last nearly 30 years, but it also led me into another collaboration with Feynman, this time on the subject of computation. Here's how it happened: In 1968, I was invited to give a talk at a workshop on semiconductor devices at Lake of the Ozarks. In those days, you could get everyone who was doing cutting-edge work into one room, so the workshops were where all the action was. I had been thinking about Gordon Moore's question, and decided to make it the subject of my talk. As I prepared for this event, I began to have serious doubts about my sanity. My calculations were telling me that, contrary to all the current lore in the field, we could scale down the technology such that *everything got better*. The circuits got more complex, they ran faster, and they took less power—WOW! That's a violation of Murphy's law that won't quit! But the more I looked at the problem, the more I was convinced that the result was correct, so I went ahead and gave the talk—to hell with Murphy! That talk provoked considerable debate, and at the time most people didn't believe the result. But by the time the next workshop rolled around, a number of other groups had worked through the problem for themselves, and we were pretty much all in agreement. The consequences of this result for modern information technology have, of course, been staggering.

Back in 1959, Feynman gave a lecture entitled "There's Plenty of Room at the Bottom," in which he discussed how much smaller things can be made than we ordinarily imagine. That talk had made a big impression on me; I thought about it often, and it would sometimes come up in our discussions on the tunneling work. When I told him about the scaling law for electronic devices, Feynman got jazzed. He came to my seminars on the subject, and always raised a storm of good questions and comments. I was working with a graduate student, Bruce Hoeneisen; by 1971, we had worked out the details of how transistors would look and work when they are a factor of 100 smaller in linear dimension than the limits set by the prevailing orthodoxy. Recently, I had occasion to revisit these questions, and to review the history of what has happened in the industry since those papers were published. I plotted our 1971 predictions alongside the real data; they have held up extremely well over 25 years, representing a factor of several thousand in density of integrated circuit components (8).

Because of the scaling work, I became completely absorbed with how the exponential increase in complexity of integrated circuits would change the way that we think about computing. The viewpoint of the computer industry at the time was an outgrowth of the industrial revolution; it was based on what was then called "the economy of scale." The thinking went this way: A 1000-horsepower engine costs only four times as much as a 100-horsepower engine. Therefore, the cost per horsepower becomes less as the engine is made larger. It is more cost effective to make a few large power plants than to make many small ones. Efficiency considerations favor the concentration of technology in a few large installations. The same must be true of computing. One company, IBM, was particularly successful following this strategy. The "Computing Center" was the order of the day—a central concentration of huge machines, with some bureaucrat "in charge" and plenty of people around to protect the machines from anyone who might want to use them. This model went well with the bureaucratic mindset of the time—a mindset that has not totally died out even today.

But as I looked at the physics of the emerging technology, it didn't work that way at all. The time required to move data is set by the velocity of light and related electromagnetic considerations, so it is far more effective to put whatever computing is required *where the data are located.* Efficiency considerations thus favor the distribution of technology, rather than the concentration of technology. The economics of information technology are the reverse of those of mechanical technology.

I gave numerous talks on this topic, but, at that time, what I had to say was contrary to what the industry wanted to hear. The story is best told in George Gilder's book, *Microcosm* (9).

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Feynman had started this line of thought already in his 1959 lecture, and we had a strong agreement on the general direction things were headed. He often came to my group meetings, and we had lively discussions on how to build a machine that would recognize fingerprints, how to organize many thousand little computers so they would be more efficient than one big computer, etc. Those discussions inevitably led us to wonder about the most distributed computer of all: the human brain. Years before, Feynman had dabbled in biology, and I had worked with Max Delbrück on the physics of the nerve membrane, so I knew a bit about nervous tissue. John Hopfield had delved much deeper than either Feynman or I had; and, by 1982, he had a simple model—a caricature of how computation might occur in the brain.

The three of us decided to offer a course jointly, called "Physics of Computation." The first year, Feynman was battling a bout with cancer, so John and I had to go it alone. We alternated lectures, looking at the topic from markedly different points of view. Once Feynman rejoined us, we had even more fun—three totally different streams of consciousness in one course. The three of us had a blast, and learned a lot from one another, but many of the students were completely mystified. After the third year, we decided, in deference to the students, that there was enough material for three courses, each with a more-unified theme. Hopfield taught "Neural Networks," Feynman taught "Quantum Computing," which ended up in the first volume of *Feynman Lectures on Computation* (10), and I taught "Neuromorphic Systems," which ended up in my book on the subject (11).

There is a vast mythology about Feynman, much of which is misleading. He had a sensitive side that he didn't show often. Over lunch one time, I told him how much he had meant to me in my student years, and how I would not have gone into science had it not been for his influence. He looked embarrassed, and abruptly changed the subject; but he heard me, and that was what was important. In those days, physics was an openly combative subject—the one who blinked first lost the argument. Bohr had won his debate with Einstein that way, and the entire field adopted the style. Feynman learned the game well—he never blinked. For this reason, he would never tell anyone when he was working on something, but instead would spring it, preferably in front of an audience, after he had it all worked out. The only way that you could tell what he cared about was to notice what topics made him mad when you brought them up.

If Feynman was stuck about something, he had a wonderful way of throwing up a smoke screen; we used to call it "proof by intimidation." There is a good example in Vol. II of the *Lectures on Physics* (5), directly related to collective electrodynamics. Section 17-8 contains the following comment:

> we would expect that corresponding to the mechanical momentum $p = mv$, whose rate of change is the applied force, there should be an analogous quantity equal to $\mathcal{L}\mathbf{I}$, whose rate of change is $\mathcal{V}$. We have no right, of course, to say that $\mathcal{L}\mathbf{I}$ is the real momentum of the circuit; in fact it isn't. The whole circuit may be standing still and have no momentum.

Now, this passage does not mean that Feynman was ignorant of the fact that the electrical current $\mathbf{I}$ is made up of moving electrons, that these moving electrons have momentum, and that the momentum of the electrons does not correspond to the whole circuit moving in space. But the relations are not as simple as we might expect, and they do not correspond in the most direct way to our expectations from classical mechanics. It is exactly this point that prevented me, over all these years, from seeing how to do electrodynamics without Maxwell's equations. Feynman was perfectly aware that this was a sticking point, and he made sure that nobody asked any questions about it. There is a related comment in Vol. III of the *Lectures on Physics* (7), Section 21-3:

> It looks as though we have two suggestions for relations of velocity to momentum... The two possibilities differ by the vector potential. One of them... is the momentum obtained by multiplying the mass by velocity. The other is a more mathematical, more abstract momentum

When Feynman said that a concept was "more mathematical" or "more abstract," he was not paying it a compliment! He had no use for theory devoid of physical content. In the *Lectures on Gravitation*, he says:

> If there is something very slightly wrong in our definition of the theories, then the full mathematical rigor may convert these errors into ridiculous conclusions.

We called that "carrying rigor to the point of *rigor mortis*." At another point, he is even more explicit:

> it is the facts that matter, and not the proofs. Physics can progress without the proofs, but we can't go on without the facts... if the facts are right, then the proofs are a matter of playing around with the algebra correctly.

He opened a seminar one time with the statement, "Einstein was a giant." A hush fell over the audience. We all sat, expectantly, waiting for him to elaborate. Finally, he continued, "His head was in the clouds, but his feet were on the ground." We all chuckled, and again we waited. After another long silence, he concluded, "But those of us who are not that tall have to choose!" Amid the laughter, you could see that not only a good joke, but also a deep point, had been made.

Experiments are the ground on which physics must keep its feet—as Feynman knew well. When any of us had a new result, he was all ears. He would talk about it, ask questions, brainstorm. That was the only situation in which I ever personally interacted with him without his combative behavior getting in the way. Down deep, he always wanted to do experiments himself. A hilarious account of how he was "cured" of this craving appears in *Surely You're Joking, Mr. Feynman.* In the end, he had his wish. In 1986, he was asked to join the Rodgers commission to investigate the Challenger disaster. After talking to the technical people, who knew perfectly well what the problem was and had tried to postpone the launch, he was able to devise an experiment that he carried out on national, prime-time TV. In true Feynman style, he sprang it full-blown, with no warning! In his personal appendix to the commission report, he concluded, "For a successful technology, reality must take precedence over public relations, for nature cannot be fooled." The day after the report was released was Caltech's graduation. As we marched together in the faculty procession, "Did you see the headline this morning?" he asked. "No," I replied. "What did it say?" "It said **FEYNMAN ISSUES REPORT**." He paused, and then continued with great glee. "Not **Caltech Professor Issues Report**, not **Commission Member Issues Report**, but **FEYNMAN ISSUES REPORT**." He was a household word, known and revered by all people everywhere who loved truth. His own public relations were all about reality, and were, therefore, okay.

In 1987, one year later, his cancer came back with a vengeance, and he died in February, 1988. Al Hibbs, a former student, colleague, and friend of Feynman's, organized a wake in grand style: bongo drums, news clips, interviews, and testimonials. It was deeply moving—we celebrated the life of this man who had, over the years, come to symbolize not just the spirit of Caltech, but the spirit of science itself. This man had engendered the most intense emotions I have ever felt—love, hate, admiration, anger, jealousy, and, above all, a longing to share and an intense frustration that he would not. As I walked away from Feynman's wake, I felt intensely alone. He was the man who had taught me not only what physics is, but also what science is all about, what it means to *really understand.* He was the only person with whom I could have talked about doing electromagnetism without Maxwell's equations—using the quantum nature of matter as the sole basis. He was the only one who would have understood why it was important. He was the only one who could have related to this dream that I had carried for 25 years. This dream came directly from Feynman, from what he said and from what he scrupulously avoided saying, from the crystal-clear insights he had, and from the topics that had made him mad when I brought them up. But now he was gone. I would have to go it alone. I sobbed myself to sleep that night, but I never shared those feelings with anyone. I learned *that* from him, too.

In 1994, I was invited to give the keynote talk at the Physics of Computation conference. That invitation gave me the kickstart I needed to get going. By the next year, I had made enough progress to ask Caltech for a year relief from teaching so I could concentrate on the new research. In June 1997, the six graduate students working in my lab all received their doctoral degrees, and, for the first time since I joined the faculty, I was a free man. I finished the basic paper on Collective Electrodynamics (12), an expanded version of which appears in the present monograph as Part 1 (p. 9). The memorial volume, *Feynman and Computation* (13), contains reprints of this paper and the scaling paper mentioned previously, along with an earlier version of this preface entitled *Feynman as a Colleague.*

By the end of 1998, I had developed the subject to the point where most of the standard problems in electromagnetic theory could be understood much more easily using this approach than by using standard textbook methods. Early in 1999, I was notified that I had been chosen to receive the prestigious Lemelson–MIT award for innovation. The ceremony celebrating this award was a gala event at which MIT chairman Alex d'Arbeloff stressed the importance of preparing the students of today to be the innovators of tomorrow. He expressed concern that neither our scientific establishment nor our educational institutions have developed approaches that are adequate to meet this challenge. At that moment, I realized that the work I had been doing was an example of precisely what was needed—the simplification and unification of knowledge. The remarks I made upon receiving the award appear in the foreword to this monograph.

In the end, science is all in how you look at things. Collective Electrodynamics is a way of looking at the way that electrons interact. It is a much simpler way than Maxwell's, because it is based on experiments that tell us about the electrons directly. Maxwell had no access to these experiments. The sticking point I mentioned earlier is resolved in this treatment, in a way that Feynman would have liked. This monograph is dedicated to him in the most sincere way I know: It opens with my favorite quotation, the quotation that defines, for me, what science is all about. In his epilogue, Feynman tells us his true motivation for giving the *Lectures on Physics*:

> I wanted most to give you some appreciation of the wonderful world, and the physicist's way of looking at it, which, I believe, is a major part of the true culture of modern times... Perhaps you will not only have some appreciation of this culture; it is even possible that you may want to join in the greatest adventure that the human mind has ever begun.

You succeeded, Dick, and we have—Thanks!

Acknowledgments

I am indebted to many colleagues who have discussed, argued, and shared many insights with me during the past eight years. Dick Lyon and Sanjoy Mahajan have read and marked more versions of the manuscript than any of us can remember and have provided many of the historic references. Bill Bridges has provided many insights and suggestions from the point of view of electrical engineering practice, as has Bill McClellan. Discussions with David Feinstein started me on the most recent active phase of the endeavor. David Goodstein, Martin Perl, Yaser Abu-Mostafa, Demetri Psaltis, Paul Klein, Paul Tuinenga, Dick Neville, Glen Keller, Mike Godfrey, Jim Eisenstein, Bill Wagner, Christoph Von Der Malsburg, Terry Sejnowski, Rahul Sarpeshkar, Tobi Delbrück, Nick Mascarenhas, Al Barr, Axel Sherer, and others have read and responded to various parts of the manuscript at various stages. Sunit Mahajan and I shared many long hours of quantum-Hall measurements. Hideo Mabuchi and Jeff Kimble shared their splendid experiments and insights about atoms in cavities. Cal Jackson, who has provided the TEXpertise throughout the effort, did the final design, page layout, and typesetting. Bob Turring provided the figures. Lyn Dupré and Dian De Sha edited the manuscript at many stages. Donna Fox provided encouragement as well as administrative support. John Seinfeld, chairman of the division of engineering and applied science at Caltech, has been wonderfully supportive over the past few years. Barbara Smith has been a boundless source of energy, wisdom, and caring.

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