biological and mineralogical world but rather to explore it. In combining chemical, mineralogical, and geological approaches, as exemplified by his opening the door to our understanding the importance of diffusion processes in compacting sediments undergoing early diagenesis, he continues to show us how some very complicated natural processes can be abstracted to simplified models that can be treated in an analytical fashion. Most recently we have seen a full exposition of this approach in his new book, "Chemical Sedimentology" which I suspect will be mined by workers in this field for a long time to come.

Mr. President, ladies and gentlemen, I have the honor and great personal pleasure in presenting to you Robert A. Berner for the Mineralogical Society Award for 1971.

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ACCEPTANCE OF THE MINERALOGICAL SOCIETY OF AMERICA AWARD FOR 1971

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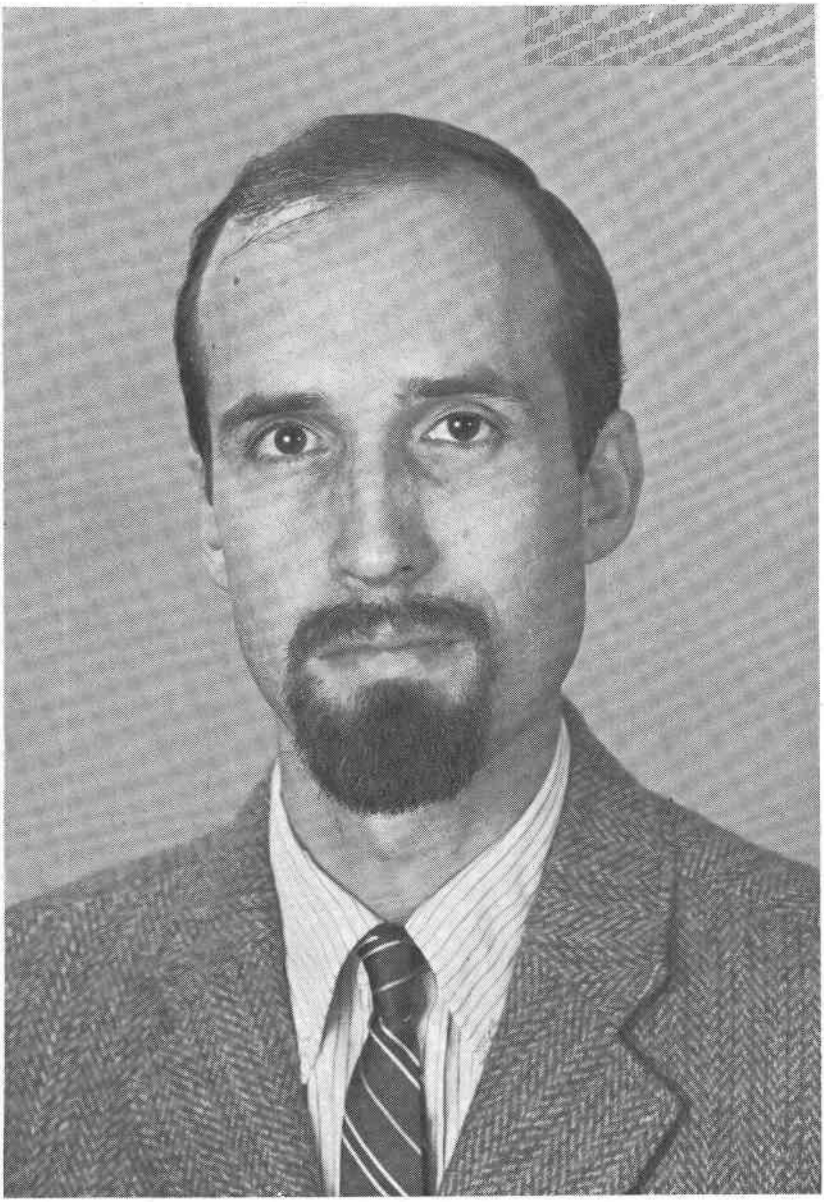
Mr. President, Ray, Ladies and Gentlemen:

One year ago in Milwaukee I was informed that I would receive the Mineralogical Society of America Award at the next MSA-GSA national meeting. Since that time I have often wondered "Why me?" I remember about ten years ago standing knee deep in the sewage of the Mystic River near Boston, Massachusetts, trying to obtain a sediment sample sufficiently enriched in mackinawite or greigite to enable proof that either mineral actually forms and persists in sediments. Some children passing by on a nearby bridge looked down at me, and began to laugh. In my defense, I yelled to them that I was finding gold (actually it was fool's gold) in the muck and that they might help me. After a short pause the reply came back, "Heck, it aint woith it, mistah!" However, my presence here today suggests that such lunacy, after all, might have been "woith it." I consider this award as a recognition that the mineralogy and chemistry of sediment diagenesis, my major interest over the past ten years, is a subject worthy of pursuit.

As is customary I would like to acknowledge some of the people



Robert A. Berner

who have been influential in my career. Since time doesn't allow it, I hereby apologize to all those many others who have helped me, but who are not mentioned by name. My career began as an undergraduate at the University of Michigan, who could not decide what subject to adopt as a major. My brother, Paul C. Berner, who is a geologist, suggested that I take a course in geology before deciding (at that time) between mathematics and philosophy. So I did, and as a result I ended up, much to my own surprise, as a geology major. It was later at Michigan that I received the most important advice that shaped my future. I planned, after taking a Masters degree at Michigan, to become a field geologist with an oil company. However, my advisor, Lou Briggs, insisted (against my protestations) that, (1) I go on for a PhD, and (2) that I go to Harvard. After much argument I relented and took Lou's advice and have never regretted it. At Harvard I benefitted greatly from association with Ray Siever who oriented me toward studies of recent sediments from a diagenetic standpoint, and Bob Garrels, who showed me how to apply physical chemistry to the solution of sedimentary problems. While at Harvard, I also spent one memorable summer doing field work on the marshes and tidal flats of Cape Cod with Dr. Alfred C. Redfield of the Woods Hole Oceanographic Institution. Dr. Redfield taught me the valuable lesson of trying to solve scientific problems, one at a time, by the simplest and most economical means necessary for doing a proper job.

Since leaving Harvard in 1962, I have been further influenced by the work of Dr. Garrels and as a result have found myself making experimental measurements of mineral stability at low temperatures and checking the Garrels and Thompson model for sea water. During this period my interests in diagenesis have evolved in the direction of trying to combine theoretical diffusion models with laboratory and field measurements to explain the distribution of dissolved species and authigenic minerals in sediments. In this work, I have been greatly influenced by a book entitled "The Kinetics of Precipitation," written by Arne E. Nielsen, and as a result have become interested in trying to apply some of Nielsen's theoretical kinetic models to the dissolution and crystallization of sedimentary minerals. Since dissolution and crystallization involve, by necessity, the interface between solids and aqueous solution, a proper understanding of kinetics requires a proper understanding of interface chemistry. This has led to my most recent interest in the work of Dr. Werner Stumm, who is an authority on the surface chemistry of minerals. In fact, I plan to study with him next year while on sabbatical.

At this point I would like to make a plea for the application of chemical kinetics to the study of low temperature mineralogical problems. Thermodynamics tells us what should happen, in a given situation, but, unfortunately, under earth surface conditions it usually doesn't happen that way. Calcium carbonate doesn't precipitate from supersaturated sea water, gypsum precipitates but often in the stability field of anhydrite, many detrital ferric iron minerals are not reduced to ferrous sulfides in the presence of high concentrations of H_2S, aragonite persists in marine sediments although calcite is more stable, and so on. All these problems require kinetic explanations. In addition, while equilibrium *is* being achieved during diagenetic burial, the mechanisms and rates of reaction are subjects worthy of study for their own sake. Although, kinetics, especially heterogeneous kinetics, is a dirty game which is fraught with theoretical and experimental difficulties, I hope that many other workers in sediments will also get interested in the subject. I believe that the future of sedimentary geochemistry lies in this direction.

And now for some final words of a more philosophical nature. We who continually use physics, chemistry, and mathematics in solving geological problems can be easily misled into using our knowledge to "snow" our more field oriented colleagues, each other, and, sometimes, even ourselves. We should guard against this tendency (I'm guilty of it myself) for fear of substituting style for substance. On the other hand, many scientific problems are complex and cannot be solved by oversimplified reasoning. A good example is the set of problems collectively referred to as the environment. Recently there has been a wealth of "solutions" offered, many of which are based on insufficient data and incomplete, if not pre-biased reasoning. Deceptively simple slogans can also be used to "snow" an audience.

Thank you very much for this award and especially for the recognition you have given to my field.