ACCEPTANCE OF THE ROEBLING MEDAL OF THE MINERALOGICAL SOCIETY OF AMERICA

CECIL EDGAR TILLEY, *University of Cambridge, Cambridge, England.*

Mr. President, Professor Buddington, Fellows and Members of the Mineralogical Society, Guests:

The Mineralogical Society has bestowed a very great honour upon me with the award of the Roebling Medal, and it is with the sincerest appreciation that I would wish to record my thanks to the Council for their most generous estimate of my merits.

When I survey the roll of former recipients of this medal I am very conscious of my own shortcomings, and if the fact that I am the thir-

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Recipient of the Roebling medal of the Mineralogical Society of America.

teenth recipient engenders in me a double awareness, I may be forgiven if I am grateful that today is not also Friday.

The occasion is one inviting retrospect and I may be permitted a personal note.

Looking back through the years I realize that my lot has fallen in pleasant places. I was particularly fortunate and privileged as a young research student from the Antipodes to begin work under Alfred Harker at Cambridge, and at a time when his interests were concentrated particularly on problems of metamorphism.

My own contributions in this field stem indeed from the stimulus I received then and through later years under his inspiring guidance.

Nor should I omit to mention at this time my debt to another great pioneer—George Barrow, who first directed my steps in the field of zonal metamorphism.

I treasure the friendships I have been privileged to enjoy with colleagues on this side of the Atlantic. If I should refer among these to one in particular of long standing, it is to that of Dr. Bowen. I recall the inspiring days—now almost thirty years ago—of participation in field excursions with Bowen and Harker in the glorious Isle of Skye, when the problems of monomineralic rocks, then a focus of Bowen's researches, were exemplified in the rusty peridotite dikes that climbed to the Cuillin sky.

Later came under his aegis, the opportunity of spending a period at the Geophysical Laboratory, a link that has remained precious through the years.

Would that I could feel that I had turned all these favours of fortune to the best account in the service of our Science.

Professor Buddington has been most kind in presenting my record, considering that he has had ample opportunity from personal experience, adequately to assess my deficiencies. His account does not hide from you that I am to be recognised as some sort of a hybrid of mixed mineralogical-petrological origin.

If then I should venture to add very shortly a few words on the prospect of some aspects of mineralogy, you would expect my appreciation to have been gleaned through petrological spectacles.

The present status of such subjects as granite, alkali rocks and the concept of nephelinization, peridotite and serpentine, carbonatite geology and metamorphic facies—to name but a few—presents an enthralling picture, and brings home to us that we live in stirring and romantic times.

Is it not already clear that to the solution of these challenging problems, experimental mineralogy has a great contribution to make? I see the prospect of mineralogy bright indeed.

Twelve years ago Professor Buddington in an address to the New York Mineralogical Club expressed the view that at least one new section of mineralogical science might be expected to develop on the average every ten years.

With the development of new experimental methods in the investigation of hydrous silicate systems, I believe such a new era in genetic mineralogy has arrived—may I call it the era of the conquest of the wet mineral silicate and the exploration of the subsolidus?

If I were to attempt to date its effective arrival, I should place it in 1949—ushered in with Bowen and Tuttle's experimental study of the system magnesia-silica-water, shedding new light yet posing new problems on the peridotite-serpentine enigma.

Our new era has already revealed something of the complexity of apparent simplicity among the great group of rock formers—the feldspars; yet in this field as each forward step is taken fresh problems loom ahead.

A new glow is emerging from the subsolidus of the nepheline system, and what prospects are in store for carbonatite geology and indeed carbonate rock metamorphism, when silicate systems bearing carbon dioxide surrender to quantitative treatment in the hydrothermal bomb?

I should not hazard a guess how soon an adequate body of data will be assembled and developed on such topics.

How true are the words of Faraday that it is the great beauty of science that advancement in it, whether in a degree great or small, instead of exhausting the subject of research, opens the door to further and more abundant knowledge, overflowing with beauty and utility.

So it is Mr. President that I foresee in our new experimental era, genetic mineralogy flourishing abundantly, opening the door to new vistas and playing a noble role in the elucidation of petrology's intractable systems.

Granted a few only by participation can hope to capture the supreme exhilaration of the chase, there remains for some the opportunity—though it may be inadequately seized—to play a part in applying the results of experimental disciplines—wet or dry—to the interpretation of their own petrological problems.

Though I have myself striven along these humbler lines, it is, I judge, more by persistence of effort than real achievement that I can in any way be considered to merit the honour you have conferred upon me.

It is therefore Mr. President, in a spirit of humility that I tender to the Mineralogical Society of America, my warmest thanks for the high compliment they have extended to me today.