Two Career-Changing Interactions with Gerry

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I deeply regret that my health does not permit me to join you today at this memorial for Gerry Schubert. (However, I confidently expect to be rejoining department gatherings in the Fall.)

Gerald Schubert was one of the leading geophysicists (and planetologists) ever, whether one looks across our department, across the AGU, or across planet Earth. Others will have to take up the challenge of summarizing his broad range and his successful applications of fundamental physics throughout our solar system. Today I just want to share two cherished anecdotes.

I first met Gerry 52 years ago, in 1974. There was a Gordon Conference in New Hampshire on the topic of "Mantle Convection and Plate Tectonics." I attended as a graduate student from MIT, while Professors Schubert and Kaula represented UCLA. We happened to share a lunch table on the first day. In response to something I said about the importance of shear-strain heating in convection, Gerry shared an insight he had recently developed:

> Consider a plane layer of viscous fluid in steady-state convection at a modest Rayleigh number. Conservation of energy tells us that the mean heat-flow coming out the top has to equal the mean heat-flow coming into the bottom. This means that total internal heating has to be zero. Therefore, the net value of shear-strain heating (which is always positive) must exactly offset the net value of adiabatic heating (which is negative, because upwellings are hotter than downwellings).

I was so impressed by this deep insight, shared in a friendly manner in simple language, that I decided I would like to work in a department with wise but friendly senior faculty like Schubert and Kaula. Then, about a year later, UCLA advertised a faculty position, and I applied, and the result was that I happily worked here for 35 years as Professor and 15 more as Emeritus.

Then, sometime during 1976-1978 when I was at UCLA, Bill Dickinson of Stanford gave an inspiring seminar here on geologic evidence for flat subduction under the western U.S. as the cause of the Laramide orogeny that formed the Rocky Mountains. I went to ask Gerry whether such flat subduction was physically possible? He took the time to walk me through all the steps of a theoretical study that he was then conducting with Aaron Tovish and Bruce Luyendyk (eventually published as *Tovish et al.*, 1978 in *JGR*). This showed that any subduction zone (with specified plate velocity, slab age, asthenosphere viscosity, *etc.*) has a minimum stable angle of dip. For any smaller starting dip, the dip is bound to gradually decrease as the asthenospheric wedge is gradually expelled. The natural consequence is flat subduction, with no asthenospheric wedge, and direct contact between the subducting and overriding plates.

I felt so fortified by Dickinson's geologic observations and Schubert's theoretical insights that I proceeded to devote a large fraction of my next 30 years of research to studying and modeling this event of flat subduction forming the Rockies. In a series of 8 papers, I believe I showed that it passes all relevant tests (whether kinematic, dynamic, or geochemical) in either 2-D or 3-D

models, and that it also explains the formation of the Basin and Range province and the elevation of the Great Plains.

Doubtless others will have similar stories to tell. I also feel sure that the hundreds (or maybe thousands?) of graduate students around the world who have really worked their way through Gerry's two fine textbooks took away fundamental insights and confidence in physics from their great tutor.