Starting Steps on My Path to Internal Atmospheric Gravity Waves

Colin O. Hines; June 26, 2013; 2013 CEDAR Workshop

I want to begin these reminiscences with an act of faith: faith that Dave would have said mainly nice things about me in his introduction, which, as I write, I have not yet heard. So: thank you, Dave for your kind introduction, whatever it was. And thank you too, and your colleagues, for conceiving and implementing this workshop session as a tribute to me; I feel greatly honored. Thanks also to two of my offspring, David Hines and Karen Hines, for their part in arranging and bringing off my end of this teleconferencing operation, which, I am told, puts me in the same league as Bill Gates — if only!!!! Finally, my thanks go to the NSF and its CEDAR program, for supporting the idea of the session and its consummation today; they have been generous in this as in their earlier support of me and my group at the Arecibo Observatory.

My remarks today will be essentially a precis of those I made many years ago at an after-dinner event in Adelaide, Australia, which have since been published in volume 130 of Pure and Applied Geophysics, 1989, if anyone cares to get more detail.

My work on gravity waves began in 1954 in Cambridge, England, where I was a doctoral candidate. Since many or most of you in the audience were not even born at that time, I cannot ask you to remember what the world was like then; instead, I must reconstruct it for you. Firstly, the world was devoid of cell phones. As a consequence, students had time to actually think about their research work. That process was abetted by the simultaneous absence of any high-speed computers to speak of, which they might otherwise have hoped would do their thinking for them en route to thesis material. The world embraced no artificial satellites to facilitate long-distance radio communications, so such communications were conducted by reflection of HF radio waves from the ionosphere, preferably with it in an undisturbed state, or by over-the-horizon scattering of VHF waves from whatever small-scale irregularities in the refractive index might exist, generated by who-knew-what? The most popular candidate as the generating mechanism, and so as a topic of research interest, was atmospheric turbulence — a field whose theory was just then being opened up by George Batchelor and colleagues, also in Cambridge.

Independently, however, medium-frequency and high-frequency observations of the ionosphere were revealing irregularities at much larger scales, and these often exhibited horizontal progression, cautiously termed "drifts" in the absence of any established mechanism of motion, though it was widely hoped that they represented winds. The largest of all these irregularities, both in scale and in intensity, were so-called "traveling ionospheric disturbances" or TIDs, which exhibited also a vertical progression, invariably downward. This downward characteristic led many to think that the TIDs might be generated at higher elevations, perhaps at what we now call the magnetopause, by interaction with what we now call the solar wind. If so, the downward progression might be accomplished by magneto-hydrodynamic or hydromagnetic waves, i.e., Alfven waves, which had been introduced to science only a few years earlier. I had already worked on such waves in Ottawa, before going to Cambridge, so I was asked by Jack Ratcliffe — the chief guru of ionospheric studies at the time, both in Cambridge and internationally — to look into that possibility. I began by viewing the observations — always a safe start for theorists — as they had been reported by Munroe from Australia. He in turn referred to fellow Aussie David Martyn, who had invoked some form of atmospheric wave as the causative agency but went on to propose an unsatisfactory geomagnetic interaction to account for the downward progression of the TIDs. His story included the effects of gravity, which I had ignored in all my previous studies, so I was prompted to include them to see if they seriously changed any of my earlier conclusions. To my surprise, they opened up a whole new class of waves to me — internal gravity waves, of course — and suddenly all the unrelated observations that were cluttering up my memory fell into place within a single scheme: the downward progression was to be expected of gravity waves generated in the energy-rich troposphere. And so forth. Some time and argument was required before this scheme was accepted widely, but I trust you will agree that at last it has been — or you would not be involved in seeking out its still-hidden treasures and in extending it, as I believe you are doing. I am looking forward to learning just how, as this workshop continues.

So many observations fell neatly into place, instantly and in an ongoing expansion, that challenging new observations, sufficient to spur the demand for and production of new theory, were in meager supply for a time. I found myself to be constrained to building new theory on old theory. This struck me as a bit incestuous and therefore dangerous, so I quit the field for a period of rehab and of exploring magnetospheric convection.

When at last I returned to gravity waves, I found to my delight that a new observational feature had come to centre-stage and was as yet unexplained — the so-called m^{-3} spectral tail. Jerry Weinstock was the first to identify it as resulting from nonlinear interaction, but the complicated mathematical formulation that he called upon to support his view was difficult to follow or agree with for many, including me. Several half-starts at alternative explanations were soon made by several individuals, resulting in a frustration of the type encountered by the legendary ten blind men trying to describe an elephant, with each holding onto a different part of the beast. Dave Fritts recorded the situation by means of a notorious viewgraph, which I trust he will now display for me, and for you. When I, in turn, produced my own theory of the tail — using the nonlinear consequences of the Doppler term of the Eulerian equations, evaluated ultimately via transformation from the linearized Lagrangian equations — Dave was kind enough to lend me his viewgraph. I copied it and then edited it to show a new onlooker — myself, unblindfolded — able to see the elephant in all its glory. (I trust he has the edited viewgraph on hand, and the decency to show it now.)

Well, that's the way I left the field upon my retirement in 1992; I have not read a word about it since then, that I recall. This leaves me poorly placed to hazard the traditional "onward and upward" forecast as to the most promising paths of future research. I can only mention the areas about which I was most curious when I lost contact with the field, two decades ago. First was the question of parameterization schemes for purposes of large-scale circulation studies: would my own scheme survive or be supplanted by one of better design; and, if supplanted, with what improvements of design and consequences? What tales would the parameterized studies have to tell about the role of gravity waves in altering the circulation? What was the most appropriate input spectrum of gravity waves for use with any parameterization? — either the general background spectrum or the spectrum for use with individual events? No doubt some of these questions have been answered by now and the forefront has moved on, beyond my ken; but I am here to learn, not to predict. I look forward to seeing the rest of this workshop, and in advance I wish to thank all participants in it, for their participation and for bringing me up-to-date. Thank you all.