Bollettino Unione Matematica Italiana

C. Denson Hill

Aldo Andreotti

Bollettino dell'Unione Matematica Italiana, Serie 9, Vol. **4** (2011), n.2, p. 295–299.

Unione Matematica Italiana

<http://www.bdim.eu/item?id=BUMI_2011_9_4_2_295_0>

Bollettino dell'Unione Matematica Italiana, Unione Matematica Italiana, 2011.

Bollettino U. M. I.
(9) **IV** (2011), 295-299

Aldo Andreotti

C. Denson Hill

For me it began at Stanford in early September of 1968. I will only tell the story of how it all began, because that is the most interesting part. I had just finished my first year as an assistant professor in the mathematics department at Stanford. The previous academic year 1967-1968 I had worked very hard, teaching a 3 quarter graduate course in several complex variables, trying to learn something about the subject. This had been very tough, since I had essentially no previous knowledge of the subject, so I spent something like 10 hours preparing each 1 hour lecture. However I had come into the situation with a solid background in classical PDE and applied mathematics. In fact, prior to that, I had been writing papers about free boundary problems. Hormander's book on SCV had recently come out. So even though I started with the oldest references I could find (Poincaré, Volterra, Osgood, Behnke - Thullen, Fuks, etc.), I found myself eventually reading Hormander's book and his Acta papers. I had never laid eyes upon the Cartan seminars, any papers of Oka or Grauert, and was completely ignorant of Andreotti-Grauert and Andreotti-Vesentini. I did not even have a good understanding of the Levi form of a real hypersurface. I had no idea what sort of research project one might attempt in SCV. The only thing in my head were some vague analogies between classical PDE and SCV, in which for example, the light cone for the wave equation should be replaced by analytic disks in SCV.

Then one day in september of 1968, some new guy appeared at tea. I had no idea who he was, but of course it was Aldo Andreotti, who had arrived as a visitor in the mathematics department for 1968-1969. He was quite friendly, unlike some of the other types at tea, so we began to talk. Not about mathematics, but about history, philosophy and religion. I found him absolutely fascinating, and started to arrive at tea early each day to talk to him. This went on for probably a couple of weeks. Almost no one else at tea was having any significant conversations with him, so I had free reign to continue these fascinating discussions about everything except mathematics. Then one day he suddenly turned to me and asked me if I believed in God. I said no. He then smiled broadly, and announced that he was a practicing Catholic. I was sure that I had fatally offended him, but before I had a chance to say anything, he added "Very good! That means we can be good friends!" And so it was.

Shortly after that he asked me what sort of mathematics I was interested in. I told him about my experience the previous year of trying to learn something about SCV. When I said I had been reading the papers of Hormander, he asked me if I could explain them to him. So we started to meet in my office each day after tea, and I would spend an hour or so at the blackboard, essentially repeating my lectures from the previous year. It was unreal. He took careful notes, as if he were my student, but soon it became obvious that somehow he knew it all already, although sometimes with slightly different terminology. I could not believe it, and found it embarassing to be lecturing to him. (I was still ignorant of the contents of Andreotti-Grauert and Andreotti-Vesentini.)

Eventually the situation evolved to where he was going to the board, and explaining things to me, which I later learned came from Andreotti-Grauert and Andreotti-Vesentini. It progressed on to open questions and somewhat vague conjectures, in which he wanted to understand the relationship between the signature of the Levi form and the famous nonsolvability example of Hans Lewy. He was rather unhappy that the intersection of two open sets had to be open. At that point I suggested that we should be thinking in terms of an initial value problem for the ambient Cauchy-Riemann equations. I also told him something I had noticed: that while for determined systems, being either elliptic or hyperbolic were mutually exclusive notions, the deRham complex, as a complex of operators, was simultaneously elliptic and hyperbolic, with as many time-like variables as the codimension. It did not work quite the same for the Dolbeault complex, but the analogy was there. That is how our collaboration began.

We worked for the remainder of the academic year, until he left to return to Pisa. Before leaving he suggested that I should spend the next year in Pisa, so we could continue our work. Probably with his recommendation, I managed to get a NATO fellowship, and arrived in Pisa the next year, knowing no Italian. But before leaving Stanford, I had a crisis of confidence: Hormander was at Stanford at that time. One day at tea, he said that he had heard that Andreotti and I were working on something, and asked me to explain what the result was supposed to be. So I went to the board in the tea room, explained our conjectured result, and tried to outline the method of proof. Unfortunately I wrote down some estimate (which fortunately turned out to be true), at which point Hormander announced to the entire tea room that it could not possibly be true, and that I had better go back to my office and check my calculations. He then looked at his watch (he allowed himself only a certain number of minutes for tea), and swiftly walked back to his office, leaving me standing there devastated with the chalk in my hand.

Working conditions in Pisa were ideal. Andreotti liked to work at home undisturbed in the morning, eat lunch there, and then take a siesta. He would appear at the university around 4 pm, looking relaxed, rested and ready to teach, attend a seminar, or discuss our work. I soon learned his route, and would hang around outside his house, waiting for him to emerge around 3:45. We would then walk together, stop for an expresso, and arrive at the university. On some days when there was no seminar we would take long walks along the lungarno, discussing everything. These are some of the sweetest times I can remember. Our first papers were written in 1970.

I must say something about the special nature of the mathematics seminars in Pisa in 1970. At that time Andreotti was lecturing on celestial mechanics, guided by the book of Carl L. Siegel. Everyone was there. At one point, there was an important identity which Siegel had obtained by expanding a messy 10 X 10 determinant by cofactors of the first 2 rows. Aldo's proof was three words: "lavoraccio con matricione". There were other seminars as well, with everyone attending, and everybody taking their turn as speaker. In order to explain the special nature of these Pisa seminars, I need to first explain that as a PhD student at the Courant institute at NYU, in my first job at the Rockefeller institute, and later at Stanford and from hanging around at Berkeley, I had gotten to know almost all of the famous professors there. So I was quite used to being in the company of mathematicians who were infinitely better than myself. But none of this had prepared me for the incredible intensity of the seminars in Pisa. All the seminars I had attended in New York and on the west coast were laid back in comparison.

When I say "everyone was there" above, I mean that the members of the 1970 seminars in Pisa were: Aldo Andreotti, Enrico Bombieri, Sergio Campanato, Ennio De Giorgi, Enrico Giusti, Mario Miranda, Giovani Prodi, Claudio Rea, Guido Stampacchia, etc. And almost everyone except me was a chain smoker, at least during the seminar. When the seminar started the air was clear. But about halfway through, the upper half of the room was filled by a dense cloud of smoke, so that you had to bend down to see the board. Whoever was speaking on a given day would begin by recalling some key definitions, and then move on to his first lemma or theorem. But he would never get to present the proof. Either the entire audience would agree that it was obvious, and urge the speaker to go on to the next lemma or theorem, or else each member would suggest their own proof, and begin to argue over whose proof was better. At some point in the midst of this chaos, the speaker would just give up, and attempt to move on to his main theorem. Sometimes he actually got to state it. But often the audience would have already guessed what was going to be his main theorem. In either case, the speaker had no chance to present the proof, because the audience had already come up with several different proofs (or counterexamples!), and had moved beyond the main theorem with improvements, generalizations and conjectures. More smoke. More arguing, especially over what was the final best improved version of the main theorem, and how to properly state the (now) new main conjecture. And throughout all of this, Andreotti sat in the middle of the first row and played a central role. An incredible experience which I am sure I will never have again.

In our first paper [1] we showed how, given a system of smooth complex vector fields, which satisfies the natural formal integrability conditions, and which admits as many functionally independent solutions to the homogeneous system as linear algebra allows, they arise locally as the tangential Cauchy-Riemann equations to a CR submanifold, generically embedded in $\mathbb{C}^N$. Here $N = n + k$, where n is the number of vector fields (the CR dimension) and k (the CR codimension) is equal to the number of variables minus $2n$. Then came the question of how to produce, at least locally, the required number of functionally independent solutions. So we employed a trick I learned from Garabedian, which allowed us to produce the required number of independent solutions, provided the vector fields were real analytic in a certain subset of the independent variables. We were a bit unhappy that we had to assume real analyticity in some of the variables. But later, as was first shown by L. Nirenberg, the result is false with only smooth coefficients. Thus there arose the notion of smooth abstract CR structures, which might not be locally CR embeddable.

In our second paper [2] we overcame Andreotti's unhappyness that the intersection of two open sets had to be open, by proving a version of the Mayer-Vietoris sequence for analysts, which is well suited to the study of the Cauchy problem with initial values on a real hypersurface, and to the analogue of the Riemann-Hilbert problem for several complex variables. This was done not just for functions, but for cohomology classes. It had the virtue of reducing many different questions to the task of proving certain vanishing theorems, with regularity up to the (partial) boundary. In particular, this indicated how one should obtain the relationship between the tangential cohomology on the hypersurface, and the ambient cohomology coming from the two sides of the hypersurface.

In our third paper [3] we proved the requisite local vanishing theorems, obtaining the results Hormander had rejected at tea. Under certain circumstances, it was possible to use the Andreotti-Grauert bump method to globalize the local vanishing, to obtain global finiteness or global vanishing theorems. Hence it was not necessary to prove directly global results. This is remarked at the end of the paper. In particular, at a point on the hypersurface where the Levi form is nondegenerate, with p positive eigenvalues and q negative eigenvalues, we showed that the local tangential cohomology on the hypersurface is infinite dimensional in bidegree (r, p) and (r, q), and is otherwise zero, for all r. This means that there are Lewy type nonsolvability examples in exactly the bidegrees corresponding to the signature of the Levi form, at least in the nondegenerate case. Thus we established the connection between the signature of the Levi form and the Lewy nonsolvability, which Andreotti was searching for in 1968. The title of our two papers was intended as a joke on the two different spellings and pronounciations of "Levi". We made an important distinction between "using the positive eigenvalues" and "using the negative eigenvalues", in which we understood that the zero eigenvalues were to be regarded morally as being positive. History, I think, has shown us to be correct on this point. The situation as of 1975 was summarized in [4]. Since then, our result using the positive eigenvalues has been improved, in which the zero eigenvalues play the role of being positive. But our result using the negative eigenvalues has, after 40 years, still not found it's correct generalization, although a partial step was made in [6].

Finally, the paper [5] was an attempt at a grandiose extension of [2]. It was not followed by a generalization of [3], because we were trying to be too general, and it all became too complicated. However later, in more manageable situations, this thread was systematically developed in an important series of papers by Andreotti and Nacinovich. They were working right up to Andreotti's untimely death.

REFERENCES

[1] A. Andreotti and C. Denson Hill, *Complex characteristic coordinates and the tangential Cauchy-Riemann equations*, Ann. Scuola Norm. Sup. Pisa, **26**, 2 (1972), 299-324.

[2] A. Andreotti and C. Denson Hill, *E. E. Levi convexity and the Hans Lewy problem, Part I: Reduction to vanishing theorems*, Ann. Scuola Norm. Sup. Pisa, **26**, 2 (1972), 325-363.

[3] A. Andreotti and C. Denson Hill, *E. E. Levi convexity and the Hans Lewy problem, Part II: Vanishing theorems*, Ann. Scuola Norm. Sup. Pisa, **26**, 4 (1972), 747-806.

[4] C. Denson Hill, *A hierarchy of nonsolvability examples*, Proc. Symp. Pure Math. (Diff. Geom.), **27**, AMS, Providence, R.I. (1975), 301-305.

[5] A. Andreotti, C. Denson Hill, S. Lojasiewicz and B. MacKichan, *Complexes of differential operators*, Inventiones Math., **35** (1976), 43-86. [volume dedicated to J. P. Serre]

[6] C. Denson Hill and J. Brinkschulte, *On the Cauchy problem for the $\overline{\partial}$ operator*, Ark. Mat., **47**, 2 (2009), 231-141.

Department of Mathematics
Stony Brook University, Stony Brook, N.Y. 11794, USA
e-mail: dhill@math.sunysb.edu

Received June 3, 2010