1. On How I Got Started in Dynamical Systems 1959-1962*

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1. Let me first give a little mathematical background. This is conveniently divided into two parts. The first is the theory of ordinary differential equations having a finite number of periodic solutions; and the second has to do with the case of infinitely many solutions, or, roughly speaking, with "homoclinic behavior."

For an ordinary differential equation in the plane (or a second-order equation in one variable), generally there are a finite number of periodic solutions. The Poincaré-Bendixson theory yields substantial information. If a solution is bounded and has no equilibria in its limit set, its asymptotic behavior is periodic. The Van der Pol equation without forcing is an outstanding example which has played a large role historically in the qualitative theory of ordinary differential equations.

There was some systemization of this theory by Andronov and Pontryagin in the 1930s when these scientists introduced the notion of structural stability. There is a school now in the Soviet Union, the "Gorki school" which reflects this tradition. Andronov's wife, Andronova Leontevich, was a member of this school, and I met her in Kiev in 1961 (Andronov had died earlier). A fine book (translated into English) by Andronov, Chaiken, and Witt gives an account of this mathematics.

The work of Andronov-Pontryagin was picked up by Lefschetz after World War II. Lefschetz's book and influence helped establish the study of structural stability in America.

* This essay is partly based on a talk given at a Berkeley seminar circa 1976.

The second part of the background mathematics has to do with the phenomenon of an infinite number of periodic solutions (persistent under perturbation) or the closely related notion of homoclinic solutions. Again, Poincaré wrote on these problems; Birkhoff found a deeper connection between the two concepts, homoclinic solutions and an infinite number of periodic points for transformations of the plane. From a different direction, Cartwright and Littlewood, in their extensive studies of the Van der Pol equation with forcing term, came across the same phenomena. Then Levinson simplifed some of this work.

2. Next, I would like to give a little personal background. I finished my thesis in topology with Raoul Bott in 1956 at the University of Michigan; and that summer I attended my first mathematics conference, in Mexico City, with my wife, Clara. It was an international conference in topology and my introduction to the international mathematics community. There I met René Thom and two graduate students from the University of Chicago, Moe Hirsch and Elon Lima. Thom was to visit the University of Chicago that fall, and I was starting my first teaching job at Chicago then (not in the mathematics department, but in the College). In the fall I became good friends with all three. I attended Thom's lectures on transversality theory and was happy that Thom and Hirsch became interested in my work on immersion theory.

My main interests were in topology throughout these years, but already my thesis contained a section on ordinary differential equations. Also at Chicago, I used an ordinary differential equation argument to find the homotopy structure of the space of diffeormorphisms of the 2-sphere. Having both Dick Palais and Shlomo Sternberg at Chicago was very helpful in getting some understanding of dynamical systems.

3. It was around 1958 that I first met Mauricio Peixoto. We were introduced by Lima who was finishing his Ph.D. at that time with Ed Spanier. Through Lefschetz, Peixoto had become interested in structural stability and he showed me his own results on structural stability on the disk D^2 (in a paper which was to appear in the Annals of Mathematics, 1959). I was immediately enthusiastic, not only about what he was doing, but with the possibility that, using my topology background, I could extend his work to n dimensions. I was extremely naive about ordinary differential equations at that tirne and was also extremely presumptuous. Peixoto told me that he had met Pontryagin, who said that he didn't believe in structural stability in dimensions greater than two, but that only increased the challenge.

In fact, I did make one contribution at that time. Peixoto had used the condition on D^2 of Andronov and Pontryagin that "no solution joins saddles." This was a necessary condition for structural stability. Having learned about transversality from Thom, I suggested the generalization for higher dimensions: the stable and unstable manifolds of the equilibria (and now also of the nontrivial periodic solutions) intersect transversally. In fact, this was a useful condition, and I wrote a paper on Morse inequalities for a class of dynamical systems incorporating it.

However, my overenthusiasm led me to suggest in the paper that these systems were almost all (an open dense set) of ordinary differential equations! If I had been at all familiar with the literature (Poincaré, Birkhoff, Cartwright-Littlewood), I would have seen how crazy this idea was.

On the other hand, these systems, though sharply limited, would find a place in the literature, and were christened Morse-Smale dynamical systems by Thom. This work gave me entry into the mathematical world of ordinary differential equations. In this way, I met Lefschetz and gave a lecture at a conference on this subject in Mexico City in the summer of 1959.

We had moved from Chicago in the summer of 1958 to the Institute for Advanced Study in Princeton; Peixoto and Lima invited me to Rio to finish the second year of my NSF postdoctoral fellowship. Thus, Clara and I and our two kids, Nat and Laura, left Princeton in December, 1959, for Rio.

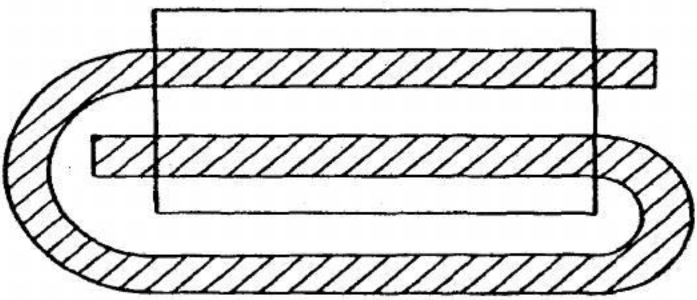
4. With kids aged 1/2 and 2-1/2, and most of our luggage consisting of diapers, Clara and I stopped to see the Panamanian jungles (from a taxi). I had heard about and always wanted to see the famous railway from Quito in the high Andes to the jungle port of Guayaquil in Ecuador. So, around Christmas of 1959, the four of us were on that train. Then we spent a few days in Lima, Peru, during which we were all quite sick. We finally took the plane to Rio. I still remember well arriving at night and going out several times trying to get milk for the crying kids, always returning with cream or yogurt, etc. We learned later that milk was sold only in the morning in Rio.

But with the help of the Limas, the Peixotos, and the Nachbins, life got straightened out for us. In fact, it happened that just before we arrived the leader of an abortive coup, an air force officer, had escaped to Argentina. We got his luxurious Copacabana apartment and maids as well.

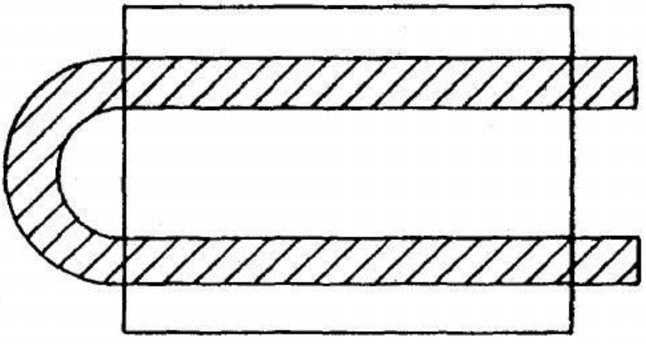
Shortly after arriving in Rio my paper on dynamical systems appeared, and Levinson wrote me that one couldn't expect my systems to occur so generally. His own paper (which, in turn, had been inspired by work of Cartwright and Littlewood) already contained a counter-example. There were an infinite number of periodic solutions and they could not be perturbed away.

Still partly with disbelief, I spent a lot of time studying his paper, eventually becoming convinced. In fact this led to my second result in dynamical systems, the horseshoe, which was an abstract geometrization of what Levinson and Cartwright-Littlewood had found more analytically before. Moreover, the horseshoe could be analysed completely qualitatively and shown to be structurally stable.

For the record, the picture I abstracted from Levinson looked like this:



When I spoke on the subject that summer (1960) at Berkeley, Lee Neuwirth said: "Why don't you make it look like this?"



I said "fine" and called it the horseshoe.

I still considered myself mainly a topologist, and when considering some questions of gradient dynamical systems, I could see possibilities in topology. This developed into the "higher-dimensional Poincaré conjecture" and was the genesis of my being quoted later as saying I did my best known work on the beaches of Rio. In fact, I often spent the mornings on those beaches with a pad of paper and a pen. Sometimes Elon Lima was with me. In June I flew to Bonn and Zurich to speak of my results in topology. This turned out to be a rather traumatic trip, but that is another story.

I had accepted a job at Berkeley (at about the same time as Chern, Hirsch, and Spanier, all from Chicago) and arrived there from Rio in July, 1960. Except for a few lectures on "the horseshoe," I was preoccuped the next year with topology. But, in the summer of 1961, I announced to my friends that I had become so enthusiastic about dynamical systems that I was giving up topology. The explicit reason I gave was that no problem in topology was as important and exciting as the topological conjugacy problem for diffeomorphisms, already on the 2-sphere. This conjugacy problem represented the essence of dynamical systems, I felt.

5. During this year I had an irresistible offer from Columbia University, so we sold a house we had just bought and moved to New York in the summer of 1961. But before taking up teaching duties at Columbia, I spoke at a conference on ordinary differential equations in Colorado Springs and then in September, 1961, went to the Soviet Union. At a meeting on nonlinear oscillations in Kiev, I gave a lecture on the horseshoe example, "the first structurally stable dynamical system with an infinite number of periodic solutions." I had a distinguished translator, the topologist, Postnikov, whom I had just met in Moscow. Postnikov agreed to come to Kiev and translate my talk in return for my playing go with him. He said he was the only go player in the Soviet Union. My roommate in Kiev was Larry Markus.

I met and saw much of Anosov in Kiev. Anosov had followed the Gorki school, but he was based in Moscow. After Kiev I went back to Moscow where Anosov introduced me to Arnold, Novikov, and Sinai. I must say I was extraordinarily impressed to meet such a powerful group of four young mathematicians. In the following years, I often said there was nothing like that in the West.

I gave some lectures at the Steklov Institute and made some conjectures on the structural stability of certain toral diffeomorphisms and geodesic flows of negative curvature.

After I had worked out the horseshoe, Thom brought to my attention the toral diffeomorphisms as an example with an infinite number of periodic points which couldn't be perturbed away. Then I had examined the stable manifold structure of these dynamical systems.

6. After teaching dynamical systems the fall semester at Columbia, I was off again, with Clara and the kids, this time to visit André Haefliger in Lausanne for the spring quarter. Besides lecturing in Lausanne, I gave lectures at the College de France, Urbino, Copenhagen, and, finally, Stockholm, all on dynamical systems and emphasizing the global stable manifolds, which I found more and more to lie close to the heart of the subject. In Stockholm, at the International Congress, I saw Sinai again and he told me that Anosov had proved all the conjectures I had made the preceding year in the Soviet Union.

In Lausanne I had begun to start thinking about the calculus of variations and infinite-dimensional manifolds, and this preoccupation took me away from dynamical systems for the next three years.