When I arrived at MIT in summer 1962, after obtaining my BSc from the Technion (Israel Institute of Technology), I was assigned to help a more senior Research Assistant with his "tubular wave breaker" experiments, in the long flume parallel to the southern wall of the main hall in Building 48. This was before the top floors were added in 1970 and the Building 48 was renamed “The Parsons Lab”. The flume was equipped with a wave generator and a floating wave dissipater - a bundle of aluminum tubes placed length-wise half-way downstream. Wave-height sensors before and after the tubes monitored the wave attenuation they produced.

The sensors were wired to a Sanborn recorder that spewed out a long strip of thermal paper with traces over time. The creaky old recorder would not keep the recording paper straight for very long, so we had to pull it into place once in a while. The results were read from the strip and replotted by hand on a 2D graph of wave-height attenuation vs. wave properties. The results showed little correlation with the explanatory variables, but the RA whom I was helping did manage somehow to put a straight line through the cloud of points. It was then I coined the term: "through any number of points in the plane one can pass a straight line … provided it is thick enough".

I worked on the wave breakwater during the summer months until the semester began when I was assigned by Prof. Arthur (Art) Ippen, Head of the Lab, to another of his projects - the initiation of sediment transport in streams. Since entrance and side effects in a flume are difficult to get rid of, and they affect initiation of motion of the bed load, Prof. Ippen proposed to build "an infinite channel" consisting of the water-filled space between two concentric vertical cylinders rotating at differential speeds. The speed difference would cause a differential drag thus creating a shear flow between the cylinders. Sand-like plastic particles were used to simulate the bed-load.

The rotational speed should create a centripetal outward acceleration 20 times that of gravity so that the particles that were "pasted" against the outer cylinder, and would be forced to begin moving when the shear force on them was large enough. Thus the gap would simulate a (side-ways) river channel. Prof. Ippen assigned to the project a Japanese Post-Doc, Hiroyoshi Shi-Igai, and me. We first set out to study the hydrodynamics of flow between rotating cylinders, which the famous G.I. Taylor had studied extensively in the 1920s (See: <http://www.jstor.org/stable/91148?seq=1> and <http://en.wikipedia.org/wiki/Taylor_number>).

Taylor’s highly complex math drew us into a thorough study of the flow field under a variety of conditions, which is characterized by geometry of the gap, the properties of the fluid, and the relative speeds of the two cylinders. Both could be rotating in the same or opposite direction, with one faster than the other. Fortunately, I had a solid education in tensor-based hydrodynamics in my undergraduate studies at the Technion and my colleague Dr. Shi-Igai also had an equally excellent background in math and hydrodynamics.

Once we mastered the math and determined the design conditions for the apparatus we began designing the experimental setup. The 36" high concentric cylinders would have diameters of 30" and 36", leaving a gap of 3". When rotating independently from each other, at around 300 rpm, the outward "gravity" would be 20g. The cylinders were produced from 10 mm thick sheet metal, bent into a cylinder, the inner surfaces creating the gap were machined and then stress relieved to produce the required cylindrical accuracy. The outer cylinder was mounted on a circular base, while the inner hung from its own concentric shaft by thin steel blades so that the torque on it could be measured by the angular displacement.

The two meter tall assembly of cylinders and shafts was housed in a three-legged frame, with a total weight around one metric ton! Two DC motors to drive the concentric shafts and two Amplidynes (AC driven rotating DC generators) to deliver power to the motors. The motors and Amplidynes were purchased from a Navy junk-yard, where they had been resting idly after having served to control large guns on WWII battle-ship.

The rotational speed of the each cylinder was measured by a light source/photo cell pair that counted the number of passing holes drilled in a circular aluminum plate attached to each shaft. The signal fed into a Wheatstone bridge that converted it into a feedback control signal to the Amplidynes. Small plastic pellets were used to simulate sand on the bottom (outer cylinder). Hydraulic similitude under 20g was used to select their specific weight so that the water + particles would correspond to the conditions in a real river. Discharge and sediment motion in this 300 rpm enclosure was measured with a special Pitot Tube driven across the gap between the rotating cylinders. Inserting the control signal for moving the Pitot Tube and bringing the reading back out was a nightmare of controls and slip rings. A strobe light illuminated the particles motion so they could be viewed through the plastic top cover of the gap as they sped past us.

I had a great time doing all this, not knowing confidently that the idea of "initiation of motion by shear flow" between two vertical rotating cylinders would work at all. I was also concerned that the rotating machine would not survive the amateurish design and construction of this mere civil engineer lacking formal education in materials, mechanical engineering, power electronics and controls. So, when I was ready to fire up the "washing machine", as it became known fondly in the Lab, I cleared the entire basement, where it was located, from everyone else, hid behind a solid concrete column and threw the switches one after the other. Lo and behold: the thing did not break through its housing structure, responded to the controls, and maintained a steady and accurate speed. It took me many more working days to convince myself that the thing worked well, which is when I placed a large placard sign on the frame inscribed with Galileo's famous words "*Eppur Si Muove*" ("And Yet it Moves").

However, it became clear after several weeks of experiments that the secondary flows (spirals, generated by the end effects at the top and bottom) in the gap defeated the dominance of the shear flow, and that initiation of motion of bottom sediments could hardly be studied adequately in this contraption running at 300 rpm. This was a great disappointment to Prof. Ippen and to us, but we had to admit that while it was a magnificent piece of machinery it would not provide the answers it was designed for. So, the facility served for several years in a few theses by other RAs to study flow of air between the rotating cylinders under different conditions, using hot-wire anemometry.

By this time I had petitioned to move to a direct PhD program working on groundwater flow and dispersion, a topic more suitable for the career I was planning upon my return to Israel. I moved to supervision by Professor Donald (Don) Harleman, whose field of research included groundwater. I developed analytical solutions for dispersion in various configurations of flow fields in layered porous media and constructed two physical models: (1) a vertical column with horizontal layers and flow perpendicular through them that would be changed from fresh to saline at time t=0. A breakthrough curve caused by longitudinal dispersion was measured at different points along the column, and (2) a long horizontal box with two sand layers, where the salinity of the inflow section of one layer would be raised at t=o and the lateral salinity profiles at different locations along the box, caused by lateral dispersion, would be measured. Experimental results were compared to analytical solutions and showed acceptable agreement. But analytical solutions are restricted to simple geometries, initial and boundary conditions, so I moved to numerical solutions. I took courses with Prof. Brian in the Department of Chemical Engineering, expert in numerical solutions of multi-phase flow, developed numerical schemes, and began shuttling to and from the Computer Center (IBM 360, decades before personal computers), handing in a deck of punched cards in the evening, getting the printed output the next morning (usually just an indication of the programming mistakes I had made) and by spring 1966 ended up with a thesis titled "Numerical and Analytic Solutions for Dispersion in Porous Media" and 1967 papers with Prof. Harleman in WRR and Proc. ASCE.

I offer a further note on having spent four years in the Lab. There were some 25-30 RAs in the Lab during those years, each working on a topic at the cutting edge of water science and engineering in a specific domain – a veritable show-case of fascinating research and innovation. I spent many hours and days in learning from my colleagues what each was doing, and made a conscious effort to familiarize myself with the advances, achievements and disappointments in many fields. This was a valuable bonus to my own research and the classes I took.

As part of my PhD program I had a minor in "Systems", and took courses in statistics and optimization in the EE Department. In summer 1962 I undertook a project on optimal design of water distribution systems. The programs I developed ran on an IBM 1620 located in Building 1. The 16Kb machine was fed with punched cards and its output came on similar punched cards that were then run through a printer in another room. It also had a set of manual control switches that could be used during debugging. Runs for a small 11-node network took several hours, so I had to sign-in for the computer room at night, brought a coffee-thermos and sandwiches, and made development, debugging and production runs that lasted most of the night. The report was given an A, and I was on my way to a career that combined systems analysis and water resources management.

During the years in the Lab I had become close friends with Chuck Howard, a Canadian RA. After completing his MSc in the Lab, Chuck went to work for a local consulting company, leading a study of the Boston water distribution system, and invited me to take part in it. To respond to the requirement of this study we developed a novel method for solving water distribution systems, which was used in the Boston study and became a joint 1968 Proc. ASCE paper on "Water Distribution Systems Analysis". The most significant outcome of this cooperation has been a life-long friendship and collaboration with Chuck that resulted in development and application of several joint problem-driven methodologies, computer programs, engineering reports and scientific papers. The more than 10,000 km between Haifa (Israel) and Victoria (BC, Canada) posed no hindrance to the camaraderie and progress of our joint creative work, that is as significant today as it was almost 50 years ago at the Parsons Lab.

Uri Shamir (PhD '66) is Professor Emeritus in the Faculty of Civil and Environmental Engineering of the Technion – Israel Institute of Technology, which he joined in 1967, after serving for one year on the staff of CEE/MIT. See: [http://shamir.net.technion.ac.il](http://shamir.net.technion.ac.il/)