Memories of Paul Terasaki
By Josh Miller

1. My Enlightenment. It was the fall of 1969. I had just joined 2 other surgeons at the Walter Reed Army Medical Center in helping to create the US Army Renal Transplant Program after a rather humorous 6 weeks of 'basic (doctor's) training' at Fort Sam Houston in Texas. I had spent the year before as a young Assistant Professor of Surgery at Stanford University Medical Center doing renal transplantation, making use of the expertise of Rosie Payne, a pioneer in the active Stanford group, who had begun to develop a system of histocompatibility typing with reagents based on (more technically demanding) leukoagglutination serology. Brack Hattler, my surgical colleague, trained by the great immunogeneticist Bernard Amos at Duke had joined our Army program simultaneously, and we attended one of the early Terasaki wet workshops at UCLA. Paul was invaluable in transferring his systematized know how to help get our Army transplant tissue typing lab operational. He was very patient with the resulting standardization needed to compare one lab to another even in those early days. This was especially important in detecting recipient preformed antibody to prevent hyperacute renal transplant rejection that had been just described by Starzl in Denver with the aid of the UCLA Terasaki lab expertise, a lab which also laid the foundation of much of the population HLA immunogenetics that we know today.

2. Steps backward, frustration and the new road to success. Imagine the chagrin when the NIH pulled the funding for Paul's amazingly productive lab just a year or two later, seemingly because of a lack of correlation of the degree of donor/recipient incompatibility with the early clinical renal results. This in part may have been due to the impatience of the surgeons who complained that transplants were being held back by the typing lab results, (I was not one of them), with the surgeons always pushing ahead (and damn the torpedoes). Paul was temporarily crushed and quietly expressed his understated irony. But this most fortunately led to his drive to become financially independent of the subjective forces influencing funding decisions and the subsequent development of his ONE Lambda industry of tissue typing resources, a venture capital well that overflowed with a flood of ideas and confirmed discoveries for the succeeding generations.

3. Our contributions with Paul. I was very fortunate to keep in touch with Paul through the years and during the time that we developed our transplant program in Miami. Dr. Violet Esquenazi, our own Tissue Typing lab director, compulsively stored prospective sera for all of our transplant recipients collected preoperatively and every 6 months postoperatively (until death do us part). Of course this led to warehouses of frozen sera samples under continuous safety monitoring and cataloging in our own biorepository from 1979 onwards. Paul knew about Violet's compulsiveness and around the turn of this century decided to raid our bank, with our enthusiastic collaboration. This led to some great papers written with his disciples describing the development of HLA

antibodies clinically correlating with chronic rejection, in prospective studies over 20 years.

4. The early early years. That period may even be more revealing as it involved the support of a visionary UCLA surgeon Dr. William Longmeyer. Bill Longmeyer was the Chairman of the Department of Surgery, a giant of his generation and was a generation in his position. He had diverse interests and regarded organ transplantation, even in the early 50's, as part of the future of surgery and performed tolerance experiments in chicks with his colleague Cannon. They took young Paul into the lab when he looked like a 'wild oriental' (Fig. 1) and of course he developed a more mechanical way to infuse donor blood cells into the chick embryo (Fig. 2). I occasionally kidded him that his first transplantation interest was on tolerance induction and only later did it morph into humeral immunity.

Fig 1

Fig2