

## Some reminiscences of my friendship with Herman Rubin

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I first met Herman Rubin in 1947 while I was writing a dissertation in absentia at Columbia University and he was a Fellow at the Institute of Advanced Study at Princeton. I had recently been appointed as a research assistant at the Cowles Commission for Research in Economics which was then located at the University of Chicago. Herman had completed or almost completed his dissertation at the University of Chicago, and we were to be colleagues at the Cowles Commission from June 1948 to September 1949.

While I was at Columbia, I was supposed to investigate the possibility of inverting large matrices by computer, because the method used by the Cowles Commission for estimating the parameters of the Economy, by maximizing a function using a gradient method, involved the inversion of matrices. I worked at the Watson Laboratories which were located then near Columbia and had use of a “Relay Calculator” which could be programmed (with plug boards) to multiply matrices. With the use of the Relay calculator and a card sorter and lots of fancy footwork, it was possible to do the job. At that time the engineers at Watson were beginning to build the electronic computer which was to become one of the bases for the future development of the IBM computers to follow. But I did not have access to that machine. However I did have access to Herman Rubin who came around to kibbitz, and to do some of the fancy footwork. At one point the sorter decided to put the cards with the digit 4 into the box for the digit 7. We counterattacked by instructing the 7 to go into the reject box. That scheme worked for a while, but the sorter replied by putting the 3 into the reject box. I think that we ended up doing some of the card sorting by hand.

At Cowles we had adjacent offices which was not exactly a blessing because Herman had a bad habit. He would come in to the office about 7 AM, pound his calculator (electric and not electronic) for an hour and then prove a few theorems for an hour, and then was ready to discuss important matters with me when I came to work. These important matters were usually how to handle certain bridge hands. Whatever I suggested was usually wrong. That did not bother me as much as the time I had to spend on bridge, a game that I never properly mastered.

I had a few friends in the Mathematics Department at the University. One of them, who had become a long term fixture, related to me how he had thought he was very smart (IQ about 180) when he was an undergraduate, until this little high school kid showed up, and obviously was more capable than most of the graduate students. Needless to say that that enfant terrible was our Herman Rubin.

While we were at Cowles we coauthored a paper, the main object of which was to show that even when not all of the standard conditions were satisfied, large sample theory demonstrated that we could still have confidence in our conclusions. I must admit that my contributions to this effort were only to translate

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