

2

Becoming a Statistician

In 1933, the coming to power of the Nazis had led to a radical change in my life. It caused me to leave Germany and abandon my dream of German literature, and to take up mathematics instead. Now, in 1942, world events once more changed my course. As a result of the war, I switched from pure mathematics to statistics and thus came under the influence of Jerzy Neyman.

Neyman's work in the 1930s had made statistics into a mathematical discipline within which I was able to function and to make reasonable progress. However, after two years as a graduate student in Neyman's Statistical Laboratory, World War II temporarily disrupted my studies. At Neyman's recommendation, I was asked to join an operations analysis group that was being formed to provide scientific and technical advice to some military command. As a result, I spent the year from August 1944 to August 1945 as an operations analyst on Guam, studying bombing accuracy. The work turned out to be rather routine, and did not involve much statistics. However, in a different way my year on Guam had an important effect on my future. It was the start of my joint work with Joe Hodges, who later became my principal collaborator.

After returning to Berkeley at the end of the war, I quickly completed my degree, and then had the good fortune to be appointed to the faculty Neyman was trying to build. As a result, I was able to stay in Berkeley, which had begun to feel like home.

6. Jerzy Neyman (1894–1981) and Alfred Tarski (1901–1983)

In 1942, as mentioned in Section 4, I moved to Neyman's statistics program. At the time I knew nothing about Neyman, and only much later learned his story and how he had come to Berkeley. He began his statistical career in Poland, but in the 1930s moved to England. Between 1928 and 1937 (partly in collaboration with Egon Pearson), he founded a new theoretical approach to statistics.

In 1937, he came to the attention of Griffith Evans, who was looking for someone to develop a statistics program at Berkeley. Evans, impressed by



Jerzy Neyman

Neyman's work and by reports of some lectures Neyman had recently given in the United States, decided that he was the best person for the job and offered him a position in the Berkeley mathematics department. Neyman arrived in Berkeley in August 1938, and he quickly established a core program of courses and a small organization consisting of a secretary and several teaching and research assistants. Within a few months, it officially became the Statistical Laboratory (the Stat Lab), with Neyman as its director.

In the fall of 1942, my initiation into the new subject began by my taking the first term of the introductory upper-division course. I did not find the course very interesting until Neyman introduced an element of considerable excitement for me personally. Out of the blue, he told me one day that he was leaving for three weeks and that he wanted me to take over the lectures of this, my first course of statistics in which I was a student. What made this assignment particularly scary was that the course did not use a text (in fact, no text existed at the time). Neyman outlined the material I was to cover, and then I was on my own. Somehow I muddled through, but was relieved when Neyman returned and I could revert from my role as instructor back to that of student.

After completing the first semester of the new program, I was faced with a basic decision concerning my future. I had agreed to give statistics a try, but had come to realize that I did not like it. It was lacking the element that had attracted me to mathematics as a boy: statistics did not possess the beauty that I had found in the integers and later in other parts of mathematics. Instead, ad hoc methods were used to solve problems that were messy and that were based on questionable assumptions that seemed quite arbitrary. Thus, at the end of 1942 I decided not to continue with statistics but to return



Alfred Tarski

to pure mathematics. The subject that interested me most was algebra, but I did not find the department's algebraist, Alfred Foster, very inspiring.

However, a new possibility had opened up. In the fall of 1942, the great logician Alfred Tarski had joined the mathematics department and was scheduled to give an advanced algebra course in the spring semester. I therefore asked Tarski whether I could work with him, not in logic but in algebra, and he agreed to accept me as a student.

Tarski was a Jewish refugee from Poland (though converted to Catholicism) who had come to New York in 1939 to attend a conference on the unity of science. He had left three weeks before the German invasion of Poland, and thus had escaped the fate of his parents, brother and most of his other close relatives, who were murdered by the Germans. (An exception was his wife, who was not Jewish, and his two children, who joined him in Berkeley after the war.)

Despite his reputation as one of the world's greatest logicians, Tarski¹ had difficulty finding a suitable job in America. One reason was that his field of mathematical logic was not a recognized university discipline. (In this respect, it was somewhat similar to mathematical statistics.) It was Evans, with his broad conception of mathematics combined with his aim to attract the highest caliber of faculty, who came to the rescue and created a position for Tarski in the Berkeley mathematics department. Tarski remained in Berkeley, where he continued his groundbreaking research and attracted some outstanding students.

¹ For a recent biography of Tarski, see Feferman and Feferman (2004).

Neyman and Tarski shared not only a Polish background (and a strong Polish accent), exceptional ability, and energy, but also enormous ambition. Both were empire builders.

Neyman, over the decade 1946–1956, managed to parlay the single appointment of himself as professor of mathematics into a faculty of ten tenure-track members, more than half his own students but greatly strengthened by a number of outstanding appointments from the outside. At the same time, he pushed relentlessly for ever-greater independence of his group, with the ultimate goal of an independent department of statistics. Such an expansion would not have been possible but for the growth of the discipline of statistics itself, with statistical methods becoming important in more and more fields of application and the resulting increase in the number of students requiring instruction in this new methodology.

Tarski too was eager to develop his own fiefdom. As with Neyman, the motives were a mix of personal ambition and the desire to provide an identity of its own for his subject, which hovered uneasily between mathematics and philosophy. He successfully obtained appointments for some of his students and for others working on his agenda, some in mathematics and some in the philosophy department. With such a faculty in place, he was able to deal with an obstacle faced by his students: they had to satisfy the stringent requirements of one or the other department, which were too specialized for the broad-based training involving aspects of both disciplines that Tarski considered necessary.

To overcome this problem, Tarski, with the help of a group of like-minded colleagues, proposed a new doctoral degree in logic and the methodology of science. The program was approved in 1958, but was listed in the university catalogue for the first time in 1964, with the faculty group administering it consisting of five members from the mathematics department, three from philosophy, and one each from oriental languages and statistics.

The outstanding parallel success of Neyman and Tarski was symbolized by the fact that at the 1954 International Congress of Mathematicians in Amsterdam, they were two of the five Americans (out of a total of twenty) invited to give one-hour lectures. They spoke, respectively, on “Current Problems of Mathematical Statistics” and “Mathematics and Metamathematics.” It was an enormous achievement of Evans to have initiated and nurtured two such outstanding and influential programs. He came close to adding a third but that one slipped away (see Section 56).

However, these developments lay far in the future when, in December 1942, I decided to work with Tarski instead of continuing with statistics. That in the end things turned out differently is due to events that occurred in the few days after my decision and before I had been able to tell Evans and Neyman about it.

After having done all the teaching of statistics himself, with only temporary help, Neyman had finally found a young mathematician, Dorothy Bernstein, who, after having turned him down earlier, accepted a tenure-track appointment for the year 1942–43. However, after one semester, she realized—just as I had—that she did not like statistics, and she told Neyman that she did not

want to renew her contract. She presumably had planned to leave at the end of the academic year, but Neyman was furious and preferred that she leave immediately.

This left Neyman shorthanded in the middle of the academic year and, not knowing that I too was planning to desert, he asked me whether I wanted to take over some of her duties: an elementary course in the spring semester that was just coming up, and the following year the upper-division course I was then taking. I was to receive a promotion to lecturer and a substantial increase in salary. There were also overtones—nothing specific, but slight hints—that this position might develop into a permanent job if things worked out.

The effect Neyman’s offer had on me—that I would be willing to give up the work that attracted me for a field that I found much less appealing—may seem surprising. However, it had its cause in my situation at the time. I felt unconnected: without a country, without a language, without a community. And now Neyman was offering me the chance to become a part of a community (his laboratory), and perhaps even the possibility of Berkeley becoming a permanent home. Germany was barred to me; neither Switzerland nor England had much enthusiasm for refugees who would settle and compete for jobs; and here I was being welcomed, encouraged, and—although I was only a beginning graduate student who had not yet taken any of his exams—being offered what amounted to a junior faculty position. It was overwhelming; how could I say no?

Although I did not know it then, I was not the only student facing a choice between Neyman and Tarski. The same issue arose at about the same time for my fellow student Julia Robinson. She had taken a course from Neyman in 1939, and two years later he asked for her as a teaching assistant. Clearly he hoped she would become one of his students. However, her reaction was the same as that of Dorothy Bernstein and myself: she found statistics “very messy, not beautiful and clear and true like number theory.”² Instead, she obtained her degree (in 1948) under Tarski, and went on to an illustrious mathematical career (see Section 5).

Julia’s and my choosing between Neyman and Tarski illustrates one aspect of a rivalry that sprang up between them. They were competing for students and resources, as well as for the reputation and visibility of their programs. This competition was undoubtedly one reason why these two Polish-American colleagues in the mathematics department were not on good terms. In addition, they were so far apart on the political spectrum—Neyman being a liberal and Tarski a conservative—that they were referred to as “Poles apart.” Finally, Tarski must have been aware that Neyman had strongly lobbied Evans against his appointment. He argued instead for Antoni Zygmund, another outstanding but more conventional Polish mathematician then in the U.S., without however having any effect on Evans’s vision.

After deciding to continue with statistics, I had no further contact with Tarski. My new career totally absorbed me. That my life’s work turned out to

² Reid, *Julia—A Life in Mathematics* (1996).



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