CHAPTER 1
ARROW'S AMBITIONS

Kenneth Arrow was never shy about engaging his past. In contrast to our other two protagonists, he gave a large number of interviews and on various occasions written sketches of different portions of his life and the development of his interests. Likely his openness to interviewers and biographers is the result of his ebullience and his lifelong interest in thinking about how ideas develop and how individuals' natures are formed. At the same time Arrow was hesitant about claiming the last word about his past. When asked to write about his "life philosophy" he began, "it is part of my life philosophy that no life can ever be examined fully and that attempts to do so are never free of self-deception. . . . Like the state in which I live, we plan and build on ground that may open beneath us" (1992, 42). Accordingly, the historian who undertakes to add another account to Arrow's self-accounts will wish neither to repeat them nor to reconstruct a self that is hidden by these self-deceptions. Thus we ask: Where did Arrow's intellectual ambitions come from?

A Precocious Boyhood in New York (1921–40)

The first ground that opened beneath Arrow during his childhood was the Great Depression. He was born on August 23, 1921. Both of his parents had been born overseas and came to the United States as infants. They were both successful academically, as his mother graduated from high school and his father from college, not a usual event for immigrants in the 1900–1914 period. His father Harry's "family

was very poor, [his] mother's hardworking and moderately successful shopkeepers" (Arrow, in Breit and Spencer 1995, 44). Arrow recalled that his father had some business successes fairly young and earned a law degree; he worked for a bank and as a result the family was fairly prosperous through the 1920s. With the Depression, his father lost his regular job and the family often had to sell household belongings in order to have money for food and rent and clothes. His father managed to do contract work for various legal firms from time to time in those years, but it wasn't until the end of the 1930s that the family began to reestablish itself economically.

Arrow was precocious. It was not simply that his school academic record was very strong, but he read extensively and deeply outside the school curriculum. He recalled that he read Bertrand Russell's *Introduction to Mathematical Philosophy* (1919) and other demanding books in philosophy, literature, and the sciences unrelated to his high school programs. On graduation he applied to Columbia University even though he was quite young (fifteen) compared to those who might have been his classmates. In an admission interview he asked the counselor about meeting the deadlines for financial aid decisions since he needed a scholarship in order to attend. The interviewer replied by telling him that he needn't bother about financial aid since he was not going to be admitted. In fact he was admitted, but the interviewer's comment had the effect of delaying the family's completing the scholarship application until after they heard about admission. By the time they realized what had happened it was too late for Arrow to apply for a scholarship. Many decades later Arrow discovered that his interviewer had been described by an historian as one of the most egregious anti-Semites in all of Ivy League education. Columbia, while not nearly as exclusionary with respect to Jews as were Harvard, Yale, and Princeton, had a *numerus clausus* (i.e., quota) arrangement to limit admissions of children of immigrant Jews living in the New York metropolitan region.

Without financial aid and with his family's own finances limited, Arrow applied to, and was accepted into, the City College of New York (CCNY). At that time, City College had free tuition for residents of the city, the result of earlier agreements and understandings

that the prosperity of the city depended upon the education of its youth independent of their financial means. Admission was strictly by merit and Arrow was certainly meritorious. Moreover, CCNY was a commuter school so Arrow could live at home. CCNY was also, for the students especially, a particularly political cauldron. As the late Irving Kristol, the neoconservative editor and writer, recalled his student years at the City College:

> Every alcove [of the City College lunchroom] had its own identity, there was the jock alcove . . . there were alcoves for ROTC people—I don't think I ever met one—and then there was the Catholic alcove, the Newman Club. There was even, I am told, a Young Republican Club, but I don't think I ever met anyone who belonged to that club and maybe they didn't exist. But pretty much our life in City College was concentrated between alcove one and alcove two, the anti-Stalinist left and the Stalinist left. And that was our world, at least our intellectual universe. (Kristol, in Dorman 1998, 46–47)

Arrow was not so politically engaged as a student but was interested in many different subjects, and early on he decided that he would major in mathematics with the long-term objective of becoming a high school mathematics teacher: "I was concerned about getting a job. I didn't look beyond college very much at that point. All I wanted was security" (Arrow, in Horn 2009, 63). In that Depression period, secure civil service employment in the New York City public schools seemed reasonable to both him and his family. As a result, in his undergraduate program he took not only a lot of mathematics courses but also courses in education, and he did student intern teaching as well. In Arrow's case, that teaching consisted of conducting preparation classes for high school students who wished to overcome their initial failure on the New York State Regents exam through a retest process. He recalled that his students were the most motivated he had ever come across: "It was the biggest teaching success of my entire life" (ibid., 64). He loved teaching but a difficulty emerged: in 1932 the education administrators had constructed a list of qualified teachers from whom future recruits to the teaching profession would

be drawn. The idea was that as teachers left the schools, those at the top of the list would be hired. But during the Depression teachers were not resigning to take other jobs. No new names, like that of Kenneth Arrow, could be added to the list until that preexisting pool of candidates had been drawn down. At least a year before his graduation, Arrow realized that he would not be able to find a job teaching high school mathematics. What was he to do?

In the summer after his junior year Arrow found work as an actuarial intern, even though he realized that if he wished to pursue this professional course after graduation, he would need to learn more statistics. Arrow thus took a course in statistics in the mathematics department, but the instructor apparently knew no statistics. However, one of the recommended books on the syllabus contained a large number of references to recent first-rate work in the field and so Arrow embarked on an independent reading program to become conversant with it all. He also did some other college work that would become important for his later success. In an interview with Jerry Kelly, Arrow spoke of how he encountered ideas in mathematical logic even though it wasn't part of any of his courses: he was "fascinated and used to aggravate my professors by writing out proofs in a very strictly logical form, avoiding words as much as possible and things of that kind" (Kelly 1987, 44). In his last term at CCNY he took a course titled "Logical Relations" with Alfred Tarski. As a result of his performance in that class, Tarski asked him to read and make necessary changes in the galley proofs of the English translation (from the German) of his textbook, *Introduction to Logic* (1941).[1]

Arrow graduated magna cum laude in June 1940, having won the Gold Pell Medal in his junior year for having the highest average in all his studies and the Praeger Memorial Medal for having the highest average in his senior year, as well as the Ward Medal for Logic. He was also elected to Phi Beta Kappa. He was not yet nineteen years old.

1 The translator, the philosopher Olaf Helmer (who will appear later in this chapter at RAND), was not a native English speaker and Tarski suspected that Helmer's translation might have been infelicitous.

Columbia, 1940–42

"By the time I graduated, in 1940, the job situation was not very good. So when I asked myself in my senior year, what do I do next, I thought—well, why not go to graduate school?" (Arrow, in Horn 2009, 64). The only place to learn advanced statistics in the New York City area was at Columbia University. There it was taught by Harold Hotelling, who had succeeded Henry Ludwell Moore. But Hotelling, as Moore before him, was not in the mathematics department but in the economics department. Since Arrow had no interest in economics, he decided that he could take Hotelling's courses as electives if he were at Columbia studying mathematics. Arrow was able, with $400 given him by his father (who had successfully borrowed it since he "knew somebody who knew somebody who was rather well off" [ibid.]), to accept Columbia's offer of admission to the graduate program in mathematics for fall 1940: "I went to Columbia because . . . well there were several problems. One was that we were extremely poor and the question of going anywhere depended on resources. Columbia had the great advantage, of course, that I could live at home, which wasn't true anywhere else. I didn't get any financial support for my first year, none at all" (Arrow, in Kelly 1987, 45). To earn some money to help the family, Arrow got a summer job after his college graduation "as an actuarial clerk. That meant doing some elementary computations, calculating premiums . . . I was very fast, I picked it up immediately. I got paid 20 dollars a week" (Arrow, in Horn 2009, 68). Beginning his graduate studies with the plan to become an actuary, he soon was taken by, and "bought" by, mathematical economics:

> I had no interest in economics. I was in the mathematics department, taking courses like functions of a real variable, and I was going to take courses from Hotelling. In the first term he happened to give a course in mathematical economics. So out of curiosity I took this and got completely transformed. The course to an extent revolved around Hotelling's own papers. . . . Anyway, then I switched to economics from mathematics. I had gone to Hotelling asking for a letter of recommendation for a

> fellowship in the mathematics department [for my second year] and he said that, "Well, I'm sure I don't have any influence in the mathematics department, but if you should enroll in economics, I've found in the past that they are willing to give one of my students a fellowship." I was bought. Incidentally, I impressed him on about the second day of the class . . . he said he was puzzled by the fact that he had never been able to produce an example of Edgeworth's paradox with linear demand functions. So I sat down and wrote out the conditions for linear demand functions to yield the paradox; these conditions were certain inequalities on the coefficients and the inequalities were inconsistent. So I came to him the next day and showed it to him. Really it was just a few lines, but from that point on he was really impressed with me. . . . Anyway, I enrolled in economics. (Arrow, in Kelly 1987, 45–47)

Hotelling's assistant at that time was Abraham Wald, who had ended up in that position through a grant from the Carnegie Foundation after a year at the Cowles Commission. We will later see the deep connection between Arrow's work with Debreu and work Wald had done in the early 1930s. It is thus worth pausing here to introduce this remarkable figure.

Wald had been a mathematics graduate student of Karl Menger's in Vienna in the late 1920s, but since he was a Romanian Ostjude he was precluded from finding a university faculty position in Austria. As a result Menger sent him to Karl Schlesinger, an economist-banker in Vienna who wished to be tutored in mathematics. It was as a result of that relationship, and Wald's participation in Menger's mathematical colloquium, that he wrote two papers on the existence of a competitive equilibrium, one using a model based on a system of exchange and the other based on a model of production and exchange; he published them both in the proceedings of Menger's colloquium (Wald 1934, 1935). Wald solved the equilibrium problem by cumbersome brute-force techniques. His approach employed a very strong assumption about household behavior, an assumption that assumed that there was only one consumer, which simplified the argument immensely. It would be one of the major accomplish-

ments of our three protagonists to construct a proof of the existence of equilibrium that followed a more natural economic logic than did Wald's proof. This point is important, and we will return to it in a subsequent chapter.

Oskar Morgenstern, a member of the colloquium, subsequently hired Wald as a researcher at the Business Cycle Research Institute he directed in Vienna (one of the several Rockefeller institutes in Europe organized on this subject). There Wald became a mathematical statistician and wrote an important monograph on seasonal variation in time series. Following the Anschluss, and Schlesinger's suicide, and in the face of Morgenstern's earlier unwillingness to nominate Wald rather than others for a Rockefeller Fellowship to come to the United States, Wald had to escape from Vienna. As a Romanian citizen, however, he needed to get travel documents there, so he traveled first to Romania and then by boat to Cuba before he was able to enter the United States with some support from the Cowles Commission then in Colorado Springs.[2]

It was during his time at Columbia from 1940 to 1942 that Arrow first met Wald while taking Hotelling's class in mathematical economics: "[As] I began to know a little more economics, I was hit by the number of extremely original papers that Hotelling had written. . . . What [Hotelling] taught was essentially the theory of the firm and the theory of the consumer. . . . I was a complete master at bordered Hessians" (Arrow, in Horn 2009, 67), a matrix of partial derivatives that was the main tool used in solving the kinds of optimization problems that Hotelling's class addressed. Arrow still hoped to secure a full-time actuarial position, and so he applied in the spring of his first graduate year for a summer student position. But even as a mathematics graduate student he was found to be unqualified:

> April 14, 1941. Dear Mr. Arrow: We've now reviewed the papers in connection with all applicants for actuarial student positions this year, and find that we have 27 candidates who have

2 The earliest discussion of Wald's role in the existence proof saga appeared in Weintraub 1983. Robert Leonard's definitive work on Menger's colloquium, which included extensive new material on Wald, appeared in 2010. The Wald of our account here is thus the Wald given to us by Leonard.

> passed the mathematical test. As we propose to employ about 6 students this year, I'm afraid a lot of good men will be disappointed. I regret to state that you were not selected to fill one of the vacancies open at the present time. Mr. [XXX], Associate Actuary, The Prudential Company of America. (KJAP, Accession 2008–0037, Early Career)

In spring and summer 1941 he wrote his mathematics master's essay, titled "Stochastic Processes," a copy of which is preserved in the Arrow Papers at Duke University (KJAP, 28, Master's Thesis). Fully engaged with statistical work while finishing his mathematics master's degree in fall 1941, Arrow sailed through his courses in economics and reached the dissertation stage very quickly. He passed his oral Ph.D. examinations by December 1941 in economic theory, public finance, statistics, and business cycles, while being certified in economic history and mathematical economics. He won a University Fellowship for 1941–42 and a Lydig Fellowship for 1942 (which he would not take up until 1946). It was in this period that he read John Hicks's new (1939) book *Value and Capital* and realized that there was a way to think about economics in a systematic fashion: "You know, after reading through the mish mash like Marshall and things like that, suddenly there was this clear, well-organized view, you knew exactly what was happening. Just the sort of thing to appeal to me" (Arrow, in Kelly 1987, 47). This would be the entry point for Arrow's first attempt at writing a doctoral dissertation, which he hoped would take a Hicksian approach to some Marshallian production conundrums.

Even though Hotelling was Arrow's primary mentor, the dominant presence in the Columbia department was Wesley Clair Mitchell, who spent his time downtown at the National Bureau of Economic Research and so was generally unavailable in the department. His course on business cycles was data based, and Arrow recalled that he appreciated the statistical care with which matters were treated. He also had a course from Arthur Burns, who replaced Mitchell as a teacher for a period of time, and from A. G. Hart, who would eventually (postwar) serve as his thesis advisor: "The place was a little bit weird, even by the standards of the time, in the sense that it was very anti-neoclassical. One of the results of this mood was that there was

not a course in price theory, at any level. . . . [Mitchell] said it was our duty to collect a lot of data. When you have collected enough data, then things will be [clear]" (Arrow, in Horn 2009, 69).

Launching a Career (1946–50)

Following the U.S. declaration of war on December 8, 1941, Arrow, who was certain to be drafted, enlisted in the hope of securing an officer's commission in the U.S. Army Weather Corps where he believed he would have a chance to use his mathematical and statistical training. He was quickly approved to attend an aviation training program at New York University in October 1942, taking "active duty" breaks from classes for rifle drill, which he and his colleagues thought rather silly. Nevertheless, he came out of that program in September 1943 commissioned as a weather officer with the rank of second lieutenant and was assigned to a weather research facility in Asheville, North Carolina; in July 1945 he was transferred to the weather division headquarters of the Army Air Force. It was during that time in Asheville that he wrote a memorandum that later, in 1949, became his first professional paper ("On the Use of Winds in Flight Planning" in the *Journal of Meteorology*). That paper presented an algorithm for taking advantage of winds aloft to save fuel on North Atlantic air crossings, an idea that was not acted upon by the military at that time but became the canonical practice for North Atlantic flight paths in the postwar period. He separated from the service on February 14, 1946, with the rank of captain.

Returning to Columbia in April 1946, Arrow needed to think about employment. During that summer he was hired as an instructor in economics at City College, and in the fall Columbia gave him an assistantship in statistics. He also returned to the problem of finding a dissertation topic. His first thoughts were connected with a course he had taken with John Maurice Clark that had examined the new work of the 1930s by Joan Robinson and Edward H. Chamberlin on imperfect and monopolistic competition. But he had other ideas as well. His curriculum vitae at the time recorded that he expected to complete, by February 1947, a thesis titled "Stability of Equilibrium in a Certain Microeconomic System," to be supervised by Abraham Wald with prospective dissertation committee members Clark, Arthur Burns, and Robert Haig, an early influential analyst of the U.S.

federal income tax. Hotelling had already left Columbia for the opportunity to create the first U.S. department of statistics at the University of North Carolina at Chapel Hill. This thesis appeared to be an extension from exchange to production of Hicks's material on stability of markets that had appeared in his 1939 book.

Arrow made very little progress on that or any thesis and at a number of points was quite prepared to give it all up, thinking that he would never be able to accomplish anything original: "Everybody thought I was very good. I thought I was very good as a student, too, but I wasn't at all sure I was capable of original work" (Arrow, in Horn 2009, 72). His continued interest in becoming an actuary was in conflict with his interest in a career of research in mathematical economics. Two letters to him in this period bracketed the possibilities:

> July 10, 1946. Dear Mr. Arrow, both Professor Hotelling and Professor Wald have mentioned your name with strong recommendations in connection with our search for a mathematical statistician-economist to succeed Ted W. Anderson in work at Cowles Commission research program. Both have indicated that you might wish to complete your Ph.D. work before continuing with other research work. However this may be, we would like to make your acquaintance and exchange ideas. . . . Sincerely yours, Tjalling Koopmans, Cowles Commission. (KJAP, 22, Koopmans)

Tjalling Koopmans, who would become a central figure in bringing our three protagonists together, at the time of writing the letter was the fugleman of Jacob Marschak, then research director at the Cowles Commission. How would Arrow respond given that he still planned to become an actuary? A second letter written by Hotelling on August 14, 1946, cleared matters:

> Dear Mr. Arrow: A letter from [a Mr. Cody] of Equitable Life inquires about you, stating that you have applied for a position in the actuarial department. In reply I recommended you highly. However, I'm inclined to sympathize with certain misgivings expressed by Mr. Cody as to whether you would really be satisfied with the kind of job in which you would find

> yourself. Practical actuarial work usually involves only elementary mathematics and becomes routine at times. As I wrote Mr. Cody, I imagine that you, unlike other men of active mind, high ability and extensive preparation, would be content with routine work for a time but would eventually become dissatisfied unless given challenging problems and greater responsibilities. . . . I had thought that your fellowship still had some time to run and that you would not be looking for a job at this time; otherwise I would have put you in touch with various positions in university teaching and also in practical statistics that have been opening up at a great rate. If you will let me know about your plans and desires with respect to further study, a job in the near future, and ultimate ambitions, I may be able to help. There are several very attractive statistical positions open in universities and industries. (KJAP, 22, Hotelling)

From then on Arrow stayed on the academic track. Hotelling suggested to Arrow that he apply for a prestigious fellowship outside the graduate economics program. In his application for a Guggenheim fellowship dated October 12, 1946, Arrow indicated that, as of that date, he had completed all requirements for the Ph.D. in economics except for the dissertation. He described his proposed fellowship project as: "Construction and possible empirical verification of a theory of major economic magnitudes based on a theoretical analysis of economic behavior of individual units of decision (Firms and Households); and the application of that theory to the formulation of ends and means in government policy" (KJAP, Accession 2008–0031, Guggenheim). This description of the project sounds much like the prospectus for the early eighteenth-century South Sea Company, one of whose investments was to be in "A company for carrying on an undertaking of great advantage, but nobody to know what it is" (Mackay 1980, 55), and it bore no coherent relationship to the thesis proposal that he hoped Wald would supervise. While the proposed study involved continuing his research for his doctoral dissertation at Columbia, Arrow also noted, "it is to be carried on using the facilities of the Cowles Commission for Research in Economics at the University of Chicago. This research group offers, I believe, a unique environment in which to carry on the project outlined"

(KJAP, Accession 2008–0031, Guggenheim). But by letter on March 24, 1947, he learned that he had not been awarded the fellowship.

The earlier letter from Koopmans had profound consequences for Arrow's career. At Koopmans's suggestion, Arrow went to talk with Koopmans at the American Statistical Society meeting at Cornell University: "There I got to know Koopmans. I knew that he had published a paper on a very interesting statistical point, and it was signed Pen[n] Mutual Life Insurance Company. So I asked him how he found working for an insurance company. And he just said, with his strong accent: 'Oh no, there is no music in it'. And the minute he said that, there was no further question in my mind about my joining an insurance company" (Arrow, in Horn 2009, 75).

After his meeting with Koopmans, he reconsidered taking a job at Cowles. That connection with Cowles would place him in the emerging postwar network of mathematical economists and economic statisticians. If he was good enough for Cowles, surely he was good. The proof was that even as he was about to be turned down for the Guggenheim, Stanford came calling.

> [February 5, 1947] Dear Mr. Arrow: Mr. Bowker has suggested to me the possibility that you might be interested in the vacancy in this department in the fields of advanced statistics and econometrics. The vacancy in question was created by the fact that Professor Allen Wallis left Stanford this year to accept an appointment at the University of Chicago. As a result of Mr. Bowker's suggestion, I have corresponded with Professor Wald and Professor Hotelling, and they both recommend you very warmly. I understand however that you're accepting a position with the Cowles Commission very shortly, and I realize that consequently you may not be free to consider the opportunity we have in mind in any event. It occurs to me however that you might be in a position to accept employment beginning, for example, January 1, 1948. . . . The position I have in mind would probably be one of rank of acting Assistant Professor, with the prospect of reappointment on a regular basis after one year if things worked out right." Signature B. F. Haley, executive head of the Department of Economics, Stanford University. (KJAP, Accession 2008–0031, Stanford)

Haley had understood correctly about Arrow's possible opportunities. But Arrow decided to stay on at Columbia for that year to work on his dissertation. In his published interview with Kelly, Arrow recalled that "at this time, I received an invitation to the Cowles Commission. At first I postponed a move because I was trying to finish my Hicksian dissertation before I went there, but I finally settled on finishing it there" (Arrow, in Kelly 1987, 49). In response to Kelly's questions about whether it wasn't unusual to leave graduate school before finishing a dissertation, Arrow replied, "You know, my knowledge of what was typical wasn't very good. The people I knew were at Columbia. I didn't know what was going on at Harvard or Chicago. It was really very provincial" (ibid.). As Kelly refocused his questions then about Arrow's invitation to Cowles, Arrow said,

> [T]hey came around and asked Wald and he recommended me. While he was primarily trained as a statistician, nevertheless he was interested in economics. There weren't many in that category and so they asked him. . . . [W]hat they really wanted me to do was to work on a statistical problem but it was a freewheeling place. At the moment, the emphasis was on the econometrics of large-scale models. So called "large" . . . three equations, five equations. Larry Klein ended up with a 20 equation model . . . but I was there to do anything I pleased and I was very obviously interested in theory. There was a feeling that theoretical foundations were also an essential part. Finishing my thesis could fit into this. . . . I really spent a year there not doing much of anything, to tell you the truth. I wrote a few tiny papers, none of which amounted to anything. I was a great contributor to discussions: argumentative, finding exceptions, errors and counterexamples. But I really felt very discouraged. (Arrow, in Kelly 1987, 50)

It was in the discussions at Cowles that Arrow developed a sense of the importance for scholarly productivity of being in an academic research community. He finally gave up his idea of pursuing a career outside academia.

> Dear Mr. Bennion: Regretfully, I feel that I cannot accept your offer of a position with the Standard Oil Company at this

> time. The major reason for my decision is the feeling that the progress in econometric model-building can best be served by continuing to work with a group whose interests and approaches coincide fairly closely with my own. Here, there is, in effect, a common language, which greatly facilitates the mutual criticism and exchange of ideas so important to scientific progress. Econometric research of the type I have been and will be doing is, like the work in the natural sciences, becoming more and more of a cooperative matter, requiring teams of individuals trained along similar lines. In the future it is hoped that the development will reach the stage where the results can be applied by individuals or businesses; but that time is not yet. (July 1947, KJAP, Accession 2008–0037)

The argument he was making—that economic research in the postwar period was becoming more "scientific"—was specific in its vision of that new economic science. It was not going to be moved ahead by isolated creative individuals writing books. Instead scientific economic knowledge would grow from work by research teams acting cooperatively. We have not found any other contemporaneous account of this change in economic research that lays out the prewar to postwar shift so clearly.

In October 1947, Arrow took up a one-year position as a research fellow at Cowles. After his fellowship was renewed for 1948–49, Chicago quickly realized Arrow's gifts, and on April 15, 1949, he received a letter from that university retroactively adjusting his appointment from October 1948 to a one-year position as an assistant professor of economics in the department of economics and as a research fellow (paid by the Rockefeller Foundation) in the Cowles Commission for Research in Economics through June 30, 1949, with a salary of $3,350 for the period. Moreover, for the period January 1, 1949, through May 31, 1949, he was to be paid for one-third time services on a contract between RAND and Cowles and for two-thirds time service by the university. He was twenty-seven years old, and his career was launched.

Nevertheless, Arrow was troubled by the gap between his ambitions and his performance up to that point. He was thrilled by the

activity and energy of his Cowles colleagues but found he was unable to engage with a sustained project: "I felt very unhappy with my lack of progress. I was wondering whether I was really capable of having an original thought. I could critique, I was very smart, I could take the best economists and find what was wrong with what they were doing. But all this is part of being a bright student" (Arrow, in Horn 2009, 75). He was launched, yes, but personally he felt he had accomplished little or nothing.

We can pause here to note how curious this all appears in retrospect. In our modern world of economics, the path to a university career of scholarship and teaching requires extensive graduate training, competing with others for a very small number of jobs, and offering potential employers at least several papers in print or under revision for publication. It certainly requires a Ph.D. before one can begin a regular appointment. Arrow had none of these credentials. He was a young man of great promise, seen as having immense potential by both Harold Hotelling and Abraham Wald. He was energetic, enthusiastic, and verbally facile. But he was Jewish, which at that time made it impossible for him to find employment at schools like Harvard, Yale, and Princeton.[3] The Cowles Commission represented an academic path for refugee scholars and American Jews. Even though it did not have a sign saying "Exiles" at its front door, its motto "Science Is Measurement" attracted technically sophisticated scholars who were otherwise excluded from Harvard, Yale, and Princeton. It was at Cowles that Arrow's career was launched despite his lack of doctoral credentials and his being Jewish.

It was also there that love flourished. As Ross Starr wrote in *The New Palgrave*, "Jacob Marschak, the Cowles Commission Research Director, arranged for the Commission to administer the Sarah Frances Hutchinson Cowles Fellowship for Women pursuing quantitative

3 Wald at Columbia was a special case, but while postwar Columbia had eased its *numerus clauses*, it faced a threat from the New York City government of an investigation into its restrictions on hiring Jews (Karabel 2005, 210). The assimilation of European Jewish émigrés in the 1930s had not gone well even as non-Jews like Morgenstern and Karl Menger found a variety of opportunities (Weintraub 2014). In New York, the New School for Social Research was called the "University in Exile" and was home for a period to Marschak and Wald and others.

work in the social sciences. . . . The fellows were Sonia Adelson (subsequently married to Larry Klein) and Selma Schweitzer. Kenneth Arrow and Selma Schweitzer were married in 1947" (Starr 2008, section "Personal and Intellectual History," paragraph 7).

Social Choice (1948–50)

Arrow's dissertation remained in limbo. He was unenthusiastic about the project, believing that it represented no original thinking or new and interesting analytic techniques. Without thesis supervisors pushing him to complete the exercise, he found the scintillating seminars and discussions at Cowles reason enough to put it off. His interests were eclectic, and his colleagues were doing fascinating work: "It was a terrifically exciting group, with Leonid Hurwicz, Lawrence Klein, Koopmans, Marschak, and some others" (Arrow, in Horn 2009, 75). Then, quite by accident, he started down a path that would bring real results in a fairly short period of time.

> Once, in lunch, we were talking about politics, left parties and right parties, and I remember drawing on a piece of paper the idea that a voter might have preferences over the parties. . . . So I wrote this thing down and started looking at the question of majorities. It's really hard to describe it. All I can say, is once you've seen it, it's obvious; it takes an hour or two. If you ask the question, the answer's fairly obvious. (Arrow, in Kelly 1987, 50)

The set of techniques that he knew to draw upon came from to his undergraduate course with Tarski on "logical relations." He began to realize that those relations were simply mathematical orderings and the apparatus of mathematical logic could be employed almost in full in understanding what was then being developed, at Cowles by the mathematicians Israel Herstein and John Milnor (and the next year by Gérard Debreu) and others, as utility representations of preference orderings.

> I spent a day or two working it up as a formal proof. And in my usual way, I sort of stalled about a month on writing it up for publication . . . but in a sense it didn't matter because within a month I picked up [the new issue of the *Journal of Political*

> *Economy*] and there's the paper by Duncan Black (1948) that had exactly that idea. (ibid.)

This was a shock. Arrow believed that this was the first time that he had had a very good, new idea. He wrote the paper expecting that it would at least salvage the Cowles year in which he had not produced anything of importance. He had even thought of using the idea as the major result of his dissertation.[4]

> This I still do not understand. I do believe in multiple discoveries, there are lots of them but usually it is because the ground has been prepared. Something happened, perhaps for some other reason someone developed something, and the next step was at least reasonable, it might not have been obvious, but at least reasonable. But what Black and I did could have been done 150 years earlier, there was no mathematical development, there was no intellectual development. Perhaps game theory in a very general way could be credited. . . . Why Black and I hit on this at about the same time, I really do not know. It was actually sheer chance. He could have done it a year earlier, I could have done it a year earlier. . . . I was really disappointed in not having priority since I had not published anything yet that was worthwhile. I thought at least to get some little note and then I was scooped. (Arrow, in Feiwel 1987, 192–93)

So at the end of that first year at Cowles, "I was still this brilliant person, very active in the seminars, but I really didn't get anything done. Another year [had] passed. Everybody expected me to amount to something. I was the only one who was doubtful about this" (Arrow, in Horn 2009, 76). Dissatisfied with his productivity and

4 Simply stated, the problem concerned a well-known paradox of voting. If there are three individuals, call them 1, 2, and 3, and three objects of choice, call them A, B, and C, suppose 1 ranks them in order as A then B then C. Suppose 2 ranks them as B then C then A. And suppose C ranks them as C then A then B. Majority voting on A versus B, B versus C, and C versus A will result in A ranking higher than B which ranks higher than C, which ranks higher than A! That's the paradox, attributed to the Marquis de Condorcet and called the Condorcet Voting Paradox. Black, and Arrow, proved that a simple assumption about preferences could rule out any possibility of the paradox's occurring.

newly frustrated by his having been "scooped" by Black, he decided to take advantage of an unforeseen opportunity.

> [T]hen that summer [of 1948] I went to the RAND Corporation—again through sheer accident. My wife, whom I met as a graduate student at Chicago, had previously worked in the Agriculture Department. She arrived there as a clerk and became a professional, a statistician. Her boss was a very distinguished mathematical statistician named M. A. Girshick. So I was friendly with Girshick who had gone to the RAND Corporation when it was started. . . . One of the things RAND was doing was inviting large numbers of visitors for the summer so Girshick urged me to come. Summer in Santa Monica didn't seem like a bad idea to me and it turned out to be far more intellectually exciting than anything I had planned because the halls were filled with people working on game theory. Everybody was fooling with zero-sum games, how to calculate them, the fundamental definition of the concepts; it was work at the conceptual level and at the technical level. (Arrow, in Kelly 1987, 51)

That 1948 summer at RAND was the start of his annus mirabilis. Game theory was much in the air, and he spent some of his time in Santa Monica learning about both the theory and its applications. One day, talking with Olaf Helmer (Tarski's translator) about games and politics, Helmer raised the following question: Suppose the players in a two-person game were the United States and the Soviet Union. Payoffs in that political game were to be based on the players' preferences. But the theory of preferences in economics and game theory was a theory of individuals' choices. What then could be the meaning of "the Soviet Union's preferences"? How did a group's preference connect to the preferences of the individuals who made up the group?

Arrow said that he could give an answer based on Abram Bergson's social welfare functions (1938) that ranked conceivable social states from lowest to highest, thus addressing issues of collective valuation of those social states. Helmer asked him to write it up as an expository piece. As Arrow began doing that, using the notion of "R" as an

individual preference ranking of alternatives for a particular individual, he thought that "One natural method of taking a bunch of R's and putting them together would be by pairwise comparison by majority voting. And I already know that was going to lead to trouble! So I figured, well, majority voting was just one of a very large number of possibilities [of combining people's preferences], you just have to be more ingenious. I started to write possibilities down" (Arrow, in Kelly 1987, 53).

But as he worked on this, he began to realize that simple trial and error was not going to lead to any result, so he began to categorize the characteristics of various rules for aggregating individuals' orderings into a group ordering. He required that both kinds of orderings should have the same properties so that group orderings (or preferences) could function exactly as individual orderings did in game theory, which would allow one to talk intelligibly about, say, "American preferences" with respect to political outcomes. After a period of weeks he began to realize that none of the sensible aggregation rules was going to work and that only in the case of a dictatorship, where the dictator's ordering *was* the group ordering, would a group rule be unambiguously derived from individual orderings. It seemed that, without dictatorship, the problem would be impossible to solve.

> Finally, one night when I wasn't sleeping too well, I could see the whole proof, you know after playing around with it for a while. . . . I felt this was very exciting. I thought "This is a dissertation." You know, it's a funny thing. One of my problems had been feeling that one has to be serious and every time I'd thought about these voting questions they seemed like amusing diversions from the real gritty problem of developing a good descriptive theory. . . . But when I got the result, I felt it was significant. I really did. (Arrow, in Kelly 1987, 54)

The theorem was both unexpected and powerful: without dictatorship, no "sensible" method of aggregating individuals' preferences could lead to a coherent group preference. Put another way, it was impossible to aggregate individuals' preferences to generate the collective's preferences without violating some notion of "rational" collective preferences. Arrow presented the result that summer of 1948

at RAND and received a lot of useful suggestions from individuals there like Girshick, Abraham Kaplan, David Blackwell, and John McKinsey. On his return to Chicago that fall as a half-time assistant professor teaching statistics in the economics department, he presented the material in six discussion papers over a number of seminars (Hildreth 1986, 92). In the published monograph version (1951), he would thank Koopmans, Herbert Simon, Franco Modigliani, (statistician) T. W. Anderson, Milton Friedman, and (political scientist) David Easton for their helpful comments. He also presented the work at the December 1948 meeting of the Econometric Society in Cleveland.

> I remember Larry Klein was in the chair and Melvin Reder was reading another paper in the same session. My recollection was that there were 30–40 people in the room. I distinctly remember that in the audience was this contentious Canadian, David McCord Wright, who objected because among the objectives, I hadn't mentioned freedom as one of the essential values in social choice and apparently he went out of the room saying that Klein and Arrow were communists—this was quoted to me by at least Kenneth May who was present. . . . [Right] after the summer I developed this, on the way back to Chicago, I stopped at Stanford to be interviewed for a job. . . . Girshick had meanwhile moved to Stanford to contribute to starting a statistics department there. He was their star and he wanted me to join him. The economics department there had already in fact made me an offer a year earlier . . . I was appointed without a Ph.D. In fact, I got tenure without a Ph.D.[5] (Arrow, in Feiwel 1987, 56–57)

Through his work at Cowles and RAND, Arrow had quickly built up working relations with many members of the emerging mathematical economics community: "I already had one invitation from

5 This was technically correct since in those days, as Arrow recalled, one needed to have the thesis printed, and the degree could not be awarded until that event occurred. The work was done, and defended in January 1949, but he already had been hired by Stanford beginning in fall 1949, before the printing took place.

Stanford, but I wanted to get something done [before I accepted the Stanford offer]. . . . The editor of the *Journal of Political Economy* asked me to write an article for the journal. That was something I hadn't thought about. From that point on I became much more productive . . . I didn't go back to social choice. I didn't continue that line. I became much more creative" (Arrow, in Horn 2009, 77). He had tasted real professional success, and that success led him to other new ideas and new problems to solve. At the end of that Cowles year in June 1949, at age twenty-eight, he moved to Stanford where he would remain, with the exception of his midlife move to and then from Harvard, for the rest of his career.