

A Bibliographical Autobiography*

If I were to ask myself (as far as I recall, nobody else has ever asked me) what were the two most fundamental principles of the universe, I would respond, first, with what I have sometimes called "Boulding's First Law" — that anything that exists must be possible. The second principle, which I have sometimes called "D'Arcy Thompson's Law", as it was through reading his great book *On Growth and Form*¹ that I first began to appreciate it, is: everything is what it is because it got that way. I exist, therefore, because I must have been possible, and I am what I am at the age of 79 1/2 because I got that way. These two principles have two corollaries. The first is that not everything that is possible exists (one should add, perhaps, unless the universe is infinite). The second corollary is that everything that is did not have to be that way. As a result of the thinking I have done about evolution, I have become a profound indeterminist. Deterministic systems, like the celestial mechanics of the solar system, are actually very rare. Today we are even beginning to recognize the existence of chaos in the solar system. Everything that is here today exists because there was a potential in the universe for the process that made it what it is. But only a very small part of potential is actually realized, and what is realized is strongly influenced by random events that have happened along the way. The actual course of evolution is dominated by the exact time at which extremely improbable events happen, like DNA and *Homo sapiens* on this planet. Each of us human beings are highly improbable events. I once calculated that each of us is one out of about 8 trillion possible different human beings that our two parents could have produced with 23 chromosomes from each. This may be too large a

* Contribution to a series of recollections on professional experiences of distinguished economists. This series opened with the September 1979 issue of this *Review*.

¹ D. THOMPSON, *On Growth and Form* (New York and Cambridge, England: Cambridge University Press, 1962).

figure, for there may be some combinations of chromosomes that will not develop into a person (we don't know how many), but the figure is still very large. Multiply this by all our ancestors, back to the first forms of life, and it is clear that we are very improbable indeed. It is a nice philosophical question whether time is infinite. Certainly our image of time is. Even if our universe began with the "big bang", we can imagine a moment before that, and if it ends in some final catastrophe, even the moment after that. Whether our image of time corresponds to a reality, however, is another question, probably an unanswerable one.

I was conceived in 1909, born January 18, 1910, at 4 Seymour Street, Liverpool, England, Europe, the world, the solar system, the universe. I remember writing out this address when I was a boy, which indicated perhaps an early interest in general systems and how we identify particular systems within them. My father and mother were active Methodists. I was an only child, indeed an only grandchild in our Methodist family. My father was a plumber who had a little business of his own in a shop behind the house. We lived right in the center of Liverpool. My only playground was the city street. Both my parents were self-educated and very intelligent. My father and my maternal grandfather were lay preachers in the Methodist Church ("local preachers", as they were called in England) and would go out to preach in little churches on what was called the "circuit" several times a year. My father was head of the Sunday School. My grandfather was a blacksmith and a very striking character. My maternal grandmother was full of wise sayings. The fact that I was probably the first member of my family to go beyond elementary school says very little about my genes, but a great deal about the social system that my ancestors lived in.

My father met my mother in a little Methodist church in London. My mother was a lady's maid in a home where my father had been sent to do some plumbing work, again a remarkable improbability. My mother told me that when she told her mother that she wanted to marry a man from Liverpool, her mother said, "Oh, you can't do that. That's as bad as going to America!" In many ways Liverpool was an American city. Its oldest building dates from the 1780s. Of my playmates on the street where we lived, only one or two were from English families. The rest of the families were Irish, Jewish, one or two Belgian refugees, even one black family. It is not surprising that I became an American so easily.

I grew up in a very adult environment. Our house, being so centrally located, was constantly full of people. My mother's two sisters moved to Liverpool with their husbands. There were various cousins. My maternal grandparents also moved to Liverpool when they got older. For several years they lived in a little apartment at the top of our house, which my father fixed up for them. Visiting Methodist preachers frequently dropped in. The conversations around the dinner table were interesting. My father was a liberal and great admirer of William Ewart Gladstone (hence my middle name Ewart). One of my uncles was a conservative and the other a Labor Party man, the manager of a cooperative store. So the political discussions were often quite lively. We loved to play dominoes, a form called "Threes and Fives", which involved a good deal of mental arithmetic. Cards and alcohol, of course, were unknown. The pub on the corner was practically regarded as the entrance to "Dante's inferno".

Some experiences during the First World War had a profound effect on me as a child. Some medical problem exempted my father from serving in the military. But my uncle, of whom I was very fond, came back from the trenches crawling with lice, with an expression in his eyes that I can still see. He went up into our bathroom, threw all his clothes out the window into the yard below, and my mother went out with a hot iron and killed all the lice. My closest friend and playmate lived next door, a Jewish family. He had an older brother who was killed in the war. Upon learning of his death, his mother came over in hysterics. I even recall being horrified at a toy I got, with wounded soldiers in little stretchers. It was about this time that I began to stutter, a speech defect which has persisted all my life, but which has interfered surprisingly little with my career as a teacher and lecturer.

My parents were very concerned about my education. At the age of about nine they took me out of the poverty-stricken, crowded, Church-of-England school at the top of the street. Thereafter I walked a mile to an excellent, originally Unitarian, school, with very fine teachers, who spent a lot of time tutoring and preparing me for the big examinations for a scholarship at the Liverpool Collegiate School, to which my parents could not have afforded to send me. Liverpool Collegiate was a day school, an elegant Victorian, Gothic building, now alas falling into ruin. It was only a ten-minute walk from where I lived, where again I had some

excellent teachers. In those days, one passed a school certificate examination in the fifth form and then went into the sixth form for three years, preparing to take scholarships at Oxford and Cambridge. I had to choose between three sections: Classics, in which one did practically nothing but Latin and Greek; Modern, in which one did modern history and languages and English; and Science, in which one did nothing but physics, chemistry, and mathematics. I was inclined to go into the Modern, as at this time I was writing poetry and essays, but my mathematics teacher, also a Methodist, came down to our house and persuaded me to go into Science. I did three years of mathematics, physics, and chemistry, and ended up winning a scholarship in chemistry at New College, Oxford.

The religious aspects of those adolescent years were also important to me. At the age of about 14, feeling as a result of my Methodist upbringing that I wanted to model my life on the teachings of Jesus, and remembering my experiences of the First World War, also perhaps the sense afterwards of having been totally deceived and betrayed by the propaganda I was exposed to at that time, I was flooded by a strong feeling that if I was going to love Jesus, I could neither kill anybody nor participate in war. This eventually led me into the Society of Friends (Quakers). The Quaker meeting house, again, was not far from where I lived (there is something to be said for growing up in the center of a city, where everything is close, even if the surroundings are slummy). I found myself immediately at home in the unfettered silence of the Friends Meeting and the Quaker community around the world has been very much a part of my life ever since.

My first year at Oxford I think was one of the most unhappy times of my life. The class structure in England in those days was so rigid that being a Methodist from Liverpool at Oxford was very much like being a black from Mississippi at Harvard. I found a circle of friends, similar outcasts, but I was very homesick for Liverpool. I "read" and studied chemistry that first year with a tutor who had rather lost interest in the subject. I was bored by the laboratory work. Professor Frederick Soddy, the leading Oxford chemist of the day, a Nobel prize winner, had also rather lost interest in chemistry and his lectures tended to be devoted to his peculiar brand of economics. At the end of the year, therefore, I decided that chemistry was not for me and wrote a letter to the warden of New College, asking if I could keep my scholarship and do Politics,

Philosophy, and Economics. With great generosity the College allowed me to do this. So at the end of the term in June I went to Lionel Robbins, who was the economics tutor there, just leaving to take up his professorship at the London School of Economics. I asked him what I should read during the summer if I was going to study economics. He said, "Well, you might read Marshall, *Principles of Economics*; Pigou, *The Economics of Welfare*; Cassel, *The Theory of Social Economy*; and Hawtrey, *The Economic Problem*". I got these books out of the library, went back to Liverpool for the long summer vacation (I was too poor to go anywhere else), read them, and then went on to become an economist. My mathematical background (I had done quite advanced calculus in high school) enabled me to appreciate Cassel, who expounded the Walrasian equations. Marshall gave me a good feeling for price theory, and Pigou, for social implications. This was in 1929. The Great Depression had really started in England under Churchill in 1926. I was horrified by the unemployment problem, which I had seen first hand not only in Liverpool but in South Wales. As an earnest young man wanting to save the world, I was pretty sure chemistry wouldn't do it. At that time the great problems of the human race seemed to be economic.

When I went back to Oxford in the fall of 1929, Henry Phelps Brown was my economics tutor. He didn't really know very much economics then. He had just gotten out of history, but that very fact I think made him a good tutor. Oddly enough, he went off to the University of Michigan at Ann Arbor to learn some economics and wrote me a letter from there. Little did I think at that time that I would later spend 18 years of my life in Ann Arbor. During my second year at Oxford I had Maurice Allen of Balliol College as my tutor, who later became an economist with the Bank of England. There was a joke going round that his qualifications were that he had never published anything, so he must be discrete. He was a good tutor. That year I won a small university scholarship. After graduating with First Class Honours, I spent another year at Oxford doing what was supposed to be graduate work, which consisted in seeing my advisor every two or three weeks. He would ask me how I was getting along. I would say "Fine", and that was about it. I did write a thesis on capital movements, which has since disappeared, probably just as well. The library facilities at Oxford at that time were so unbelievably bad that I had to go to the London School of

Economics if I wanted to read something. That year I applied for a fellowship at Christ Church. Confidential letters written on my behalf were all sent to me by mistake and they all said in effect, "This is a bright boy, but he is not one of us", which I wasn't. I always felt something of an alien at Oxford with my Liverpool, Methodist background.

It was in 1931, my last year as an undergraduate, that I wrote a little article, "The Place of the 'Displacement Cost' Concept in Economic Theory", which I sent to John Maynard Keynes, editor of *The Economic Journal*. He accepted it after writing some extensive comments suggesting revisions.² It was a most extraordinary piece of courtesy towards an unknown Oxford undergraduate. In that year also came Keynes' *Treatise on Money*, which I read with great excitement, especially the historical chapters at the end. It gave me a wholly new view of history, which up to then had never made much sense to me as it was taught in England. Herbert Stein in his book, *The Fiscal Revolution in America*, quotes both myself and Samuelson on the impact of Keynes on us as young men, each of us quite independently using Wordsworth's famous line on the French Revolution, "Bliss was it in that dawn to be alive, but to be young was very heaven!"³ I used this quote in reference to the *Treatise on Money* in 1931; Samuelson, in reference to the *General Theory* in 1936. Stein points out that we were both 21 at the time and our "bliss" may have had more to do with being 21 than with Keynes, but it does I think capture the sense of excitement that Keynes was producing even in 1931, though unemployment still remained a deep mystery. Alfred Marshall was still the bible of economics. Econometrics had hardly risen above the horizon, and the world as presented by economists seemed a very long way from the real world of unemployment and poverty. Even though the Great Depression was in full swing during the years I was at Oxford, I recall very little attention paid to it. We somehow lived in another and earlier world and were surprisingly insensitive to the economic problems of the day.

In 1932 I won a Commonwealth Fellowship to the University of Chicago. Edward, the Prince of Wales, was the patron of the

² K.E. BOULDING, "The Place of the 'Displacement Cost' Concept in Economic Theory", *Economic Journal*, 42, 165 (March 1932): 137-141.

³ H. STEIN, *The Fiscal Revolution in America* (Chicago: University of Chicago Press, 1969), p. 162.

Commonwealth Fund, which awarded the fellowship, so the new fellows went to St. James's Palace to be blessed by him. He asked me where I was going. I said "Chicago". He responded, "Don't get bumped off"! That was my final blessing. I sailed from Liverpool for America on the S.S. Laconia in September with eight other Fellows. My family and friends all waved me off at the landing stage with yellow dusters. This was the last time I ever saw my father. He died a year later while I was in the United States. We travelled first class. Professor Joseph Schumpeter was also on the boat. The trip in those days took nine days, so we got quite well acquainted. I had with me the thesis I had written at Oxford, which he read and we discussed. When we landed in New York I went up to Albany on the old Hudson Day Line. I was quite overwhelmed by the beauty of the scenery. My impressions of America had been largely drawn from cowboy movies, so I thought it was treeless. To find these enormous forests, and even these forested cities, was a great revelation. We got the train in Albany to Chicago. Coming in through Gary, Indiana, I must say I wondered what we were coming to. The University, with the Midway and its Oxonian gothic buildings, was another revelation.

My advisor was Professor Jacob Viner. I took my Oxford thesis to him. He flipped through it and said, "Oxford, no footnotes". Then he suggested I should take a Ph.D. After he described what I would be required to do, I decided I had much better things to do with my life. At that time, of course, I expected to go back to Britain and I already had my First from Oxford, which at that time was a sort of entry ticket to academic life. So I decided to use these two years in Chicago to learn, read, and write what I wanted, not what the University wanted. I did, however, learn a good deal from Professor Henry Schultz, one of the founders of econometrics. I studied also with Professor Frank Knight, who by that time had lost interest in risk, uncertainty, and profit, but his classes nevertheless were enormously stimulating, though I thought rather disorganized. Those were the days when it took a whole afternoon to work out regression and correlation coefficients on what we still called "adding machines". I remember Professor Schultz coming around us, sympathizing with our labors, and saying, "I know this is very boring, but you are getting familiar with the data", which we were. Today, of course, the computer gets familiar with the data but nobody else does. Henry Schultz was a remarkable teacher. His

econometric skills never diverted his attention from the real world, and he always looked on econometrics as a servant rather than as a master. He was killed in an automobile accident in the middle thirties. I have sometimes thought that econometrics might have been a little different if he had lived, for he was not the sort of man who would substitute technique for thought.

There was no "Chicago School", of course, in those days — the leading figures did not particularly share a common ideology — but the general atmosphere was very exciting. Albert Hart was a fellow graduate student with me and we became very good friends. He had one of the most fertile minds I have ever known. Even so it is surprising, looking back on that experience in the depth of the Great Depression in 1933, how little understanding there was of what was going on. People talked about an "economic blizzard" as if it was just meteorology. The equilibrium concept had so dominated economics that nobody came up with the idea that what we were really facing was a profound disequilibrium process of positive feedback. Cybernetics, after all, had not been invented at that time. I recall a headline in the Chicago Tribune, soon after I arrived, saying "No construction in Chicago this week", apparently not even a dog kennel. But nobody seemed to ask why. Of all the people I knew in Chicago, Henry Simons had the most insight, with his critique of the banking system and proposal for 100 percent reserve. But nobody listened to him very much. He was a very discouraged man.

Another surprising thing, looking back on it, is how little attention was paid to Irving Fisher's work. Even at Oxford I had been enormously impressed with Irving Fisher, who still seems to me the greatest economist America ever produced. And he was one of the few people who had real insights into the role of interest in the financial system in the Great Depression.

During the summer of 1933 two friends and I travelled around the United States in an old open Buick, in the middle of the Dust Bowl, in the depths of the Great Depression. I am struck, looking back, at how invisible the Great Depression was to us as graduate students. We actually did very well during this time, as our stipend levels had been set in the twenties and had not been changed, in spite of the deflation, a good example of how the deflation had upset the relative price structure. It took me a long time to become as rich as I was when I was a graduate student with no dependents, getting about \$3,000 a year. While at the Grand Canyon I got a cable saying

that my father had died. I went back to England to clear up his little business. I have sometimes said I learned more economics in that experience than from all my teachers. He had been insolvent for at least 20 years. The bankers and creditors had kept him going in the hope of a better year next year.

That fall I spent working with Professor Schumpeter at Harvard, especially reading the Austrians and Bohm-Bawerk, concluding, I am afraid, that they were another example of the failure of equilibrium theory to deal with economic reality. Unfortunately, I came down with a spontaneous pneumothorax and had to spend about eight weeks in the old Stillman Infirmary. My mother arrived from England while I was still in the hospital. I have never forgotten the kindness of the people around me: a fellow student met her boat; Professor Frank Taussig, then in his last year at Harvard, found lodgings for her and paid the bills out of his own pocket, all this for an unknown graduate student. When I recovered we went back to Chicago and spent six more months there. I wrote several articles, especially on capital theory, which were published, and developed a strong affection for America. Nevertheless, under the terms of the fellowship I had to go back to Britain.

In the summer of 1934 my mother and I returned to Liverpool, staying with relations, as we had no home to go back to. I think there were about two job openings in economics that summer, and I got one of them, at Edinburgh. We moved to Scotland. We managed to buy a little apartment in a duplex (with one apartment upstairs and one downstairs), overlooking the Firth of Forth. After Chicago, the University of Edinburgh seemed very dead. The people were friendly, but I made myself rather unpopular by giving a speech at a student conference, which came out in *The Scotsman* with the headline, "Scottish University Sitting on Haunches for the Last Fifty Years"! I think the most important thing that happened to me intellectually at Edinburgh was that my good friend there, William Baxter, who was teaching accounting, introduced me to Paton's accounting theory. For the first time in my life I discovered what a balance sheet was, which nobody had ever taught me at Oxford. This I think really revised my whole view of the theories of the firm and of capital. I saw the firm as governed by a principle that might be called the homeostasis of the constantly changing balance sheet. In the short run, the firm simply responded to changes in the balance sheet resulting from purchases. When customers purchased finished

goods, inventory went down, cash went up, and the cash would be spent on labor and materials to make more finished goods. This equilibrium balance sheet, however, would be constantly changing as new technologies, new goods, and new enterprises came into play.

While I was at Edinburgh my old professor Frank Knight published an article entitled "The Theory of Investment Once More: Mr. Boulding and the Austrians", commenting on some of my earlier articles.⁴ I have often said that this put me in such good company that I never had to take a Ph.D. Out of those years also I think came two very fundamental ideas: One I think might be called the demographic theory of capital, which consists of populations of valuable objects which are added to by birth (production) and subtracted from by death (consumption) and, perhaps arising out of this, a concern that economics has suffered from the confusion between stocks and flows, capital being a stock and income being additions and subtractions from capital, that is, a flow.

In August 1937 I went to a world conference of Quakers in Philadelphia. While I was there an old Chicago friend got hold of me by telephone and said there was a job going at a little college in upstate New York, Colgate University. After the conference I went up there, looked them over, they looked me over, I accepted the job and never went back to Britain. It is amazing how one's whole life can sometimes hang on a telephone call. After a year my mother joined me in the idyllic little village of Hamilton. The teaching load was very heavy, but my summers were free. In two summers I wrote my first book, *Economic Analysis*, an intermediate textbook. I sent the manuscript almost at random to Harper & Brothers and they published it almost immediately.⁵ It went through four editions, the last in 1966. The first edition fundamentally followed Irving Fisher and Keynes' *Treatise on Money*. Even though I had read Keynes' *General Theory* by that time, I think I had not really understood it. I am not quite sure that I do now. The second edition, however, in 1948, was a thoroughly Keynesian general theory. My life was very much overshadowed by the rise of Hitler in the Second World War. The contrast between the beautiful and peaceful environment in which I was living and what I knew was happening in Europe

⁴ F.H. KNIGHT, "The Theory of Investment Once More: Mr. Boulding and the Austrians", *Quarterly Journal of Economics*, vol. L (1936): 36-67.

⁵ K.E. BOULDING, *Economic Analysis* (New York: Harper & Brothers, 1941).

produced a great internal tension which released itself in the writing of poetry. This was not an easy time to be a Quaker and a pacifist, but a deep religious experience convinced me that I had to stay with this commitment.

In May 1941 I met Elise Bjorn-Hansen at a Quaker meeting in Syracuse. We were engaged in eighteen days and married in three months. We moved to Princeton, where I was working for the old League of Nations Economic and Financial Section on the recovery of European agriculture after the First World War. Looking back on it, I can see now that I spent most of the Second World War period preparing for the peace that was to come. At the League of Nations I participated in an important study, out of which came the United Nations Relief and Reconstruction Administration. A lot of mistakes which were made after the First World War were somehow avoided after the Second. The year in Princeton introduced me to what might be called the geography of statistics. We broke down Europe into small divisions to see what had happened to agriculture between 1913 and 1928. This revealed that national frontiers were not very significant. European agriculture had a sort of productive triangle, stretching roughly from Rome to Belfast to Stockholm. Outside this triangle in all directions the yield of crops fell very sharply, both in 1913 and in 1938.

In June 1942 I was fired by the League of Nations for an indecently Quakerly statement which my wife and I had circulated. We then went down to Fisk University, a black college in Nashville, Tennessee. While there I wrote *The Economics of Peace*,⁶ and, again, I think made a small contribution to the success of the peace in the post-war years. A year later we went to Iowa State College (as it was then called) in Ames, at the invitation of Professor Theodore Schultz, who wanted somebody to teach labor economics and had the idea of hiring somebody who knew nothing about it, who was a general economist, and getting him to convert to the field. This I could not resist, although we were happy at Fisk. I spent the year 1942-43 becoming a labor economist. I visited about 85 head offices of unions around the country and visited nearly every local union in Iowa, doing what today would be called casual empiricism, but I found it a most valuable learning experience. As a result of this, I

⁶ K.E. BOULDING, *The Economics of Peace* (New York: Prentice-Hall, 1945; reprinted: Freeport, New York: Books for Libraries Press, 1972).

decided that if I was going to study something like labor movements, I had to do sociology, political science, and anthropology, as well as economics. This got me interested in the unification of the social sciences. I saw the social sciences as all essentially studying the same thing, which was the social system, but from somewhat different mountain tops. Although I taught labor economics for a few years, I did not really become a labor economist. In those years at Ames I wrote *A Reconstruction of Economics*,⁷ the reconstruction being around two major theses. One was that capital was more important than income, especially from the point of view of the household, where the use of household capital is much more important than consumption.

The second reconstruction was a macroeconomic theory of profits, originating from Keynes' *Treatise on Money* and his concept of the "widow's cruse". This is what in later years I came to call the "K Theory", as it originates in the work of Keynes, Kalecki, Kaldor, and Kenneth (myself).⁸ Economics, however, obstinately refused to be reconstructed. As far as I can judge, my work in this field has had very little impact.

While at Ames I also worked for the Committee for Economic Development on the whole problem of the economic transition from war to peace. One of the remarkable successes of the American economy was the great disarmament that followed the Second World War without ever producing a serious post-war depression. I like to think I made some small contribution to this success.

In 1949 we moved to Ann Arbor and the University of Michigan. We had liked Ames very much. It was a wonderful university, rooted in the soil as a college of agriculture and mechanic arts, where the sheer logic of education, well supported by the state legislature, largely populated by its alumni, pushed it into becoming a first-rate liberal arts institution. The University of Michigan, however, was much larger and a more prestigious institution. By this time I was in a good bargaining position. I was very much involved in the integration of the social sciences and said I would come if I could teach a seminar in this, to which they agreed. The chairman of the economics department, I. Leo Sharfman, was a remarkable man who over the years built up a very unusual department. I also had

⁷ K.E. BOULDING, *A Reconstruction of Economics* (New York: John Wiley & Sons, 1950).

⁸ K.E. BOULDING, "Puzzles Over Distribution", *Challenge*, 28, 5 (Nov./Dec. 1985): 4-10.

some excellent secretarial assistance and my rate of publication increased very substantially.

Ann Arbor was a happy environment. We went there with our two-year-old, first-born, and had four more children during our 18-year stay there, so these were the great years of raising a family. The Ann Arbor Friends Meeting was a very congenial community. We had what I have called a non-kin extended family, a number of families all with children about the same age. So there was a great circulation of children around the various homes.

The years in Ann Arbor were punctuated by three very important years away. The first (1954-55) was spent at the Center for Advanced Study in the Behavioral Sciences in Stanford, California. This was the first year of its operation, and there was a very interesting group of people there, including Clyde Kluckhohn, the anthropologist, and Ludwig von Bertalanffy, the founder of general systems. The yearly seminar I had been running in Ann Arbor on the integration of the social sciences had really turned into a seminar for anybody I could integrate (this had gotten me in touch with Bertalanffy and general systems). Each year I would select a topic and involve interested people from the relevant disciplines around the university. The term before I went to California we had just finished a seminar on the theory of growth, with the participants ranging from biologists (on the growth of cells and organisms), to architects (on the growth of buildings), to, of course, economists (on the growth of economies).

One day shortly after we arrived at the Center, four of us were sitting around the lunch table: Bertalanffy; Anatol Rapoport, a mathematician and game theorist; Ralph Gerard, a physiologist; and myself. It became clear that we were all pursuing general systems from very different angles: Bertalanffy from biology, Rapoport from game theory and neurology, Gerard from physiology, and myself from economics. Somebody said, "Why don't we start an association"? The American Association for the Advancement of Science was meeting that December in Berkeley, so we decided to call a meeting and see if anybody came. We drew up a little manifesto around the lunch table announcing a call to the meeting. About 70 people actually turned up at the meeting, there was a lot of interest, and the Society for General Systems Research got underway. The society still exists (recently renamed the International Society for the Systems Sciences).

Another new development that got underway at the Center was peace research. A group of us, including Anatol Rapoport, Herbert Kelman, Harold Lasswell, and Stephen Richardson (Lewis F. Richardson's son), met to discuss how it was that while war and peace were clearly the most important problem of the age, nobody was doing much research on it. So we decided to start a journal. I have sometimes said that we created a vacuum to see if anything would rush into it. Anatol Rapoport and I went back to Michigan after the year in California and with Robert Angell and one or two others, we started the *Journal of Conflict Resolution*, which in recent years has become the official journal of the Peace Science Society (International). This led to the formation of the Center for Research on Conflict Resolution.

At the end of that year at the Center, when nearly everybody else had gone home, I dictated *The Image*, which was really an attack on behaviorism, arguing that behavior came out of the image of the world that people have in their minds rather than out of a stimulus.⁹ It is perhaps ironic that the Center for Advanced Study in the Behavioral Sciences (the name, incidentally, was invented because it was feared that Congress would think that "social science" would look like socialism), should have had as one of its first products my attack on behaviorism. The book has had a curious impact. As far as I know, it has had very little impact on psychology, although cognitive psychology did develop shortly afterwards. Cognitive geography and cognitive anthropology owe something I think to *The Image*, and the fact that it is still in print after a third of a century suggests that it has had some effect.

In 1959-60, my family and I went down to what was then called the University College of the West Indies, in Jamaica, for a year, in the strange position of visiting head of the economics department. It was there that I wrote *Conflict and Defense*, in some ways a little bit of economics imperialism, as it was in part an attempt to apply the contribution of economics to the larger, just developing, field of peace and conflict studies.¹⁰ This field of study has since turned into a discipline of its own, with its own journals and professional societies, such as the International Peace Research Association,

⁹ K.E. BOULDING, *The Image: Knowledge in Life and Society* (Ann Arbor: University of Michigan Press, 1956).

¹⁰ K.E. BOULDING, *Conflict and Defense: A General Theory* (New York: Harper & Brothers, 1962; reprinted: Lanham, Maryland: University Press of America, 1988).

which my wife and I helped to found in 1962, and of which she is currently the Secretary-General. The year in Jamaica was particularly interesting because it was our first experience of the tropics, and of a colonial world just coming to an end. This was the year before Jamaica became independent, so it was a bit like living in the United States in 1775, except that this was a peaceful transition. It was there I think that I first became interested in the problems of different forms of power.

The year 1963-64 we spent abroad in Japan, where I was a visiting professor at International Christian University in Mitaka, on the western edge of Tokyo. This was a wonderful year for all of us. I realized what an ignorant Westerner I was and what a wonderful stream of human life and experience had come out of Asia. It was there also that I got really interested in the evolutionary theory of human history. Most of my students were Marxists. The teachers' union in Japan at that time was oriented that way. I kept suggesting to them that though there were dialectical elements in human history, there were also non-dialectical processes, which, of course, they had never thought of. At the end of my term there I gave some lectures on "Dialectical and Non-Dialectical Elements in the Interpretation of History", which eventually turned into a book, *A Primer on Social Dynamics*,¹¹ expanded later into *Ecodynamics*.¹² I have been back to Japan a number of times and I have a great affection for it.

I remember asking my Japanese students what European country they thought Japan was most like. Almost without exception they said, "Italy". During the sixties I had a number of delightful holidays in Italy with my English cousin, Edwin Wells, and fell in love with its rich cultural heritage and its lively, friendly people.

On the way back from Japan, the summer of 1964, my family and I stopped off and I taught summer school for a few weeks at the University of Colorado. I fell in love with the beauty of the place then. So it is perhaps not surprising that a couple of years later, when I was giving a lecture there, when a friend from the economics department who was driving me in from the airport said they were

¹¹ K.E. BOULDING, *A Primer on Social Dynamics: History as Dialectics and Development* (New York: Free Press, 1970).

¹² K.E. BOULDING, *Ecodynamics: A New Theory of Societal Evolution* (Beverly Hills, California: Sage Publications, 1981).

looking for a senior economist and asked whether I could recommend anybody, I said, "What about me"? My wife was then just finishing up her Ph.D. in sociology at the University of Michigan, so I said if they could find a job for her as well, we would be interested, which they did, and so we moved to Boulder in the fall of 1967 and have lived here, to our great delight, ever since.

One of the developments in the last years at Michigan was Grants Economics, that is, the study of one-way transfers of economic goods. This came out of my interest, first of all, with conflict, and then with power. I had been curious as to why some conflicts were creative and some were destructive. I decided that the major difference lay in the exercise of what might be called "integrative power", involving such things as legitimacy, respect, affection, love, and so on. This indeed I have argued is the dominant form of power. Without it, both threat power and economic power are very ineffective. But I was very much puzzled by it. Being an economist, I was looking around for a measure of it and hit upon the idea of the grant, especially the voluntary grant. Economic power rests to a very large extent on exchange — I give you something and you give me something in return. If I give you something and you don't give me anything, at least nothing that is recognized by accountants, this is a grant. I thought then if one could get something of a matrix of the grants economy, that is, who gave grants to whom, this should be indicative of the nature of integrative structures. So I got a grant from the Ford Foundation to study grants. I took on a young man by the name of Martin Pfaff to help me, then at Michigan State University at East Lansing (now of the University of Augsburg). He turned out to be not only a good friend, but a remarkable entrepreneur. We (with the assistance of Professor Janos Horvath of Butler University) organized the Association for the Study of the Grants Economy. It became very clear that there were two sources of grants: one, threat, and the other, integrative. So I called my first little book on the subject *The Economy of Love and Fear: A Preface to Grants Economics*.¹³

I have never been much interested in power, but rather in truth, and this perhaps comes out of my long association with the Quakers (there is a famous Quaker pamphlet with the title "Speak Truth To

¹³ K.E. BOULDING, *The Economy of Love and Fear: A Preface to Grants Economics* (Belmont, California: Wadsworth Publishing Company, 1973).

Power"). Nevertheless, I seem almost to have made it a hobby to be elected president of professional societies. In 1955 I was the first president of the Society for the Advancement of General Systems Theory (the name soon thereafter changed to the Society for General Systems Research and recently has become the International Society for the Systems Sciences). I was president of the American Economic Association and also the newly formed Association for the Study of the Grants Economy in 1968, president of the International Studies Association in 1974, and of the American Association for the Advancement of Science in 1979. In 1968, when I was president of the AEA, there was a tremendous uproar over the Democratic convention in Chicago and Mayor Daley's behavior. There was a strong movement among professional associations not to meet in Chicago by way of protest. The AEA was scheduled to meet in Chicago that December and my Executive Committee was split 6 to 6 on whether we should meet somewhere else, which put the responsibility for the decision wholly in my lap, without any alibis. I went to the AEA offices in Evanston, Illinois, and met with the people from the hotels that we had contracted to stay in, and then went and communed with my soul and decided that we should stay in Chicago. As a regular member of the Executive Committee, I would probably have voted the other way. To find that having power changes one's views and decisions was something of a shock to me!

The years in Boulder have been very happy and productive. I have had a remarkable secretary/administrative assistant, Mrs. Vivian Wilson, who has been with me now for 22 years. She transcribes my dictation, edits my writings, organizes my travel, keeps my office in order, and compiles my bibliography. Partly as a result of this happy arrangement, partly through good health and aging slowly, I have continued to be productive during my seventies, I have continued teaching (to which I am practically addicted), and I have had various visiting professorships around the world. I feel I have been remarkably fortunate and have had a wonderful life. My wife Elise is a distinguished scholar in her own right, a wonderful mother and partner. This year we celebrated our 48th wedding anniversary. Our five children have all turned out to be interesting individuals. We have 14 grandchildren and one soon to come, which gives me a slight sense of Malthusian guilt.

To try to put the six decades of my life's work in order, I have

constructed the table which shows the number of items in my bibliography (including articles, books, book reviews, monographs, and pamphlets) by subject matter, listed in order of the first publication in each area. It is interesting to note that while I have expanded my interests in each decade, except the most recent, there

KENNETH BOULDING BIBLIOGRAPHY (1932-1988)
(by number of publications* per category per decade)

	Year of first publication	1930s	1940s	1950s	1960s	1970s	1980s	Total
1 Price theory	1932	3	6	3	8	3	3	26
2 Marxism	1932	1	0	0	3	3	1	8
3 Population & capital	1934	4	0	4	4	5	3	20
4 Stocks & flows	1935	1	1	6	1	0	0	9
5 Quakerism	1938	6	16	2	10	5	8	47
6 Dynamics/development/ future	1939	1	2	10	34	74	26	147
7 Economics of peace & war	1941	0	5	2	6	6	4	23
8 Firms & organizations	1942	0	3	5	7	3	1	19
9 Peace & conflict	1942	0	1	9	62	45	35	152
10 Power & legitimacy	1944	0	1	0	7	5	2	15
11 Labor	1945	0	2	2	2	2	0	8
12 Agriculture	1947	0	1	3	1	1	2	8
13 Economics/scope & method	1948	0	3	9	9	18	11	50
14 Policy questions & political life	1948	0	2	6	22	18	6	54
15 Economics & religion	1950	0	0	12	6	1	0	19
16 Profit & interest	1950	0	0	7	0	1	1	9
17 General systems	1951	0	0	6	14	15	21	56
18 Economics as a social system	1952	0	0	7	14	5	5	31
19 Knowledge, information & education	1953	0	0	4	39	44	25	112
20 Ethics	1953	0	0	3	9	10	2	24
21 Economics/graphics	1954	0	0	1	1	3	0	5
22 Evolution, ecology & environment	1955	0	0	3	2	17	21	43
23 Images	1956	0	0	1	6	3	1	11
24 Water problems	1956	0	0	1	2	0	2	5
25 Urban studies	1958	0	0	1	3	6	2	12
26 Grants economics	1962	0	0	0	5	18	6	29
27 Human betterment	1963	0	0	0	3	25	16	44
28 Art & culture	1968	0	0	0	1	3	0	4
29 Family	1970	0	0	0	0	8	1	9
30 Energy	1973	0	0	0	0	9	6	15
31 Aging	1977	0	0	0	0	3	2	5
Totals		16	43	106	281	359	213	1019

* Publications include articles, books, book reviews, monographs, and pamphlets.

are only a few cases in which my interests have not continued almost to the present. These categories should not be taken too exactly. It is often rather difficult to say in which category a publication should lie, but the table at least gives a rough view of my interests, spread out over a lifetime.

The table suggests I think that although I have certainly gone "beyond economics" (the title of a book of collected papers published in 1968),¹⁴ into areas such as ethics, peace and conflict studies, general systems, and religion, my interest in economics has continued throughout my whole life. "Price theory" (Category No. 1), for instance, has been a long continuing interest. I do see the relative price structure as a very important, though constantly changing, condition in social systems. The concept of a moving equilibrium is useful, going back very much to Alfred Marshall and Adam Smith. The equilibrium concept implies that some prices may be "too high" and others may be "too low". If they are too high, they will tend to fall; and if they are too low, they will tend to rise. I still hold very strongly the liquidity preference theory of relative prices in the market which I developed in the early forties and published in 1944, though it never received very much attention. The fundamental idea here is that exchange consists of a redistribution of assets among the exchangers. That hence the relative price structure depends both on the total stocks of these many different kinds of assets, which have to be held by somebody, and on the aggregate preference for holding them. These preferences depend in large measure on beliefs about the future of the relative price structure. If there is a strong belief, for instance, that the price of wheat is going to rise, the preference for holding wheat will increase and the price of wheat will rise. The same holds true of money. If people think that the relative price of money is going to rise, they will want to hold more of it, prices of commodities and securities will fall and the price of money will rise.

I have sometimes told my students exactly how to get rich: Always hold your assets in the form which is rising most rapidly in relative value. I also tell them that I give them this advice for free, which is what it is worth, as nobody ever really knows precisely which prices are going to rise, even when they have some sort of

¹⁴ K.E. BOULDING, *Beyond Economics: Essays on Society, Religion, and Ethics* (Ann Arbor: University of Michigan Press, 1968).

inside information. If the total real value of all assets in markets is constant, then the markets become a genuine casino, as Keynes called them, in which those who happen to be holding assets whose relative prices rise gain at the expense of those who happen to be holding assets whose relative prices fall.

The distinction, going back to Adam Smith, and developed in Marshall, between market price and what Adam Smith called "natural" and Marshall, "normal" price, is also very important. Normal price equilibrium rests on the proposition that the set of relative market prices determines a set of relative advantages in the production of the commodities concerned. If the "market price" of wheat is "too high", in this sense wheat production will become relatively profitable, people and other resources will go into it, the output of wheat will rise, the stocks of wheat will rise, and the price of wheat will fall. A still unsolved problem is the relation between the relative price structure and technical changes, which changes the underlying equilibrium structure. This goes back to Adam Smith's famous deer and beaver example, in which the market price depends on how many deer are in the market looking for how many beaver, and in which the natural price is how many deer can be obtained in the woods by giving up the resources producing one beaver. Here again, if beaver in the market are "too dear" relative to the alternative cost in the woods, beaver production will be more advantageous and there will be a shift of resources out of deer into beaver which will bring down the price of beaver in the market. The question which very few economists have raised is whether other reactions to this disadvantage or unprofitability of producing something in the market may not be possible. For instance, technical change which will change the alternative costs. This problem has been surprisingly little studied by economists. I must confess I don't know the answer myself.

My interest in "Marxism" (No. 2) goes back to my undergraduate days at Oxford, when I had to read the three volumes of *Das Kapital*, which really turned me much the other way, although I felt that some of Marx's criticisms of early capitalism were valid. Though I have always believed that the free market can have very serious pathologies, the Marxist solution seemed to me to have far too high a cost in terms of the loss of human freedom and the worship of violence to be acceptable.

"Population and capital" (No. 3) reflects a conclusion that I

came to fairly early, that the universe consisted of populations of all kinds of species — chemical, biological, and social —, that all these follow a very fundamental demographic principle, that the increase in anything is equal to the additions minus the subtractions, birth and death in the case of biological species, production and consumption in the case of social species like commodities. This led eventually to my conviction that economists had gotten the factors of production wrong, that land, labor, and capital were hopelessly heterogeneous aggregates from the point of view of production functions, with all the validity of earth, air, fire, and water, and that production, whether biological or social, had to be thought of in terms of a genetic factor, which I have called "know-how", something which the genes have in biological production and which exists in human minds, documents, and so on in the case of the production of commodities. This genetic factor, if it is to realize its potential, has to be able to capture energy in particular forms and places in order to select, transport, and transform materials into the product, whether this is a baby or an automobile. This is not to say that wages, profit, interest, and rent are not significant economic concepts, but that land, labor, and capital are factors of distribution, not of production.

My interest in "stocks and flows" (No. 4) goes along with my interest in population and capital. Economics always seemed to me to make a great mistake in confusing these, going right back to Adam Smith. As a result of this, I have conducted a long, and quite unsuccessful, campaign against the idea that the object of economic activity is consumption. The difference between the rich and the poor consists mainly in the capital stock of useful objects with which they are surrounded and to which they have access and use. I get no satisfaction out of the fact that my clothes, my house, or my car are wearing out, which is consumption. I get satisfaction out of wearing them, living in them, and driving them, which is use. Consumption is an element in the overall welfare or riches function. I take some satisfaction in eating as well as being well fed, and there are good evolutionary reasons for this. Fundamentally, production is necessary because using goods involves their depreciation and consumption. It is only as production exceeds consumption that the stock of useful things increases, along with the use that we get out of them.

My writings on "Quakerism" (No. 5) I will pass over, although as my biographer pointed out, there has always been a "creative tension" between my identity as an economist and my identity as a

Quaker, which I am quite sure has affected my interests and my work.¹⁵

"Dynamics and development" (No. 6) turns out to be the second largest category of my publications, one which has risen fairly steadily during my lifetime. I have always been unhappy with what might be called "Samuelsonian dynamics", which is fundamentally based on celestial mechanics, with its emphasis on models with constant parameters over time. This I argue is very inappropriate for social systems, where parameters change all the time, where we have the phenomenon of "regions of time",¹⁶ at the boundaries of which the parameters of the system change. The macroeconomic theories of economic development, popular from the forties on, beginning with Harrod¹⁷ and Domar,¹⁸ seem to me to have been very misplaced, mostly because of a fallacious theory of production which assumed what I have called the "cookbook theory" — where you mix land, labor, and capital, and out come potatoes. If we see production as always originating in a genetic structure, which fundamentally is part of knowledge and know-how, the accumulation of capital in the form of tools, machines, buildings, and so on, is part of this process, where these are fundamentally limiting factors, not genetic factors. When we have limiting factors, it is the *most* limiting one that is most significant, and this can change all the time. Fundamentally I look upon economic development as an evolutionary process and as a learning process. Learning, the change in the genetic factor of know-how, may be partly accidental, as in the mutation of biological genes. But when it comes to creatures in which the phenotype has learning abilities, we get what I have called "noogenetic" evolution, in which learned structures are actually communicated from one generation to the next. Social evolution and economic development are almost entirely of this nature, although there are examples of accidental learning, new ideas which appear spontaneously, and so on, which are more reminiscent of biological mutation.

¹⁵ C. KERMAN, *Creative Tension: The Life and Thought of Kenneth Boulding* (Ann Arbor: University of Michigan Press, 1974).

¹⁶ K.E. BOULDING, "Regions of Time", *Papers of the Regional Science Association*, 57 (1985): 19-32.

¹⁷ R.F. HARROD, *Towards a Dynamic Economics* (New York: Macmillan, 1948).

¹⁸ E. DOMAR, *Essays in the Theory of Economic Growth* (New York: Oxford University Press, 1957).

An essential element of a theory of development and evolution is some kind of theory about the growth and development of individual members of the populations which comprise the total system. It is not surprising, therefore, that I have been interested in the theory of the firm, which is one contribution of economics to this problem, and also in the general theory of organizations of all kinds (No. 8). This involves both the theory of homeostasis, of what behavior preserves the existing character of organizations, as well as some kind of theory of the growth and decline and eventual death of organizations. It also involves a genetic theory of the production and the maintenance of such structures, involving some kind of know-how, the capacity to replace depreciation and consumption. Not surprisingly, this has been a pretty constant interest of mine.

Another aspect of my interest in evolutionary dynamics is my work on "knowledge and information" (No. 19), actually the third largest area of my publications. One of the most important intellectual developments of my lifetime has been the development of information theory, which suggests that in the evolutionary process matter and energy are significant mainly as carriers of information, in terms of improbabilities of structure. But we have to go beyond what I have called the "Bell Telephone" information concept of Shannon and Weaver.¹⁹ The casual conversation of two teenagers over the telephone may have exactly the same amount of information as communication between Mr. Bush and Mr. Gorbachev over the "hot line", but the knowledge and power significance may be very different. Knowledge is essentially a stock; information is a flow, which results in additions to, and occasionally subtractions from, the stock.

Knowledge has a number of aspects or phases. There is first know-how, which is what the fertilized egg has. My own fertilized egg knew how to make a *Homo sapiens* male with pale skin, blue eyes, and originally black hair. It did not know how to make a dark-skinned, brown-eyed female, much less did it know how to make a hippopotamus. Beyond know-how there is "know-what", that is, conscious knowledge, images in our mind of a world beyond it, which may have various degrees of truth or realism. Beyond know-what there is "know-whether", which involves a valuation

¹⁹ C.E. SHANNON and W. WEAVER, *The Mathematical Theory of Communication* (Urbana: University of Illinois Press, 1949).

system that selects out of the broad variety of potentials for action those which are considered best. We might even distinguish a "know-whom", which is important in the power structure, which is one reason why people go to Oxford or Harvard. The whole question of the distribution of the knowledge structure among members of society is very important. The distribution of wealth and income is closely related to the dynamics of the distribution of knowledge and know-how. While there are important random elements in the process of getting richer, knowing how to do it is certainly a great help. The knowledge structure is also very important in the problems of "power and legitimacy" (No. 10), also related to the problem of "images" (No. 23). These topics are the subject of my latest book, *Three Faces of Power*.²⁰

The most general expression of my interest in knowledge is in "general systems" (No. 17). The general systems movement was basically an attempt to introduce economics into the knowledge acquisition process by identifying those theoretical structures which are common to two or more of the conventional disciplines. The general systems movement, which is still very much alive, can perhaps be divided into what I have sometimes called "special" general systems, concerned especially with mathematical modeling, and "general" general systems, which is the more philosophical approach, to which on the whole I myself prescribe. Perhaps this is just another example of a general system of specialization.

A special case of my interest in the general problem of knowledge is my concern with the methodology of knowledge acquisition, particularly in economics. I have argued for what I have called "appropriate methodology" in various disciplines, arguing that how we find out things depends on what there is to find out. The methodology which is appropriate, for instance, for celestial mechanics, which is a system with highly stable parameters and easily quantifiable observations, is not appropriate in systems which have information as an essential component. Information introduces irreducible uncertainty into the system, with a non-existence theorem about exact prediction. Information, according to information theory, has to be surprising or it is not information. We cannot predict what we are going to know ten years from now or we would

²⁰ K.E. BOULDING, *Three Faces of Power* (Newbury Park, California: Sage Publications, 1989).

know it now. Even the biological sciences have to conform to this principle. We have to recognize that even biological evolution is a process with profound uncertainty and is dominated by the exact time at which highly improbable events happen. The very success of celestial mechanics, therefore, has had a somewhat catastrophic effect on other fields of knowledge.

Statistics also has suffered from the failure to recognize the implications of uncertainty being very distinct from probability or risk, in the language of my old professor Frank Knight. Information systems also can only make a limited use of quantification. Counting things which are not identical loses information about them, though it also may help to create knowledge, which I have sometimes argued is obtained in part by the orderly loss of information. Reality, especially social reality, is orders of magnitude more complex than a single number. This is why we are forced into taxonomies for breaking down aggregates into categories. Yet taxonomy, by its very nature, is inaccurate. It can never do justice to the complexity of the world. It is strange that in the philosophy of science there seems to be very little discussion of the evaluation of taxonomy. But we are constantly putting things which are alike into different taxonomic boxes and putting things which are different into a single taxonomic box.

Another of my categories, which might be considered basic, the study of "economics as a social system" (No. 18), goes back to my early interest in the integration of the social sciences and my strong conviction that all the social sciences are studying the same thing, which is the social system, from somewhat different angles and perspectives. Another category which might be considered basic is the category of "ethics" (No. 20). This is essentially part of the expanded knowledge structure, particularly involving "know-whether". It involves such things as the magnitude of the area or the field over which we are making judgments, such aspects of human behavior as benevolence and malevolence, and indeed is quite critical to the understanding of human behavior in general. Another category which also involves the learning and knowledge process is that of "graphics" (No. 21), in which I have had a long-time interest, growing out of my conviction that the real world consists primarily of topology, that is, shapes, sizes, structures, patterns, fittings and so on, and that numbers are primarily significant as a guide to the topological structure of the world. My favorite example of this is the

computer which has in its memory the latitude, longitude, and altitude of a very large number of places on the earth's surface. These are stored as numbers, though arbitrary numbers, as all measurement is. If the computer printed out the numbers, it would tell us nothing. Based on these numbers, however, a clever computer can print out a map that tells us a great deal. Computers currently are less skilled in producing maps of the space-time structure. This is something that still needs further work. I am hoping to bring out a book on this subject in the next year or two.

Another category that can be regarded as basic is "profit and interest" (No. 16), which might perhaps better be called "macro distribution". This is the problem of what determines the distribution of income as between profit, interest, rent, and wages, to which we should perhaps add a fifth category of grants (No. 26). This body of theory is an attempt to throw some light on the question as to why profits, for instance, in the United States and over a central part of the world economy, became negative in 1932 and 1933; why interest in the American economy has increased, for instance, from about one percent of the national income in 1950 to 9 or 10 percent today; and why the proportion of national income going to wages fell sharply in the United States between 1932 and 1942, the period of the New Deal, when there was a great rise in labor union membership and a great increase in collective bargaining. My first major attempt to try to answer these questions is in my book *A Reconstruction of Economics*. I found the key to the question as to what determined the proportion of national income going to profit in the suggestions of Keynes in the *Treatise*, which he called the "widow's cruse", further developed by Kalecki, and to some extent by Kaldor. There are some errors in *A Reconstruction of Economics* which I corrected in "Economic Theory: The Reconstruction Reconstructed".²¹ The basic theory is that, looked at from the point of view of the collective balance sheets of the economy, profits represent a gross increase in total net worth. This can only come from perhaps two or three major sources. One is net investment, which adds directly to net worth. Another is expenditure of households out of income received from profits. This is what enables firms to charge more for their product than the average cost. This item reflects the famous statement by

²¹ K.E. BOULDING, "Economic Theory: The Reconstruction Reconstructed". In: *Segments of the Economy - 1956: A Symposium* (Cleveland: Howard Allen, 1957), pp. 8-55.

Kalecki, which seems to be part of the Cambridge oral tradition, that "capitalists get what they spend and workers spend what they get". A third factor, significant at times, though probably small, is the increase in the money stock of businesses, resulting partly from the shift of money out of households, partly from the creation of new money. It seems to be almost impossible to find any data on this. I have called this "K Theory",²² but it has made very little impact on mainline economics, which still clings to the aggregate marginal productivity theory, in spite of its utter failure to explain the redistributions which took place during the Great Depression.

The other element in macro distribution, grants economics, developed as a separate enterprise. It has both a micro and a macro aspect, the first to explain why grants are given, the second to discover their total impact on society. This macro aspect is still to be developed, as up to now the work on the grants economy has been mainly at the micro level.

My interest in the problems of "evolution, ecology, and the environment" (No. 22), which really straddle the basic and the applied, is a rather late development, traced back perhaps to a 1955 conference on "Man's Role in Changing the Face of the Earth", held at Princeton, New Jersey. However, my interest in these problems increased substantially in later years.

"Human betterment" (No. 27) likewise straddles the basic and the applied and originates perhaps in an early interest in welfare economics,²³ which I eventually found rather sterile. It was too much confined to price theory, neglecting the larger aspects of human behavior and the grants economy. This larger concept of human betterment — that is, how we evaluate things as going from bad to better rather than from bad to worse — culminated in my 1985 book on this subject.²⁴

My interest in the applied fields has certainly grown over the course of my life, including such things as "labor economics" (No. 11), "agriculture" (No. 12), "water problems" (No. 24) (which goes back to my serving on a California state commission in 1958 on The Social and Economic Consequences of the California Water Plan),

²² K.E. BOULDING, "Puzzles Over Distribution", *cit.*

²³ K.E. BOULDING, "Welfare Economics", in *A Survey of Contemporary Economics*, Vol. II, B. Haley, ed. (Homewood, Illinois: Richard D. Irwin, for the American Economic Association, 1952), pp. 1-34.

²⁴ K.E. BOULDING, *Human Betterment* (Beverly Hills, California: Sage Publications, 1985).

"urban studies" (No. 25), the role of "art and culture" (No. 28) in society, especially in economic life, the place of the "family" (No. 29) in society, the problems of "energy" (No. 30) (my interest in which perhaps goes back to the energy crisis of the seventies, when I was a member of the National Academy of Sciences Committee on Nuclear and Alternative Energy Systems), and the problems of "aging" (No. 31) (an interest which is probably related to the fact that I have been getting older). The "economics of religion" (No. 15) has been a long-standing interest, perhaps as a result of the tension between my life as an economist and as a Quaker. The general area of what might be called "policy questions and political life" (No. 14) has also been a long-standing interest.

The largest category in the whole list, however, is "peace and conflict" (No. 9), which, again, certainly has something to do with my identity as a Quaker. I was active in the very early days of the peace research movement in the founding of the *Journal of Conflict Resolution* in 1956. I have often said that my interest in peace research arose out of the feeling that the peace movement provided a demand for peace, but not very much supply. As an economist, of course, I believed in both. My two major contributions here I think are my book *Conflict and Defense* (1960) and *Stable Peace* (1978),²⁵ which came out of a year I spent as the Tom Slick Visiting Professor of World Peace in the L.B.J. School at the University of Texas, in Austin. I have argued that a new discipline has developed in the last 40 years, which the French called "polémologie". In English it is usually referred to as peace and conflict studies. This goes back to the work of Lewis Richardson²⁶ and Quincy Wright²⁷ in the early part of the century. It is now embodied in the International Peace Research Association. This is comprised of about 100 institutes around the world and 800 individual members. I think I might like to claim that peace and conflict is the most important part of my work. The development of aerial warfare and the nuclear weapon has created an enormous crisis for the human race, the closest parallel to which would seem to be the development of gunpowder and the effective cannon in the 15th and 16th centuries, which destroyed the

²⁵ K.E. BOULDING, *Stable Peace* (Austin: University of Texas Press, 1978).

²⁶ L.F. RICHARDSON, *Statistics of Deadly Quarrels* (Chicago: Quadrangle Books, 1960).

²⁷ Q. WRIGHT, *A Study of War*, 2nd ed. (Chicago: University of Chicago Press, 1965; originally published 1942).

feudal system and the baron and in effect led to the creation of the national state. Today I would argue that unilateral national defense in the national state is as obsolete as the feudal castle, and that we have to move towards new concepts of the use of threat systems and the development of universal security.

I have to confess that my life has been great fun, almost indecently so in a time of such world tragedy. And in spite of a bout with prostate cancer last spring, now I am glad to say in complete remission, I look forward to some more years of creative thought and writing.