

Chapter Five

Graduate Studies

I began my graduate studies in February 1946 and finished my course work in August 1947, after I had passed my oral exam for the doctorate. In short, I took courses in five semesters, including two summer terms. In summer term courses, professors as well as students tend to take it rather easy because of the heat. I enrolled in the Slavic Department, taking third-year Russian, Russian history, and a seminar on problems in contemporary world politics. Russian history was taught very well, and the seminar on world politics allowed me to pursue a stimulating topic largely on my own. The instructor in the Russian language course was an interesting and eccentric man, but a poor teacher. The students in the seminar were a lively group with a wide range of political opinions, which generated much good discussion. I also took an independent reading course, but really had little idea what to read and where to start. The professor left me to my own devices, trusting that I would make good use of my time, but I did not. At the end of the first semester I realized that I was not interested in Slavic languages, literature or linguistics; I was interested in history and politics and without any difficulty transferred to a doctoral program in the Government Department. There, I took courses for another twelve months.

It seems to me in retrospect that in these courses I learned little. What I did learn came not primarily from courses and seminars but from discussions with fellow students outside of class. They included some very bright people from many different countries and backgrounds; some of them made illustrious careers in public life. The faculty advisor to whom I was assigned was very kind and hospitable. The parties he gave for his graduate students in his spacious Belmont home were delightful. William Yandell Elliott—that was his name—was a Southern Democrat of a type that predominated in American politics at the time. Born and raised in Eastern Tennessee, he had attended Vanderbilt University and then gone to Oxford as a Rhodes

Scholar. An early supporter of the New Deal, he had broken with Franklin Roosevelt over his attempt to pack the Supreme Court, but still had lots of good friends in Washington. I remember a heated discussion he had with a graduate student during a seminar session; the student had suggested that the race problem in the United States would be solved through intermarriage, and Elliott passionately declared 'miscegenation' to be abominable. All his life he had had some cause that he pursued with vigor and tenacity. In the 1920's, it had been his realization that fascism presented a clear and present danger. In the 1930's, it had been his conviction that there were flaws in the U.S. constitution, which might be remedied by our adopting some elements of British government. Now, in the 1940's, his preoccupation was with the perils posed by Marxism and communism, and this fear caused him to drift further to the Right politically.

Professor Elliott welcomed me as his student and encouraged me to write a doctoral dissertation about the founding father of communism, V. I. Lenin. Unfortunately, my thesis did not turn into the anti-Communist indictment he obviously had expected, and he therefore did not promote my career any further. I found that academia in the U. S. functioned very much like an old boys' network in which senior people make the crucial decisions affecting their disciple's careers. In the end, Bill Elliott found a student who was much more in tune with him politically, and whom he had spotted when the student was still an undergraduate. I once met this young man when Elliott called me into his office, saying, "Al, I want you to meet Henry. He is very bright and will be going places." Henry Kissinger indeed owed the rocket-like start of his career to this professor.

Professor Elliott was kind and supportive, however, and I am grateful to him. One thing he did not provide, however, was academic guidance. Whether I should have asked for it more, I do not know, but I would guess that at Harvard the faculty thought that graduate students knew, or should know, what they were doing. I did not know what I was doing, did not have enough sense of direction, and certainly did not know what amount and variety of knowledge I was expected to have before getting the doctoral degree. I shopped around for courses, occasionally

audited some that seemed to cover interesting topics. I certainly was not lazy, but my study program did not have enough depth. When at the end of three terms in the Government Department, I declared myself ready for the dreaded oral exam, my adviser should have told me that I was out of my mind. This was especially true in view of the fact that I offered myself for examination in a field in which I had done no coursework whatever. Instead, he scheduled the exam, and I passed by the skin of my teeth. If I were to sum up my experience as a graduate student, I would be tempted to say that I started to learn something mostly after obtaining my Ph.D., and what forced me to learn was the need to prepare courses. There is a Latin saying that I have adopted as my own motto: *Docendo discimus*--we learn by teaching.

My introduction to teaching came soon. When I transferred to the Government Department, I was offered a Teaching Fellowship. Recipients of such fellowships either assisted various professors in courses with large enrolment, or they served as tutors—academic advisors to undergraduate students in the Honors program. Advising them meant seeing each ‘tutee’ fairly regularly, discussing their course work with them, trying to stimulate their interest in related courses or subjects, and supervising their work on a Senior Honors thesis. Even before the new academic year had started, one Senior Honors student to whom I had been assigned as a tutor came to me for advice. He was a brilliant student, and socially successful as well. He had managed to enter a prestigious club—not one of the most exclusive ones, but a distinguished one, nonetheless. His dormitory was Eliot House, if memory serves me right, which at that time was the dorm preferred by the social elite. He was also the commanding officer of the Naval ROTC unit at Harvard. In the summer prior to his last year in college, he had participated in a summer cruise on a U. S. Navy vessel. While on that cruise, he had been invited by one of the sailors to a homosexual encounter. The invitation had been a trap. When he entered the designated room, he was greeted by a group of jeering sailors. The matter became a scandal, which, of course, was reported to the Dean at Harvard. Now, Harvard was going to throw him out unceremoniously.

I considered it my duty as his tutor to come to his defense, and I made an appointment with the Dean of the College of Arts and Sciences. He received me politely, heard my plea on behalf of the student, and then remarked that he would not even consider retaining him. Harvard, he said in so many words, already had a reputation as a place for wimps. Given such a reputation, it could not afford to keep a student who had even the slightest brush with homosexuality. I often wondered what became of this young man. This incident reminded me of another young man I once met. In early 1942, I was doing guard duty at the Presidio of Monterey—guard duty of an unusual kind. A soldier had been tried for homosexual activity; he was convicted and sentenced to life imprisonment. He was now in the post guardhouse awaiting shipment to the military prison in Fort Leavenworth, and, because he was believed to be suicidal, had to be guarded around the clock. He was a gaunt young man with an intelligent face who talked pleasantly with those who guarded him. During the night, while he slept peacefully, I found it difficult to stay awake and alert, sitting by his bunk with my rifle at the ready.

When I entered graduate school, Russian studies hardly existed in the United States. At Berkeley, Columbia, Harvard, and the University of Chicago, perhaps in other places as well, there were a few individual professors who taught courses on Russian history, literature, or geography. The Slavic Department at Harvard had only one tenured member, however. Once one moved into the field of Soviet studies—the post-revolutionary era—the pickings got even slimmer. Columbia and Harvard had professors who had studied law or administration in the USSR, but at Harvard, the professor teaching Russian history stopped when he came to the revolution of 1917; at that point, he said, Russian history had come to an end, and politics had begun.

For anyone interested in theory and ideology, the situation was even bleaker. I doubt whether a student could have taken a course in Marxism at any American university. Philosophers claimed that Marx was not a philosopher; most economists argued that he had not been an economist—or at least not an economist whom one needed to take seriously; specialists

in political or sociological thought said similar things. To the best of my knowledge (and I may not be well informed on this), the course on Marxism I taught at Harvard in 1951 or 1952 was the first such course in an American institution of higher learning.

I couldn't learn the subject from anyone. My interest in the history of social thought had been awakened, however; my knowledge of Russian naturally directed that interest toward a specialization in Soviet ideology, and my advisor encouraged me to go in this direction. Since Soviet ideology supposedly was based on the writings of Engels and Marx, it seemed appropriate that my study of it should also begin with a study of Marxism. Since no one at Harvard seemed to have sufficient knowledge of it to serve as my tutor, I had to study it on my own through independent largely unsupervised reading. A book I published in 1956 was based on this attempt to come to grips with the basic propositions of Marxism. While it contained little that I had later cause to repudiate, in retrospect I felt that it showed how inadequately I had surveyed the relevant literature. Of course, had I really taken the time to read everything I should have, I would never have managed finish writing the book.

My academic career was largely shaped by the Cold War. Many different views could be advanced regarding when the Cold War began. For instance, one might argue that it began around 1943, when George Kennan, then an official at the U. S. Embassy in Moscow, began sending message after message to President Roosevelt warning him that, once Germany had been defeated, our next enemy would be the Soviet Union. At the very same time, the Kremlin alerted Soviet leaders to the likelihood that, once Germany was defeated, the Soviet Union would face a hostile Western alliance. One could go farther back, to 1919, when anti-Communist sentiments were high in the United States, the Attorney General staged raids on left-wing organizations, and veterans of World War I founded the American Legion to combat all forms of left-wing ideology. One might even go back to November 1917, the time of the October revolution. Czarist Russia, in the eyes of the American public, had been the evil empire, because its government was as far removed from the ideas of American constitutional democracy as any European government

could be. When czarism fell and was replaced by the Provisional Government, a wave of popular good will hailed the dawning of democracy in that country. This good will turned into deep hostility, however, when the Provisional Government was overthrown, signaling the beginning of the Soviet regime.

Instead of moving back from 1943, one could also move forward in time and suggest that the cold war began in May 1945, when Germany surrendered, or one could point to August 1945, when President Truman twice displayed the awesome might of our atomic weaponry. One could also cite the summer of 1946, when Secretary of State Byrnes in his speech in Stuttgart hinted that U.S. policy ought to aim at the reconstruction of West Germany as a bulwark against the Communist menace.

From the point of view of American academia, the Cold War began around 1946 or 1947. This beginning was signified by the attention which, for the first time, American universities were paying to things Russian and Soviet. At Columbia University, a Russian Institute was founded, and a year later Harvard followed suit with the creation of its Russian Research Center. The Rockefeller Foundation sponsored research fellowship in Russian studies at the Hoover Institution on the campus of Stanford University. I received one of these fellowships and, in early September of 1947, Eva and I were off to California, a trip we made by car.

We had thought of buying a used hearse to get us there. That would have been quite reasonable. Hearses usually are sturdy cars, such as Cadillacs or Chryslers. They rarely if ever are driven at top speed, which may mean that their motors have not been abused. They also are roomy; and on a long trip coast to coast, two young vagabonds could easily stow all their belongings, including two sleeping bags and a camping stove, in them to save on motel and restaurant bills. We actually looked at one or two such vehicles, but my mother-in-law was horrified by the idea of her daughter traveling in a hearse, and we then bought another used car—a real lemon, as it turned out. It got us there, however. For the first few days or weeks, we lived

in a rented room, but then found a charming little house a few miles away from the campus, where we spent the remainder of the academic year.

Stanford University is located on a peninsula that stretches from San Jose in the South to San Francisco and the Golden Gate in the North. Today that peninsula is densely settled almost up to the crest of the picturesque mountain chain that runs along its middle. In 1947, however, it was still primarily orchard country, its apricot, peach, and almond groves a miracle of fragrant pink and white blossoms in the spring. Along the crest of the mountain ridge, one could drive through stands of redwood trees, and on the Western shore, where artichokes were the principal cash crop, there were beaches with wild surf and very few visitors. The Stanford campus was pretty with eucalyptus and palm groves and its own little lake. Right next to the Hoover Institution there was an outdoor swimming pool, for men only, where it was customary to swim and sunbathe in the nude. Most of the time the weather was mild and sunny; the fog that often enveloped San Francisco came down to Stanford only rarely if at all.

The Hoover Institution was located in a phallus-shaped tower jokingly referred to as "Hoover's last erection." It housed the thousands of books, pamphlets, and documents that Herbert Hoover's employees had amassed in the years immediately following World War I, when Hoover was sent to Europe on famine and refugee relief missions. Since then, much more material on wars and revolutions had been added, including many crates of documents recently obtained in China. The curator of these documents, Mary Wright, was a splendid scholar, outgoing, energetic, and warm-hearted, exceedingly bright, and politically to the left of center. She later taught modern Chinese history at Yale University but died very young. The top floor of the Hoover tower contained the former President's own offices, where he worked, assisted by Mr. Wilbur, his former Secretary of the Interior. One day in the spring of 1948, the entire staff of the institute gathered in the lobby of the building to greet Herbert Hoover on his birthday. He was sitting in a wheelchair when he came out of the elevator. Someone made a speech in his honor, and while the speaker droned on, Hoover sat sunk in his chair, looking old and tired. When it

came time for him to reply, however, he rose, straightened up, immediately looked twenty years younger, and gave an off-the-cuff thank-you speech. He had obviously learned to conserve his energies in very effective fashion; it was an impressive performance.

The Hoover Institution today is a bastion of political conservatism, or at least it has that reputation. That is not, however, what it was in 1947. To be sure, the curator of the Russian collection may have been conservative, but the Director of the Institution was not. And the Rockefeller fellows who were there together with me ranged all over the political spectrum and included at least one man whose ideas were close to those of the Communist Party. Nobody seemed to mind. There was little interaction between the several fellows; each one did his or her research undisturbed by the others. I was given an office and access to the stacks of the library, and I spent my days reading the collected works of V. I. Lenin, trying to find out what that man was about, how his ideas cohered, and what kind of sense one could make of them. I did not know of any work that had tried to explain Lenin's political thought systematically.

To write a book about a political leader's ideas, however, one does not merely read his writings and speeches, even if they are as richly annotated as they were in the second Russian edition of his collected works—twenty or thirty thick volumes in small print. Ideas arise and are developed in several contexts, and to understand what a politician is talking about one has to know this context well. This includes the issues and problems that his speeches, pamphlets and books address, the political culture that has shaped the language, the habits, and the priorities of the man and his party. It also includes the subculture within which he grew up and against which he may be rebelling, as well as the leader's personal background and previous experiences that may have shaped his or her character and view of the world. Of course, having studied Russian history, I was familiar with some of this context, but by no means sufficiently. If the book that came out of this study turned out to be a pioneering work, that was, in retrospect, somewhat of a miracle.

Reading all those many thick tomes of Lenin's work from cover to cover, in Russian, was not only stupid, but also unpleasant. Lenin's writing style was that of a radical orator—angry, repetitive, and dogmatic; full of invective for anyone not agreeing with him; and ultimately very boring. In time, I did get bored, and boredom gave me intellectual indigestion. I had mountains of notes on his ideas and pronouncements, but I did not know how to organize them into anything coherent. That got to be so bothersome that in the end I laid all of it aside and began to read unrelated material, just to give my mind a vacation. One day I was reading a book by George Sorel, when I made myself some notes that turned out to fill an entire page. When I looked over what I had written, it became apparent that I had written an outline for my book—not the outline that I ultimately used, but at least something to start with. The ice had been broken.

Eva was not very happy at Stanford. She had never been away from Cambridge for as much as an entire year and she was homesick. She may also have been bored, with no job of her own, and only a tiny house to keep. Sometime during the year, she got herself a dog, but that was not enough to keep her busy and happy. Eventually, she found work as a research assistant on an interesting project conceived and run by a famous political scientist, Harold Lasswell, and two young political sociologists that oversaw the actual management of the project. It was a comparative study of political culture in different nations based on so-called 'content analysis' of newspaper editorials—an example of the application of 'behavioral studies' or 'positivism' to political science.

For many decades, students of human behavior and human interaction—economists, sociologists, and historians—had looked with envy and admiration at the natural sciences—especially physics. There they saw branches of learning that were rigorously objective and the results of this scientific objectivity were spectacular. On the basis of discoveries made in physics, chemistry, geology and biology, the human species had been able to harness incredible amounts of energy and erect a technology that routinely performed miracles every day. Instant communication over the entire globe was commonplace, as was travel at supersonic speed,

wonder drugs, and synthetic materials built to specification. Science had made many people believe that there are no limits to the human potential for reshaping the world in our image and doing away with material want.

It was generally accepted that this science, which had given us almost god-like powers, depended on certain rigorous methods of inquiry that had been recognized and refined over the centuries. Central to this scientific method was the importance of measurement, hence of expressing all reality in terms of measurable quantities and of making all reality fit into mathematical formulas. A second element of scientific method was that laboratory research, which isolated the phenomena to be measured from outside influences. Still another trend in the development of scientific method was that of breaking reality down to its smallest possible components, referred to as miniaturization.

Positivism in the social science disciplines was based on the conviction that these methods of rigorous scientific inquiry could and must be applied to the study of human behavior and human interaction—to economics, politics, history, social relations, and psychology. Harold Lasswell and Daniel Lerner were pioneers of this kind of positivism. In the decades after World War II, it dominated most American social science, and the development of computers and other data-processing techniques gave it a tremendous boost; for now researchers were able to handle volumes of quantified data that earlier would have totally overwhelmed them. For the cause of its popularity, I borrowed a term (in altered form) from the vocabulary of Sigmund Freud: ‘physics envy,’ by which I meant envy of the rigorous method of the ‘hard’ sciences. I became convinced that the emphasis on these methods was significantly impoverishing the social sciences.

I was first introduced to the behavioral mode in social science when I was a graduate student at Harvard. There the core of the doctoral program was the history of political theory: a survey of philosophic systems from the ancient Greeks to the 19th century, with certain key contributors singled out: Plato, Aristotle, St. Augustine, St. Thomas Aquinas, Machiavelli, Hobbes, Locke and Rousseau. They were the Great Teachers, those who had asked the eternally

important questions about the purposes of government, the duties of citizenship, the bases of authority and legitimacy, human rights, and the advantages or disadvantages of different government institutions and styles. The history of political thought was thus supposed to be a study of the great ideas that had supposedly shaped our civilization.

This approach, of course, ignored the extent to which civilizations, including ours, had been shaped by human passions, most of them combative and destructive, backed up by bad ideas about human failings and differences, but enveloped in a halo of 'noble' principles. This would turn the history of political thought into a history of fraud, deception, illusion, and self-deception. It would turn the history of political ideas into a history of political ideologies.

Other courses at Harvard analyzed political institutions and their functioning—state, city, town, and county government, the interplay between various government organizations, the presidency, various federal departments and agencies, and the Congress. Similar institutional analysis was provided for various government systems in Europe and for international politics and international law. Political thought, taught this way, could stimulate fruitful reflection about citizenship, leadership, authority and obedience, rights and duties. It could also, however, turn into an antiquarian concern with issues that had little relevance to contemporary problems. Indeed, it could be a deliberate flight into an intellectual ivory tower; then all political theory turned into a self-seeking and ultimately futile concern to make the world safe for intellectuals.

The institutional-legal approach also appeared arid as soon as one realized that actual behavior deviates from formal rules. Why study international law when the great powers consistently disregarded it? Why learn about the President's authority over the federal bureaucracy when the U. S. Army Corps of Engineers, with the help of its friends in Congress, could repeatedly defy his will? Why should one be interested in the role of the French *Préfet* or the precise relationship between the British Labour Party and the trades union leadership if these various political institutions were studied purely by the book, in terms of their formal arrangements?

In the 1930's, some students of politics had rebelled against the traditional ways of studying the subject. Their pioneers, teaching at the University of Chicago, called themselves behaviorists. They wanted to make the study of politics scientific. Their model was not actually physics, but classical economics, which they thought had scientific validity, and the ability to predict economic trends.

Economics does deal with quantifiable data—volumes of production and consumption, prices for goods, money and labor, turnover and depreciation. It studies the relationship between these, and is able to express these relations in mathematical terms. On the basis of experience, hunch, bias, and other motives, economists also generate theories about the grand interrelation of all these things, and, guided by theory, formulate hypotheses that can then be tested. In micro-economic research, discrete economic events are isolated and tested as if under laboratory conditions. While some of the hypotheses may try to identify cause-and-effect relationships, most—a bit more modestly—seek to find recurrent correlation between different variables, and probability theory determines whether a correlation is significant or random.

When I was a graduate student, the first beginnings of the behavioral revolution in political science were felt at Harvard. Some of my fellow graduate students began to dismiss the Department's preoccupation with political theory as unscientific; they were interested in 'facts,' not in 'values,' they said. I myself found some of their arguments plausible, although I had not thought about the problem sufficiently to have mature views.

Harold D. Lasswell had studied at Chicago and had become one of the most ardent advocates of behavioral science. If the study of politics could be pursued with scientific rigor, we would be able to predict toward what forms of society the major powers were moving. The scientific method was based on quantification; instead of gathering impressions, we needed hard and fast evidence; and while impressions were subjective, data must be objective and replicable. Since the world of politics was complex and the amount of data infinitely great, we had to employ sampling techniques.

Lasswell was no mere methodological faddist. He was a man deeply committed to a democratic and egalitarian ethic. He distinguished between progressive and reactionary trends in politics, seeking to promote the former and to prevent the latter. One of the first people to warn against totalitarian trends in American politics, he was politically on the Left. That is easily forgotten because most of his disciples wound up more in the mainstream of American politics and ideology. In contrast to them, Lasswell had absorbed the writings of Marx as well as of Freud and sought to synthesize them; he seems to have believed that liberation from oppression and exploitation should be combined with sexual liberation. At the same time, his appraisal of the real world of politics seems to have been somewhat cynical. He was convinced that all government, even the most democratic type, was ruled by elite groups, and he stressed the seductive and deceptive power of words, the importance of propaganda in all politics, and the irrationality and gullibility of the masses. He was fascinated by the power of language. In the sermon, the lawyer's argument, and the rhetoric of the politician, words became tools to sway minds and attitudes. Hence, he was interested in symbols, their manipulation and their effect.

During his entire career as a student of politics, Lasswell had been interested in propaganda and its use in political life, and he was convinced that by studying propaganda we would be able to predict political behavior. The choice of propaganda 'symbols', he apparently believed, not only revealed the intentions of those that emitted the messages, but determined the actions or inclinations of those that received them. What distinguished Lasswell's study of propaganda from other such studies was the quantitative approach. He broke propaganda messages down into their smallest component parts, words that he called 'symbols'. Symbols were categorized according to the message they carried—democratic or authoritarian, critical or laudatory, referring to specific 'values' like wealth, prestige, or security. He then counted the changing frequency with which key symbols were used, placed the resultant numbers into statistical tables, and these tables supposedly revealed trends of developing political attitudes. When extrapolated to the future, the statistical curves suggested where the countries under study

were headed. The entire procedure was called content analysis, although the content of communications was precisely what this method of word counting neglected.

Content analysis had been applied to the slogan issued each year by the Soviet Communist Party for its May Day celebrations as well as to propaganda statements by the Communist International. These were ritualistic statements made on regularly recurrent occasions, and therefore they were ideally suited as material for tracing shifts in the Kremlin's priorities between 1918 and 1943. This research, therefore, did no more than provide 'scientific' confirmation of trends long recognized by careful observers of Soviet and *Comintern* politics—that the Soviet Union had become conservative and isolationist, and that the Communist International was a total failure, and its programmatic statements had become increasingly unrealistic. Content analysis of this material not only proved the obvious; it was wasted effort, because these obvious insights were unpopular among the political, academic, and journalistic elite of the United States, to say nothing about the general reading public; therefore this type of work had no influence.

The project set up by Lasswell at the Hoover Institution, and for which Eva did some research work, was called RADIR, meaning Revolution and the Development of International Relations. It was an exercise in content analysis. What was to be investigated was the changing frequency of democratic words ('symbols') appearing in the editorials of establishment ('elite') newspapers in different countries between 1890 and 1950. Democratic symbols were words symbolizing shared power, shared respect, shared wealth, skill, or well being. When the study appeared in print, its conclusions were disturbing; they suggested a growing polarization of the world and trends toward militarization and totalitarianism everywhere. Could that, however, not have been predicted on evidence other than a mechanical sampling of words?

When Eva discussed her work with me, we both agreed that what she was asked to record was indeed mechanical and mindless—key words or phrases that fitted into any of the categories Lasswell had established. If the word 'war' occurred, that was taken to be an indication of

belligerence on the part of the editorial writer. Words, however, are never used standing alone; instead, it is the context that gives the word positive, negative, ambivalent, or valueless connotation. Content analysis as practiced in this study neglected context and therefore overlooked the evaluation behind the words. In this way, the 'content' of the editorials being studied was disregarded. What was being studied was empty form. There were many other things wrong with this project, such as the choice of newspapers deemed to be representative, the exclusion from the study of any attempt to show the effect of these symbols on the reader, and the neglect of the style in which the editorials were written. When I communicated my reservations to one of the two men whom Lasswell had left in charge of the project, he asked me to put my criticism in writing; he even paid me a few dollars for it. Some years later, I was pleased to discover that Lasswell himself seemed to agree with much of my criticism.

Lasswell had many young disciples who took from him his quantification methods and his elitist view of politics. They did not, however, adopt his breadth of vision, his humanist attitudes and radical critique of trends in American society, and certainly not his habit of questioning his own views and methods. Behaviorism in general, with its emphasis on quantification and mathematical modeling became the dominant mode of studying politics. Its preoccupation with rigorous methods of inquiry induced many of its advocates to spend much effort on money on irrelevant, trivial stuff just because it lent itself to measurement. In particular, there was a tremendous growth in the study of electoral behavior which, given the basic similarity of the two major parties in the United States, and the practical lack of any meaningful alternative, represented a preoccupation with political shadow-boxing.

Mathematical precision in the study of politics was misplaced not only because what was quantifiable often was much less interesting than what did not lend itself to quantification, but also because to concentrate on what is measurable often meant asking the wrong questions. More generally, no terms in the study of human relations were well defined. Words such as democracy, equality, legitimacy, welfare, and the like, had multiple meanings. Further, in human relations

context is everything; nothing is unrelated. Finally, all social science theory had to begin with some assumptions that were not provable, such as a rational actor theory, a theory of human depravity or, conversely, of innate human goodness, of a theory of development or progress. These initial assumptions colored all our studies, including those done with rigorous behavioral methods.

My life's work—the study of the Soviet Union and of communist regimes in general—made me sensitive to the issue about the validity of the positivist approach. The reasons for my sensitivity were several. First, the Soviet Union until the late 1950's was absolutely inaccessible, and even after that, access to it was severely limited. Western scholars could not travel everywhere, could not freely talk to Soviet citizens, and did not have access to governmental records. Simply because of this, some of my colleagues seriously advised doctoral students not to study Soviet or communist political systems; since they could not freely gather quantifiable data there, they would be able to produce only worthless impressionistic studies.

The general assumption among political scientists in the United States, and indeed in the Western world, was that the Soviet Union and its client states were unique and that therefore one could not, or should not, apply to it social science models or concepts used to study other political systems. Those of us who tried to integrate Sovietology with general social science by using its language were accused of equating communist states with Western societies. Totalitarianism, we were told, was a unique and novel phenomenon; and to describe totalitarian societies in terms borrowed from theories about Western societies was an attempt to play down this uniqueness and novelty. Moreover, since the concepts and models specific to the study of Western societies were specific to democratic societies and therefore had moral implications, their use in the study of communist systems implied a shameful betrayal of democratic values. Some critics of Sovietology called this alleged betrayal the theory of moral equivalency.

Sovietologists, in general, were trained to be aware of contexts and connections; they were alert to the inseparability of such factors as geography, economics, history, literature,

religion, culture, social structure, and even language. In order to study Soviet politics properly, they had to have some knowledge from all or many of these disciplines. In the eyes of the strict methodologists, that made them appear as dilettantes; and in many academic departments, 'area specialists,' devoted to studying a particular region of the world, often found themselves on the margin of their profession, not promoted to tenured rank, not rewarded with high salaries. Later, after the collapse of the Soviet Union and its European client states, they were criticized for not predicting these events. Now that the successor states are open to any Western scholar who wishes to do research there, the positivists have begun to rush in to do their survey work. Their ignorance of the relevant languages, history, geography, and so forth, does not seem to trouble them.

A joyous event occurred just at the time I had completed my doctoral dissertation; the birth of our son, Stefan Garris. Garris was the name of a dear friend of ours, the tenor John Garris, whose career with the Metropolitan Opera had been taking a promising turn until he was murdered while on tour in the South. Whenever he happened to be in Boston, he and his male companion would visit us. He would sing entire Schubert song cycles while sitting at our piano, accompanying himself. Shortly before Stefan's birth, the Metropolitan Opera had performed in Atlanta, and the next morning John Garris was found dead in an alley. By giving his name to our son, we set him a monument in our hearts. The initial, G, however, was also to remind me of my murdered father, Gustav.

Once Eva came home from the hospital with the baby, there were several forces that that clashed in our home: the baby's, his mother's, the pediatrician's, my own humble opinion, and that of my mother-in-law, whose strong will won out over all the others. The clash of wills and opinions led to such friction between Eva and her mother that we decided to flee the field of battle, take a vacation, and let Grandma take care of the baby for a while. During that vacation, in a farmhouse near Peterboro, NH, the baby's sister was conceived. She was born a year and a week after her brother, and was named Vera.

My dissertation was finished. The formal defense seemed little more than a formality. In the summer of 1950, I got my Ph.D. The Russian Research Center kept me on as a post-doctoral fellow and assistant to the Director; and in my last year there, my title was changed to Assistant Director. That was a purely cosmetic change; I am not sure whether my duties were different from what they had been before. I taught a couple of courses at Harvard—one on Marxism and another on totalitarian governments, both fascist and communist. Some of the students I had in these classes had interesting careers, subsequently; having studied with me did not seem to have permanently handicapped them. I enjoyed this work, and I was pleased to be a member of the faculty in so distinguished a university.

Whenever I had lunch at the Harvard faculty club, I would order the specialty of the house—horse steak with mushroom sauce. It had been introduced into the menu during the war and turned out to be so popular that the chef continued to serve it. In the lobby of the club one could see a large lithograph by Daumier showing two men dining at a horse meat restaurant; one of them seemed barely able to choke his meal down. Women were allowed into the faculty club, but had to use a back entrance.

Clyde Kluckhohn was then working, together with Alfred Kroeber, on a book in which these two cultural anthropologists sought to survey all the meanings and connotations that the term 'culture' had assumed. Because I was fluent in German and Russian, he asked me to do some research assistant work for him. I was to survey the many uses of the term in German, Russian, and Soviet writings. I did as told and, after some weeks or months, handed him two lengthy reports on the matter. Kluckhohn and Kroeber liked these reports so much that, without any alteration, they printed them as appendices to their book.

I learned about this only after the book came out. It was the first time that I had seen my name in print as the author of anything, and was therefore something of a milestone. Years later, an anthropology professor asked me whether I was THE Alfred G. Meyer; I knew exactly what

he meant, because in those years the book on culture was must reading for all cultural anthropologists.

The 1947-48 academic year ended. It was summer, and by the end of August, my fellowship at the Hoover Institution was coming to its end; with it the monthly stipend and the library privileges. I do not remember having made any effort to find employment for the next year; if I did, I was unsuccessful; if I didn't, it was because I knew nothing about the academic job market, and nobody had found it necessary to enlighten me about it. By the beginning of August, however, I must have started to worry about it.

Then, in the middle of the month, I received a job offer for an instructorship at a distinguished liberal arts college in New England. The offer came from the chairman of its political science department, who had been a guest professor at Harvard, and I had taken some course work from him. The salary he offered me was \$2700 a year. I immediately sat down to type a letter of acceptance when I received a wire or phone message offering me a job as Graduate Student Fellow with the newly created Russian Research Center at Harvard University. The pay was slightly higher. My duties would be to complete my doctoral dissertation and to participate in the Center's discussions and seminars. It seemed a good deal, and I accepted it.

In subsequent years, I often wondered whether that had not been a mistake. Had I accepted the teaching position, I would have received tenure within a few years, and that would have meant lifetime security at a very fine school. My job worries would have been over forever, unless I was an academic entrepreneur who used his current position merely as a jumping-off point for a more attractive one. I was never such an entrepreneur and paid a price for this. Thus, job worries stayed with me for another decade.

To Eva's delight, we moved back to Cambridge. Just at that moment, her parents moved to Vermont because her father had taken a job as a professor of economics at Middlebury College. We therefore could move into their old apartment and did not have to hunt for housing. At the Russian Research Center, then located in an old frame house on Quincy Street, I was given

a desk in an office that I shared with another student fellow, and then I concentrated on writing my dissertation.

The Russian Research Center had been created in early 1948 on the basis of a generous grant from the Carnegie Corporation, whose President, John Gardner, took a personal interest in it and made his weight felt in its management. When Gustav Hilger came to the Center to look for a co-author, Gardner insisted on interviewing every candidate for this job himself.

Hilger, born in Moscow, the son of a German industrialist, had spent virtually all his life, except for a few years as an engineering student, in Russia; he knew the country and its culture well and actually spoke German with a slight Russian accent. When, in 1919, the German republic resumed diplomatic relations with Soviet Russia, the German embassy in Moscow hired him as someone familiar with the country. Except for a very brief interlude, he stayed at the Embassy until the time of the German invasion in June 1941, rising to the rank of Embassy Counselor. Although many of his views on international politics, and on politics in general, were naive and unsophisticated, his reputation among the members of the Moscow diplomatic corps was high. He had, after all, dealt personally with virtually every major communist leader—Lenin, Trotsky, Stalin, Bukharin, Radek, Chicherin, Rakovski, and many others. He could be a fascinating *raconteur* about his experiences with these fabled people. Of course, he also had been associated with interesting personalities in the German Foreign Service in the Weimar republic, as well as in the Third Reich.

After the war, when the Soviet Union wanted to try him as a war criminal, he went into hiding and his friends from their Moscow days, George Kennan and Charles ("Chip") Bohlen, secretly spirited him to the United States. They set him up in an apartment in Washington and hired him as a consultant to the Department of State, where he wrote position papers on past and present issues. They also urged him to write his memoirs, but he correctly argued that he did not have the talent to write a book and needed an academic person to do it for him or with him. Kennan then went to the Carnegie Corporation to get funding for such a project, and John

Gardner referred him to the Russian Research Center. Thus, Hilger came to Cambridge to pick a co-author for his memoirs. I was one of the graduate students he interviewed, and he and I liked each other, even though (or, perhaps, because) I identified myself to him, from the very beginning, as being to the left of center politically. When John Gardner interviewed me and asked for my opinion of Hilger, I voiced some opinions that he thought disrespectful, and he strongly opposed my appointment as Hilger's co-author. The Director of the Center, Clyde K. M. Kluckhohn, managed to make him acquiesce in my appointment, however.

Kluckhohn, a cultural anthropologist, had done important fieldwork among the Navajos of New Mexico. During the war, he, together with Ruth Benedict, had studied Japanese culture for the OSS. Several of the senior fellows at the Center had had similar connections. Barrington Moore, perhaps the sharpest mind I have ever met, had worked there, and so, if I am not mistaken, had Alex Inkeles; and one of the non-academic godfathers of the Center, still known as "General" Osborne, had been with the Psychological Warfare branch of the War Department. When "the General" came to inspect the Center, we were warned to be on our best behavior.

From the very beginning, the hidden connections to the Federal government made themselves felt in the form of ideological screening. One of the graduate student fellows, a historian, turned out to have had left-wing associations, and quickly lost his appointment; and the Assistant Director, a brilliant historian, was removed from his job because during the 1948 election he supported Henry Wallace and his Progressive party. He got an appointment at Stanford, but ultimately returned to Harvard.

In 1952, when I was serving as Assistant Director of the Russian Research Center, Clyde Kluckhohn once called me into his office for a confidential chat. "Once in a while," he said, "I send a memo around to all the members of the Center in which I suggest that we discuss a specific problem." Of course, I had seen such memos and responded to them. "Well," he continued, "such suggestions of mine usually come from the local field office of the CIA, who phone me, saying, 'Our uncle in Washington would like to know what you people think about

such and such a problem.” Kluckhohn told me that during the next semester he was going to be on leave, and the CIA agents wanted someone appointed to be their contact person. “Would I mind serving in that function?” he asked.

“Not at all,” I said, “but I guarantee you that I will flunk clearance.”

He did nominate me, and came to me weeks later in considerable embarrassment to tell me that I had indeed flunked clearance. He then nominated a senior professor of impeccable ideological credentials, who also flunked clearance and was furious about it. The third nominee finally passed muster.

In the earlier decades of the post-war era, many Americans regarded people engaged in Russian and Soviet studies with suspicion. In 1949, I was chatting with a neighbor—an officer on leave from the Navy to pursue graduate studies at MIT. He asked me what I was doing, and I told him I was writing my doctoral dissertation. He asked on what topic I was writing, and I told him it was on Lenin. Whereupon he looked at me with a cold stare and said, “Well, I guess I disagree with you a hundred percent.” A year or two earlier, I had given a talk on some aspect of Russian history or Soviet government at the International Student Center in Cambridge. During the discussion after my presentation, a student asked me whether I would like to live in Russia. When I said that I would not, she asked, “Then why do you talk about it so much?” Similarly, in the 1950’s, students in my courses on Marxism or Soviet politics made it clear that they did not want their parents to know that they were taking such courses.

At the same time, those engaged in these studies believed that they were serving an important national intelligence function; so, obviously, thought those that had created and financed institutes like the Russian Research Center. So also thought the students who nicknamed my courses the ‘Know thy enemy’ courses. Indeed, in the 1950’s, many of my students were politically conservative, conventional in dress and appearance, and often came to class in their ROTC uniforms; ten or fifteen years later they would be replaced by the radical students of the counter-culture.

At the Russian Research Center, we took it for granted that our scholarly work was in the national interest. That was made even clearer a few years after the Center had been founded. In the early 1950's, it launched an ambitious project that sought to apply up-to-date social science survey methods to large numbers of former Soviet citizens then living in the Western world, primarily in Germany and the United States. That project was 'target research' financed and sponsored by the U. S. Air Force; they, quite understandably, were interested in our exploring the strengths and weaknesses of Soviet society. Simultaneously, the U. S. Navy was financing a similar project headed by Margaret Mead. They wanted her and her colleagues to apply psychological and anthropological techniques to predict Soviet behavior under stress; a typical question they wanted answered was how Soviet submarine crews might be expected to react when bombarded by depth charges.

Opponents of the cold war have often assumed that research sponsored under such auspices would be politically tainted, and that its results would therefore be invalid. Anyone familiar with the studies produced by the Air Force project, however, will know that these studies in fact went against the grain of generally accepted cold-war images about the Soviet Union. The works produced on the basis of thousands of interviews with former Soviet citizens actually dispelled much ideological nonsense about the nature of Soviet totalitarianism. The totalitarian 'model' of the USSR had been sketched out by George Kennan's famous article, in which he suggested the heavy fist of dictatorship had so atomized the society that the social structure had virtually been destroyed.

Kennan further suggested that no significant groups in the society had benefited or were benefiting from Soviet rule, and that the Party would therefore be able to maintain itself in power only if it engaged in foreign adventures. According to Kennan, the compulsion to expand into other lands was built into the Soviet system. Ideas of this kind were supplemented by books like those of Nathan Leites, who argued that the leaders of the Soviet Union were, in essence,

suffering from collective paranoia and were therefore likely to respond only to force, not to reason.

The survey research done by the Russian Research Center for the U. S. Air Force demonstrated the falsity of these and other images linked to the totalitarian model. It discovered widespread resistance to party policy, evasion of party rules, and a variety of informal arrangements that people made under the cover of formal rules and regulations. It showed that this 'informal' behavior often looked like inefficiency, but just as often allowed the entire system to function. It discovered groups of the population who had benefited from Soviet rule, and found out, rather to the scholars' surprise, that even some of the sharpest critics of the regime had some good things to say about it, and felt they had shared some of its benefits. In short, the interview project had discovered an actual society beneath the hard crust of the party dictatorship.

Just how unpopular some of the findings of the project were with the American public, saturated as it was with cold-war propaganda, is illustrated by a response that Clyde Kluckhohn received when he reported some of the results of the survey in a lecture at a Harvard alumni club. He told the alumni that many of the former Soviet citizens had been interviewed twice, the first time in a German displaced persons camp, the second time after immigrating to the United States. Among those interviewed twice, the trend had been to voice disappointment in the United States, after being here for a while. Kluckhohn was booed for giving this information and was unable to continue his lecture.

Just because research was sponsored by the government did not mean it was flawed. The military, after all, was interested in facts, not ideology. As they used to tell us at Camp Ritchie: "Your Commanding General is not interested in the enemy's intentions, but only in his capabilities." The honesty of the work done by the Air Force project should have been compared to the dishonesty of some of the work done at the other end of Massachusetts Avenue. Sometime around 1950, a Center for International Studies had been set up at MIT. Some of its leading scholars, as well as the financing of the Center, came from the CIA. One of its projects was to

produce a book on the nature of the Soviet system. Its chief author, possessing no specialized knowledge of that subject, hired a couple of young historians to do the basic research and, in effect, draft the book for him. Most of these drafts were done by a good friend of mine, a solid scholar and, today, a widely acknowledged authority on the history of communist revolutions and their regimes. To his dismay, my friend discovered that the principal author was rewriting his drafts in such a fashion as to give all the material a Cold War twist, thus systematically transforming his scholarship into ideology. Although being identified as a principal contributor to the book would have given my friend's academic career a tremendous boost, he did not wish to be identified with such intellectual dishonesty, and therefore asked that his name not be mentioned in the book.

The Russian Research Center was governed by a board of directors who were senior professors at Harvard; most of them were not specialists in Russian or Soviet studies. Besides Kluckhohn, the most influential member of this board seems to have been the then eminent sociologist, Talcott Parsons. Members of this board reviewed all book manuscripts produced by Center staff personnel; the Center had an agreement with Harvard University Press, according to which the Press would publish any book produced at the Center as long as the board of directors gave it their imprimatur. In the case of my books, Professor Parsons seems to have written the decisive positive opinions, although in his recommendations, he made judgements about them that made very little sense to me.

The working staff of the Center included a number of senior fellows who also held professorial appointments in various departments of the university—Merle Fainsod in the Department of Government, Alexander Gerschenkron in Economics, Harold J. Berman in the Law School, and others. They may also have held membership on the board of directors at one time or another. Most of the graduate student fellows, all of them working on their dissertations, and graduate student assistants who were not yet at that stage also did genuine pioneer work in Soviet (and related) studies and became leading scholars in their fields. They included Gregory

Grossman, Franklyn Holzman, and Joe Berliner in economics, and Robert V. Daniels, Hans Rogger, M. Kamil Dziewanowski, Mimi Haskell Berlin, and Richard Pipes in history. Others included Marc Field and H. Kent Geiger in sociology, Adam Ulam and Zbigniew K. Brzezinski in political science, and Raymond A. Bauer in social psychology. When I took a seminar in 19th-century Russian history with Professor Michael Karpovich, the class included Don Treadgold and Nicholas Ryazanovsky, Marc Raeff, Leopold Haimson, Richard Pipes, Robert MacMaster, and Stephen Fischer-Galati—a stellar group, all of whom became important people in their field.

All people in the Center—from the Director to the clerical employees—were on a first-name basis, which made me feel uneasy, because I had been reared in the spirit of strict Prussian rules of politeness. I don't think I ever managed to call Merle Fainsod or Talcott Parsons by their first names; but that was my hang-up, not theirs. Altogether, the Center was a remarkable community of very bright people, most of whom earned great distinction in various areas of Russian, Soviet, or Chinese studies (for some years, Chinese studies, not yet having its own center, were an appendage of the Russian Research Center). After the Center moved to a larger building, that had once been a dormitory for the wealthiest Harvard undergraduates—a so-called gold coast dorm—each of us had his or her own spacious office and a typewriter. Lunch was served every day at the Center, so that we could meet informally to share ideas and information.

The Center had its own library with a growing accumulation of microfilms. Whenever any of us, senior faculty or graduate student, wished to present some of his or her work to the whole group, a seminar would be held. Similar seminars frequently featured interesting people from the outside, whether scholars from other universities or people who had inside information, such as former Soviet citizens or former activists in communist parties. There was plenty of stimulation, discussion, and disagreement. We all learned a lot from each other and, since Russian and Soviet studies were so new to the American academic scene, we thought ourselves intellectual pioneers mapping out hitherto unknown territory. (Many of us were unfamiliar with

the mapping that had earlier been done by German scholarship, both in the Weimar period and under Hitler, and by Trotskyite, Bukharinist, and Menshevik critics of the USSR.)

Former communist leaders or functionaries who had turned to the far right politically frequently gave presentations at the Center. Ruth Fischer, for instance, was a former General Secretary of the German Communist Party, now collaborating with the House Committee on Un-American Activities, before which she testified against various people, including her two brothers, Gerhart and Hanns Eisler. She would talk to us about her own experiences as the leader of German communism, and how her party was manipulated by its bosses in Moscow; she also kept herself informed about current developments in the communist world, and since she was a former insider, her judgment carried weight. This was less true of Franz Borkenau, a former functionary in the German communist student movement, whose interpretations of current developments within international communism at times seemed far-fetched. When asked where he got his information, he would tell us that he had special intellectual antennas or intuition (he used the German term, *Fingerspitzengefühl* (literally, that means, special sensitivity at the tip of his fingers) that enabled him to understand these things. Another former communist activist was Karl August Wittfogel. He had served in the cabinet of the short-lived socialist and communist coalition government in Saxony in the early 1920's. After that, he had remained active in the party. I once saw a collection of flamingly revolutionary one-act plays for the proletarian stage that he had written. He had been in France, I believe, when the Germans had marched in, and an eminent scholar studying Inner Asia had saved his life. That was Owen Lattimore, of Johns Hopkins University. Lattimore had invited Wittfogel to the United States, or in some way arranged for his immigration. When, in 1950, Senator Joseph McCarthy in vicious and totally irresponsible fashion destroyed Lattimore's career, Wittfogel does not seem to have defended Lattimore and may, indeed, have assisted McCarthy.

In 1954 or 1955, I participated in a faculty seminar at the Center for Far Eastern and Russian Studies at the University of Washington. Wittfogel at that time had some appointment

both at Columbia University and the University of Washington. He was then working on his major work on the concept of Oriental Despotism, and the members of the faculty seminar had been given a draft of one of the chapters in this book. There was a lengthy note in that chapter in which Wittfogel denounced all those communist scholars or ideologists who had falsified Marx's and others' ideas concerning oriental despotism. The list of these falsifiers included Lenin, Stalin, Mao Tse-tung, a few American communists and John K. Fairbank.

Professor John K. Fairbank taught Chinese history at Harvard. He was a scholar of the highest repute, incredibly erudite, a marvelous teacher and a warm friend. He was mild-mannered and courteous in a way that made him appear almost like an eminent scholarly mandarin and he was as thoroughly decent a person as I have ever met. Where he stood politically, I have no way of knowing, but I assume that he was no further to the left of center than John Kennedy's principal advisers. I have reason to assume also that John Fairbank had hated the corrupt and oppressive *Kuomintang* regime. Millions of people in China had loathed that regime so deeply that they had greeted the communist troops of Mao as liberators. Revolutions generally serve as punishment for oppression and corruption practiced by the regimes they overthrow.

When Wittfogel appeared at the seminar in Seattle, a young colleague of mine began the questioning by asking him whether he had meant to imply that John Fairbank was a communist. Wittfogel answered evasively, whereupon I took the cue from my colleague and asked the question again, rephrasing it. Wittfogel once more refused to give a straight answer. The two of us, my colleague and I, did not give up, but tried to pin him down, until at last the Director of the Center lost patience and directed the conversation to some other point. After the seminar, Wittfogel buttonholed me and said, "I honor your loyalty to your friend John Fairbank; of course, I know he is not a communist, but I refuse to say that in print."