Gabriel A. Almond

VENTURES IN POLITICAL SCIENCE

Narratives and Reflections

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Ventures in Political Science

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1

Introduction

MOST OF THE CHAPTERS IN THIS BOOK were written in the 1990s, the decade of my eighties. The book treats topics appropriate for an octogenarian—historical narrative about the political science discipline, and reflections about democracy and democratization. But in this first part of this introduction I write about my *lehrjahre,* my education and early career, which gave me the tastes and distastes that I express in this and other publications.

I had a long apprenticeship. I wasn't really on my way, so to speak, until 1946, after World War II, when I was in my mid-thirties. It was then that my European and German experience combined with my University of Chicago training to give me access to research opportunities in comparative politics and international relations. How this came about is told in Chapter 5, "A Voice from the Chicago School," where I place my beginnings in the setting of the University of Chicago in the great days of Charles Merriam and Harold Lasswell. The University of Chicago thread takes me from the Midway in the 1930s to Yale in the 1940s.

It was the Yale Institute of International Studies under the leadership of Frederick S. Dunn and William T. R. Fox that I was to join after World War II. Fox, trained under Quincy Wright, Merriam, and Lasswell at the University of Chicago, had been brought in by Dunn to help develop the institute along the lines of the new political science. Fox in turn had brought in Bernard Brodie and Klaus Knorr—also Chicago Ph.D.'s, the former specialized in military and security affairs, the latter in international economic affairs. Bringing me in furthered the Dunn-Fox strategy of basing postwar international relations research and teaching on the social sciences, in addition to its traditional legal and institutional components.

I had returned from the wars no longer a fit with my Brooklyn College job, where I was limited to the teaching of U.S. government. I was bursting with the latest information on the politics of post–World War II Europe. And I had a set of methodological tools, primarily from Harold Lasswell, waiting impatiently to be put to use on challenging problems. I needed an academic setting where I could pursue my studies of the new world of politics that was taking shape at the end of World War II. Yale had pioneered in the development of international and comparative studies, establishing a research institute in international affairs and offering graduate degrees in international relations already in the 1930s. With the University of Chicago additions, Yale was very strong, clearly the strongest center in the country.

Within a few short years it had published the book that defined the structure of the postwar system of international relations (Fox, 1944), the book capturing the essence of the new postnuclear international security system (Brodie, 1946), and the first book treating U.S. foreign policy from a sociocultural perspective (Almond, 1950). However, this very prominence and visibility, and its research support by foundations, made the institute vulnerable to the envy of parts of Yale not so favored, and to the anger of disappointed colleagues.

My shift from Yale to Princeton might be explained by wounded vanity, that of a Yale president whose tenure had been delayed, who was said to have been "born with a silver foot in his mouth" by a Princeton president who described these events in classic Ivy League terms: "Yale fumbled and Princeton recovered the ball." For all of its pointlessness, the Ivy League battle spread Yale's treasure of innovating scholarship in international studies to Princeton, Columbia, MIT, and the Rand Corporation. It provided me a career with foundation support and limited teaching obligations from 1946 to 1963. The privileges and resources made available to me during these years made it possible for me to produce my U.S. foreign policy book (1950), *The Appeals of Communism* (1954), *The Politics of the Developing Areas* (with James Coleman and others, 1960), and *The Civic Culture* (with Sidney Verba, 1963).

To explain this productivity I would have to go back a bit in time, and away from the coasts where I spent my mature career, to the Middle West and the University of Chicago during the time of the Great Depression, which coincided with my graduate years. I did my first serious research while directly experiencing the depression, and then later in World War II in Washington, D.C., and Germany. From these beginnings I have always thought of political science as dealing with very urgent and palpable evils, such as civil conflict, economic breakdown and poverty, and war, and hence being strongly impelled toward the applied rather than the pure side. Its subject matter is rather more like the "clouds" of meteorology than the "clocks" of physics.

Chance brought me to the University of Chicago at a time when great innovators in the social sciences were at work in the improbable setting of the Chicago Midway in the 1920s and 1930s. I believe we are still living off the solid yield of Chicago's Merriams, Lasswells, Gosnells, V. O. Keys, and Trumans, while some of the recent "innovations" in political science may turn out to be affectations.

I received a notice from the university in the spring of 1932 of my admission to graduate study in political science, with a "service" scholarship. It seemed the logical thing to do after receiving the admission notice to go to the bookstore and buy a copy of Aristotle's *Politics*. It is interesting that I did not buy Plato's *Republic*. Aristotle was empirical and quantitative, a political sociologist, attaching significance to the same variables that had been emphasized in the social sciences in my undergraduate years at the University of Chicago. Whatever my motive in reading Aristotle, it left me with a respect for the tradition of political theory, and with a healthy regard for classification and typology.

The value of a graduate "service" scholarship then was $300, just enough to cover tuition for three quarters, and one had to do a certain amount of departmental work (like grading papers) to "earn" the scholarship. In order to save a bit of money, and help relieve financial pressure at home, I got a job as a casework aide in the Stockyards district of the Unemployment Relief Service. Since the rate of saving was slow on a monthly salary of $87.50, I delayed my entry into graduate school until February of 1933. But my early employment at the relief service had a great influence on my future, as did my entrance into the University of Chicago at this creative time.

My job at the Stockyards unemployment office was to interview newly unemployed applicants for relief, and relief clients who had complaints. They were mostly foreign-born—Poles, Russians, Ukrainians, Bohemians, Slovaks, Lithuanians, as well as Greeks and Italians, Mexicans, and African Americans. It was my job to hear their stories, and decide whether they should be seen by a caseworker, a great responsibility for a twenty-one-year-old fresh out of college. The caseworker would have to decide how needy they were, whether they had other sources of income or support, and the like. I sat at a front desk with two other complaint aides, taking down pleas, demands, even threats as these men made their cases (they were all men, mostly fathers; it didn't strike me as odd at the time). As I sat there day after day writing complaints on three-by-five slips of paper, it occurred to me that I was witnessing human behavior, and that perhaps it was interesting and researchable. I had taken a course in "Nonrational Factors in Political Behavior" with Harold Lasswell in my senior year at the University of Chicago, in which Lasswell had invited us to consider all human interaction and behavior as relevant to politics.

I remember, as though it were yesterday, telephoning him from a phone booth near my place of work and telling him with great excitement what I was doing, where I was working, and that it looked like a great research opportunity. He agreed with me, and we set up a project under which I would have the complaint aides in that office record on each slip of paper how the client acted—whether he made aggressive demands, was submissive and ready to go away at the least show of administrative impatience, or wheedled and smiled his way to a favorable outcome. Over a period of six months we would have several thousand complaint slips, each marked to indicate how the client made his complaint. Over the six-month period most clients would have appeared several times, before more than one complaint aide, satisfying the need for control. We then could classify our cases according to their behavioral propensity, and look into their case histories to explore the association between behavioral patterns and demographic, occupational, educational, even police backgrounds.

The article that Harold Lasswell and I wrote, reporting these data, appeared in a 1934 issue of *American Political Science Review,* under the title "Aggressive Behavior by Clients on Public Relief." Lasswell supplied the theory, which argued that protest and revolutionary politics would emerge out of the anger of the resentful unemployed, and that the aggressive types among the unemployed were the potential protest elite. In other words, this apparently innocent research enterprise at the Chicago Stockyards was giving us some hints on what the revolutionary American elites might be like, if there were an American revolution. And in 1932 in the Chicago Stockyards district, riot and revolution didn't seem so farfetched. In this enterprise I supplied the data and basic analysis. This early published evidence of my commitment to research convinced Lasswell, Harold Gosnell, and most important, Charles Merriam, that I might be worth carrying through to a Ph.D.

To do that I had not only to pass examinations in all of the major fields of political science, but to write a dissertation. At the time, these two hurdles seemed like insurmountable heights. I remember sitting next to V. O. Key Jr. at one of our departmental teas. He had passed his prelims the year before, and I asked him, naively, whether he thought that someone like me could pass the prelims. In his slow, Texas way, V. O. said he did not put it beyond the realm of possibility. Those of us who passed their prelims that spring of 1935 went to the Edgewater Beach Hotel (which no longer exists) and danced by the then sparkling waters of Lake Michigan.

As for my dissertation, Lasswell was developing "elite" theory at around this time, arguing that politics could be boiled down to "a struggle of elites over who gets income, power, and safety." The book he was writing then was called *Politics: Who Gets What, When, and How* (1936). The student milieu in which I lived in the 1930s was alive with left-wing politics. There was a sit-down strike at the Republic Steel plant, which drew students into sympathetic participation. The Young Communist League, the Young Socialist League, and the Trotskyites were actively recruiting on the campus. Socialism, Marxism, and Communism were all in the air, and Fascism was the threatening enemy. I was moved by all these events and trends, but part of me was detached, wanting to get into the causes and consequences of it all.

I was granted a predoctoral field fellowship by the Social Science Research Council in 1934, enabling me to spend the academic year 1935–1936 in travel, research, and writing. I decided to do a dissertation on the political power and influence of the economic elites in the United States, doing nothing less than an empirical test of Marxist theory that bourgeois democracy was dominated by capitalism.

I decided to do the dissertation in New York City, since its colonial origins gave me some historical background to compare with the present, while Chicago, where I had begun my research on elite politics, had a shallow past, no more than a century. This was a fateful choice, for in New York City I sought out Hans Speier, who was then engaged in recruiting for the "University in Exile" of the New School for Social Research. I sought him out for what he could tell me of Max Weber, who had become my scholarly hero. He and Lisa Speier conspired to bring me together with Dorothea Kaufmann, my future wife and life companion, the mother of my children, and a steadfast advocate of the welfare of the child.

The unifying philosophy of *Ventures in Political Science* is that the politics we study in political science is an objective reality and probabilistic in its unfolding. The aggressive unemployment behavior that I recorded, numerated, and interpreted in my first political science research encounter in the summer, fall, and winter of 1932–1933 gave me an empirical grounding that lasted the rest of my academic life. That winter was a hard winter, made brutal by hunger and hopelessness. The desperation and anger of the unemployed was an "objective reality," not a "constructivist" one as some of our contemporary methodologists might assert, nor something that could be captured in mathematical equations.

I spent the academic year of 1935–1936 in the New York Room of the New York Public Library, poring over biographies, memoirs, reports of social clubs, old almanacs, and records of philanthropic activities. I called my dissertation *Plutocracy and Politics in New York City.* What I was able to demonstrate in this study of the relation of the economic elites to politics over the course of the three centuries of New York's history was a transition from an economic oligarchy to what I called an open plutocracy. This was an ambiguous kind of power structure in which the business elites were influential but not all-powerful, and not in direct control. They had access to political organizations and elites who defended their interests in return for financial support. When the corruption of the politicians got out of hand, part of the business elite entered directly into politics through reform movements, reestablishing some equilibrium between the political "machines" and the public service. Similarly in the crises of depression and war, the business elites would be drawn into more direct involvement in politics and public service.

For my dissertation, I in effect had done a case study of New York City's politico-economic development, in a test of the Marxist proposition regarding the control of the polity, despite appearances, by the economic elite. Marxism was a hypothesis at this point in my thinking about politics, and my dissertation research left me with a sense of the interrelation between economics and politics more subtly modulated than was prescribed by Marxist theory. It took a bit of chutzpah to do that kind of dissertation, and not all of the departmental faculty viewed it as an appropriate topic. But Harold Lasswell, Harold Gosnell, and Charles Merriam were the members of my dissertation committee. And these blessed scholarly innovators combined to give me my doctoral imprimatur.

Some explanation is necessary for the fact that my doctoral dissertation was not published until 1998, sixty years after it had been accepted by the department. In 1938 it was a quasi-requirement, not enforced, that doctoral dissertations had to be published in order to complete the requirements for the Ph.D. degree. It was my intention in 1938 to revise my dissertation for publication, and I did indeed work on it for several years. One of the principal conclusions of the manuscript I submitted in 1938 was that while the democratization of the suffrage in the nineteenth century had broken the political monopoly of the economic elites, they nevertheless were enabled to exercise control and protect their interests through indirect means (e.g., pressure groups, control of media, and the like) and through occasional direct involvement in politics, as in times of depression and war. I had nagging doubts over the adequacy of these observations about the relation of wealth to politics in American democracy, in view of the spread of psychoanalytic ideas and theories into the social sciences in the 1930s and 1940s.

Two of the great European powers seem to have gone berserk in the 1920s and 1930s. They were mobilized and in lockstep following violently nationalist, charismatic leaders and resolved to overturn the balance of power if necessary by resort to war. These perturbations could not be accounted for by simple economic interest or rational motivations. As the world moved toward war in the 1930s, social scientists sought an explanation for this militarism and hyper-nationalism in Freudian instinct theory, in psycho-anthropological theories of childhood socialization and national character. What came to be known as the psychocultural approach was spreading among social scientists, and in the course offerings of the universities. My doctoral dissertation became increasingly obsolete in my thinking, insofar as it seemed to make the assumption that political attitudes were unambiguously derivable from economic self-interest, and that the holding of public office, or the participation in policymaking, by a business elite necessarily meant that this power would be exercised in a conservative direction. The new psychocultural approach assumed that there was an intervening psychological variable that would make the relation between economic self-interest and political policy consequences more complex than posited by the theory of economic interest.

So I spent my leisure hours in Brooklyn and in Washington, D.C., in the next several years researching and writing a new part to my dissertation, one that I called "The Political Attitudes of Wealth." This new part, its theory based primarily on versions of Freudianism emphasized in the writings of Karen Horney, Erich Fromm, Ruth Benedict, Margaret Mead, Erik Erikson, and Harold Lasswell, presented three chapters containing case histories of wealthy conservatives, liberals, and reactionaries, claiming to show how parent/child/family relations affected the political development and attitudes of such figures as Chauncey Depew, Dwight Morrow, Elihu Root, Jay Gould, John D. Rockefeller, Abram Hewitt, Andrew Carnegie, and the like.

In retrospect this revision of my dissertation reflected much hubris, adding an additional venturesome component to what was already a venturesome dissertation. In 1944, when I submitted my revised dissertation to the University of Chicago Press, the department was in a shambles. Scorned by Robert Hutchins, the humanist president of the university, Lasswell and Gosnell had departed. Merriam had retired; there was no effective department chair. When I talked to Merriam about the submission of the dissertation in its revised form, he seemed to be in a deep depression. He only said one thing—that I should remove the new part on the political attitudes of wealth. The press soon informed me that they were not prepared to publish the book. Since I had invested a substantial amount of pride in this addition to my dissertation, I went away from this experience in a mood of anger and defiance. The war was going a bit badly at the time, and I had an invitation to go overseas as a civilian employee of the U.S. Air Force. So I sent the disputed part of my dissertation to the *Journal of Politics* and went off to the war. Thus it turned out that the only part of my doctoral dissertation that got published was the disputed part—"The Political Attitudes of Wealth"—in the August 1945 issue of that journal.

The dissertation sat in manuscript in Harper Memorial Library from 1938 on, where it was occasionally examined by curious students, who spread the notion that failure to publish *Plutocracy and Politics in New York City* demonstrated how capitalism suppressed scholarship. Charles Merriam—the leading funder of social science scholarship at the University of Chicago—was accused of suppressing a major study on the power of economic interests in the United States in order to protect the Rockefeller family, who were major benefactors of the social sciences and of political science.

The difficulty with this explanation of the fate of my doctoral dissertation is that it flies in the face of the evidence. The part of the dissertation that Merriam wanted to have removed was the psychological part, the part that was least Marxist, or most critical of Marxist materialism. The original dissertation of 1938 only considered the material–political power case. Other motivations were not considered. I have been convinced for some time that Merriam acted on grounds of quality control. The new chapters that I submitted were based upon a pretty thin collection of biographical data, perhaps justifying an article in a journal, but not a serious work of scholarship put out by a university press.

To return to the earlier narrative, I got my first teaching job as an instructor at Brooklyn College, and in my first three years in that junior rank I taught American government in its legal-institutional version some thirty times, five times a semester for six semesters. Coming on top of my University of Chicago social science training, this gave me a balance of institutional and behavioral sensitivity early in my career, such that I could never understand the "institutions old and new, lost and regained" polemic of the 1980s and 1990s. What had the "new institutionalists" really added to the lively Merriam "institution-behavior" discussion in his presidential address of 1925, "Progress in Political Research" (1970)?

As I went from class to class teaching American government at Brooklyn College from 1939 to 1941, the sounds of the Asian and European battlefields kept coming nearer and nearer, until they finally broke through on December 7, 1941, with FDR's "Day of Infamy." The United States declared war on December 8. I was in Washington employed by the Office of Facts and Figures, later to be renamed the Office of War Information, in February 1942. I was put in charge of a small unit assigned to gather information about the enemy—Germany, Italy, and Occupied Europe. I had a small but distinguished staff including Herbert Marcuse, who then was a relatively harmless Hegelian Marxist; Henry Ehrmann, who later became a leading authority on French politics; and a couple other European specialists. Beginning with a knowledge of German, I began to think of myself as a European specialist, and as a comparativist, during these middle years of the war.

This opportunity to experience World War II as a form of postdoctoral training was greatly enhanced when I was hired by the air force to participate in the work of the U.S. Strategic Bombing Survey (USSBS), an agency established by the air force to study the effects of strategic bombing on the German war effort. The idea was to learn from our European experience and apply it to the strategic bombing plan for Japan. The Morale Division of the USSBS, led by social psychologists Rensis Likert and Angus Campbell (of later University of Michigan fame), was then experimenting with survey research. For the first time, they were using probability sampling technique. Their task was to do a survey of a probability sample of the German population in the immediate aftermath of the war, to ascertain what effect strategic bombing had had on German attitudes and behavior. Supplementing the survey of German attitudes, I was given the special assignment of hunting up documents dealing with the air war, and of interrogating police and Gestapo officials regarding problems of internal order in the last years of the war as the air bombardment intensified. My team spent fifteen weeks "in the field"—from April until July 1945—interviewing former Gestapo, SS, and police officials in British and U.S. internment camps, and searching in SS, police, and party headquarters for documents that might have relevant information.

My team did indeed turn up a remarkable German document with details of the impact of British and U.S. air raids from their beginning until mid-1944, in the form of half a dozen large notebooks containing the day-by-day telegraphic ticker tape record sent from the regional offices of the Nazi Propaganda Ministry to the Berlin headquarters. As recounted in Chapter 5, this document had been evacuated along with the contents of the ministry library (including the librarian and his family) as the fall of Berlin became imminent, to a rural inn not far from the Elbe-Mulde river crossing, which demarcated the U.S.-USSR military boundary. The inn bore the sign "Gasthaus zum Goldenen Fass" (Inn of the Golden Barrel).

My team was located in Leipzig, not far from the U.S.-USSR temporary boundary, where we spent the first two weeks after VE (Victory in Europe) Day digging in smoldering heaps of documentary records and interrogating police officials and individuals who identified themselves as members of anti-Nazi movements. Something like a German "underground" surfaced in the last months, weeks, and days of the war, and in a number of cases contributed to peaceful surrender of such towns as Halle, where we did some investigating from our Leipzig headquarters.

In these early post-VE days there was a stream of refugees coming out of the Soviet zone, mostly "slave laborers" freed by the Russians and walking homeward, living off the countryside and the handouts of friendly mess sergeants. One of these, a Belgian, learning that our team was "air force intelligence," stopped by our quarters and told us the strange tale of an evacuated Goebbels ministry library, whose librarian, together with his family, had taken shelter at the Inn of the Golden Barrel. The Belgian had spent the night there and the librarian had confided in him and shown him some of the library's treasures. The librarian was hoping to escape Soviet capture. At the moment, this area where the U.S. and USSR troops had made their Elbe meeting had lost its military quality. Americans and Soviets were fraternizing, exchanging drinks and toasts, visiting each other's barracks, and sightseeing on both sides of the boundary. The Saxon fields through which we passed in our jeep and weapons carrier on our way to the Inn of the Golden Barrel the next morning were littered with the prone bodies of Soviet troops sleeping off hangovers.

The Belgian had told us of the Propaganda Ministry's air war intelligence cache, and we had quickly organized a visit and inspection. The Soviet commander, intrigued by the sightseeing Americans and proud of being able to offer us Lucky Strikes in exchange for the Camels we offered him, waved us across the river, and we soon found the Golden Barrel on the outskirts of Torgau. The contents of the Goebbels library consisted of a variety of files and records, cameras and photocopying equipment, a graphic art collection, and other art objects. We decided to take only the air war record, on the theory that if the Soviets discovered what we were up to we could justify the records as mission-related, while anything else would look like espionage compounded by looting.

As we sat there in the inn with our fabulous intelligence document, and surrounded by valuable art and hardware, fantasies of prison cells and firing squads passed through our imaginations. Actually we got safely home through a simple ruse. Still playing innocent tourists when we got back to the river, we begged the Soviet officer in charge to give us some Soviet boundary flags for souvenirs. He waved us through in embarrassed haste, and did not take the trouble to look into the weapons carrier, where the notebooks were lying hidden under a tarpaulin.

We lost no time in getting the Propaganda Ministry's air raid record to our local G2 headquarters, where we sat down and laboriously made a copy of the record, using a blueprint machine, if I remember correctly. It took several hours to copy the several-hundred-page document. We wanted the copy for our own analysis; the original we felt bound to send to central air force intelligence at Supreme Headquarters Allied Expeditionary Forces (SHAEF) in Frankfurt. We had a tight schedule of cities to cover, so we left our copy at our own headquarters, where we expected to retrieve it and analyze it on our return from the field.

In fact, when we returned to our headquarters at Bad Nauheim in mid-July, expecting to find our documentary materials, including the air raid record, available for analysis, we were told that the document had been cut up for content analysis and simply didn't exist anymore. For our social psychologists, who were making the survey study of the recollections of the German population about how the air raids had impacted morale and working effectiveness, the Propaganda Ministry's air raid log and local reports over the four-year period were simply examined for any references to morale in the aftermath of bombing. These were coded and treated like the responses of the later interviewees.

I reacted to this with shock. It was my first experience with the "mechanization" of social science research. Though I had an appreciation of survey research and later directed a major cross-national survey of political attitudes myself, I retained great respect for the historical perspective, for in-depth "case studies," and for "clinical" studies as means of formulating hypotheses. What we had in the Propaganda Ministry's record of air raids, day by day, night by night, city by city over four years of the war—from 1940 to 1944—with descriptions of types of bomb, areas of destruction, extents of destruction, popular reactions, and so forth, was a detailed account of the impact of the air war as it occurred and where it occurred. This should have been used in the analysis of the later survey; and should