This essay is devoted to tracing the evolution of the systems concept in David Easton’s work, from the point of his undergraduate education to his first explicit statement of the elements of the political system as a conceptual framework for the study of politics. To give an account of the development of Easton’s thought during these twenty years is in many respects to trace the central transformation in the twentieth century evolution of North American political science. Heinz Eulau once remarked that the Political System was in effect the autobiography of a generation, and the confrontation with the book became part of the autobiography of another generation -- to which I belonged.

**Reading the Political System**

I first encountered the book in the Fall of 1959, when I was a relatively new graduate student in political science at the University of California at Berkeley and specializing in political theory. The theory program, led by Sheldon Wolin, was at that point much enmeshed in the controversy about behavioralism both at Berkeley and in the discipline as a whole. I read the book in the context of a seminar that was devoted to analyzing critically the image of science that was prominently associated with the emerging behavioral revolution in political science. The instructor, Norman Jacobson, wished to demonstrate, by an examination of the ideas and practices of working scientists, that the image by which social scientists were possessed was far removed from direct encounters with the actual history and current practices of natural science.
Jacobson had recently attended a conference in which Easton participated, and he suggested to the class that this book, by a young political theorist at Chicago, was an interesting critical evaluation of the field. The book seemed to me in many respects to accord with Jacobson’s own worries about political theory degenerating into a debate between moralism and scientism. I doubt, however, that Jacobson realized at this point that Easton would soon be identified as the principal exponent of the account of natural science and its application to social science that Jacobson had been subjecting to scrutiny. I had attended a liberal arts college that, apart from a course in the history of political theory, had left me quite innocent of knowledge of the field of political science, so I had little basis on which to judge the book. My main impression was simply that it was a critique of the general character of American political science and a call for a more theoretical and integrative approach to the study of politics. But, as it turned out, the profession as a whole, including Easton’s colleagues at Chicago, initially had a dim sense of the direction in which the book was leading.

My next direct encounter with the book was in 1965, a year after I had completed a Ph.D. dissertation focusing on Plato, but this was also at the height of the popularity of the systems approach to the study of politics and its identification with the behavioral movement and particularly with the study of comparative politics. Because of my dissertation work on “political philosophy and time,” I was invited to participate in a six-week symposium at Berkeley, which dealt with conceptions of temporality involved in the study of developing countries. The symposium took place at the very end of the tumultuous student revolt and “free speech movement” on the Berkeley campus, which

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had further divided the political science department along both ideological and
methodological lines. In the course of investigating perceptions of time and history in
various cultural and political settings and considering how the concept of time was
involved in approaches to the study of politics, I became concerned about the manner in
which the concepts derived from frameworks such as systems analysis tended to
become reified when applied to diverse cultural and political contexts and therefore
obscure the beliefs informing indigenous forms of social action. By this point, Easton,
who in that year published his principal books on system analysis, had become the
main spokesperson for the behavioral movement and the leading articulator of the
commitment, in his words, “to emulating the methodology of the natural
sciences.” It is
tempting, and to some extent quite correct, to read, retrospectively, what he defined as
the “tenets” of the behavioral program back into the text of the *Political System*, but, as I
will attempt to demonstrate, in doing so, some of the original character of the work and
its context is lost.

The third phase of my encounter with the book was in the late 1960s. By 1968, I
had concluded that the very idea of conceptual frameworks as an approach to political
analysis was linked to flawed secondary and tertiary accounts of an increasingly
dubious philosophy of science that had become both the basis of what Easton had
referred to as the “credo” of behavioralism and its methodological guide. This critique of
the behavioral persuasion, and particularly the manner in which Easton’s work
exemplified the behavioral image of science, represented a significant part of my work
through the early 1980s. By the late 1970s, however, I had begun to turn my critical attention back toward my own subfield of political theory and particularly to its absorption with synoptic historical accounts of the “great tradition” from Plato to NATO. This led me to engage arguments such as that of Leo Strauss about the contemporary “decline of political theory,” but the criticisms contained in Easton’s 1951 version of that “decline” and its origins in the hegemony of the study of the history of political theory, which became a central element of the *Political System*, began to resonate. While the book had become retrospectively treated, and in many respects correctly, as adumbrating Easton’s work of the 1960s, a central emphasis had been on the need for a revival of creative and politically relevant value theory instead of simply retailing past ideas. But it also seemed apparent to me that there was something odd about Easton’s ascriptions of “historicism” to individuals ranging from William Dunning to George Sabine. Their work did not actually resemble very closely the portrait that Easton had drawn, and this suggested to me that they had been to some extent, either purposefully or mistakenly, surrogate objects of criticism. I now think that it is reasonable to assume that Easton’s principal concern in this respect was really his experience as a graduate student at Harvard and the context that he encountered when he first arrived at the University of Chicago. In both instances, the study of the history of political theory represented methodologically and ideologically conservative perspectives.


It was at this point that I was asked to write the chapter on the subfield of political theory for the first edition of *Political Science: The State of the Discipline*. I decided that rather than producing another typical potted account of what was going on in the current literature, I would attempt to write a short history of academic political theory as it had evolved in the context of American political science. This project was the unanticipated beginning of what became a long involvement in the study of the history of political and social science, but it was also the beginning of a new understanding of Easton’s work.

**Recovering the Genesis of the Concept**

In 1985, I was invited to participate in a conference in Finland on the history of political science, which was jointly sponsored by the Finnish Political Science Association and the International Political Science Association. This was on the eve of the publication of a book of mine that was devoted to a comprehensive account of the condition of academic political theory, in which I critically engaged the work of contemporary historical, behavioral, and normative political theorists. In this work, I reprised my criticisms of the philosophical grounds of the behavioral image of science and with special emphasis on the work of Easton. At this point, Easton and I had never met, and it was with considerable trepidation that I learned that he was attending the conference and that we would be participating in the same discussion group. Given the background of my past criticisms, I was indeed nervous, but the saving grace was that


Easton had a long-standing commitment to avoid reading and responding to criticisms. While I knew a great deal about his work, he knew virtually nothing of mine apart from the chapter on the history of the field. To condense what could be a rather long story, it is sufficient to say that the formidable image of Easton that I had conjured up was dissolved when I met the actual gentle, friendly, self-effacing, and intellectually tolerant person. During the conference, my wife, Dede, and I bonded with David and his wife, Sylvia, and during the following years, we became close friends.

At the conference, David initiated a plan for the comparative study of the development of political science under the auspices of the IPSA, and, for the next decade, I worked closely with him on this project, which focused on differences and similarities in the discipline as it took root and developed in different countries, with special emphasis on its relationship to the theory and practice of democratic politics. When the American Political Science Association asked to do Easton’s oral history, he agreed on the condition that I would conduct the interviews. We met in several long sessions at various times during 1988 and 1989 and produced the most extensive oral history in the APSA archives, which in many respects was an aid in charting a path through my later detailed history of academic political theory in the United States as well

as in writing a subsequent book on the concept of democracy in the history of American political science. But the oral history was also a central source of information for this essay. So I now confront the Political System once again in an attempt to situate it in the history of the development of Easton's systems concept.

Easton, who was born in 1917, began his higher education in political science in 1935 at the University of Toronto during the height of the great depression. His concerns about contemporary political issues soon led him to become dissatisfied with the institutional and jurisprudential approach of the curriculum. Although he attempted to switch to sociology at the end of his sophomore year, university rules did not allow the change. His original emphasis was in pre-law, because he viewed himself as preparing for an active political life, and Marxism and other social ideas permeated much of the intellectual atmosphere. Both he and his future wife, Sylvia, belonged to the Cooperative Commonwealth of Federation, which was Socialist in orientation and much like the left-wing of the Labour Party in Great Britain, and it was opposed to both Conservatism and Stalinism. He graduated in 1939, and considered continuing his education in the United States, which he believed was both more academically innovative and ideologically active than the typical preferences of his generation for Oxford and Cambridge. He applied to, and received offers from, among other institutions, Harvard, Chicago, and Columbia, but he was most impressed with the Chicago program. He accepted a fellowship, but at that point Canada had entered World War II, and emigration was suspended. Because a hearing impediment

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disqualified him for military service, he took a position with the Canadian War Time Prices and Trade Board but also worked part-time as a store clerk while, in 1941, beginning studies for his Master’s degree in political science with an emphasis on political economy. His practical concerns with social transformation in both domestic and international politics as well as his primary intellectual influences fostered, from the beginning, a general holistic theoretical approach to the study of politics.

Easton’s thesis was on the Beveridge report, which initiated social welfare legislation in England, and his focus was on the economic basis of social reform. Sylvia had been in a tuberculosis sanitarium from 1938 to 1941, but they were married in 1942 while David was completing his degree. His teachers included C.B. Macpherson who was famously a critic of capitalism and classic liberalism, but one of his greatest early influences was Harold D. Innis who was an economic historian and a pioneer in communications theory. Innis had received his PhD. from Chicago where he had studied with the sociologists George Herbert Mead and Robert E. Park, who had stressed how communication created and maintained cultures and institutions, and while at Chicago, he had been exposed to the latent influence of thinkers such as Thorstein Veblen and John Dewey. Innis returned to Canada after writing a dissertation on the Canadian Pacific Railway, and his subsequent studies led him to believe that the West was facing a major intellectual and political crisis, which, among other areas, was manifest in commercial pressures on academic thought and in a general preoccupation with the present that obliterated a systemic sense of the relationship between both past and future and space and time. Innis’s approach to economic and political analysis as well as his focus on communication significantly influenced his younger colleague,
Marshall McLuhan and his formulation of the image of the medium as the message, but it also played a large role in forming Easton's perspective on social inquiry.

By this point, Easton had definitely decided that he did not want to continue working for the government and that an academic study of political science held more promise for influencing public policy than the study of law. After completing his Masters in 1942 at age 26, and receiving an exit permit from Canada, he reapplied to Harvard and Chicago. Although still primarily attracted to Chicago, he accepted a more lucrative offer of a Harvard fellowship and came to the United States, but forever would retain his Canadian citizenship. From the very beginning, however, Harvard failed to live up to his hopes for an education in politics and social science. Because he chose political theory as his major field, he somewhat reluctantly accepted the intellectually and politically conservative William Yandell Elliott as his adviser. He was drawn in some respects to the other most dominant person in the department, the German émigré Carl Friedrich, who advanced a general view of politics in terms of the concept of power, but other members of the department such as Samuel Beer, Herman Finer, Charles McIlwain, and Benjamin Wright seemed to offer little that related to Easton’s vision of what should be involved in studying politics from a scientific, interdisciplinary, and socially engaged perspective. Elliott, from the early 1920s, had been was a consistent critic of empiricism in political science and of the often associated group theory of politics, and he continued to be vocal in his criticisms of the theory and ideology of the Chicago program and figures such as Charles Merriam, Harold Lasswell, and G.E.G. Catlin, which had

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8 See Gunnell, John G. 2005. “Political Science on the Cusp: Recovering a Discipline’s Past,” American Political Science Review, 97: for a full discussion of Elliott’s work as well as Catlin’s opposed views which closely linked to Merriam, Lasswell, and the Chicago school as a whole and important in the evolution of Easton’s work.
attracted Easton to the United States and which continued to intrigue him. The very things that Elliott criticized, such as the focus on science and psychology, were what Easton wished to explore. Friedrich, however, was more analytical and theoretical and quite closely associated with Talcott Parsons in sociology with whom Easton managed some direct contact as well as a vicarious connection through a student who was studying with Parsons. Discussions with sociologists led to familiarity with the work of individuals such as Max Weber and Wilfrid Pareto, and he had indirect contacts with people in psychology such as Gordon Allport. He also sat in on Carl Schumpeter’s classes in economics and continued to explore a variety of areas that were not formally part of the political science curriculum.

One author that particularly gained his interest was the late Walter B. Cannon, the Harvard physiologist, who had written *The Wisdom of the Body* (1932), which popularized and expanded on Claude Bernard’s concept of homeostasis as the basic principle of the human body’s system of self-governance. Easton found little of intrinsic interest within the department, and in part because Elliott was often away in Washington DC during the war years, he continued to seek out people from other geographical areas such as Latin America and from other disciplines. Members of Easton’s Harvard cohort such as William Riker, Robert Lane, John Walhke, and Harry Eckstein had similar dissatisfactions with the department and, like Easton, would go on to become founders of the behavioral revolution, each in the end and in their own way making a stark choice between political theory as conceived at Harvard and a new sense of theory as the core of an empirical science of politics. There was also a paternal and socially exclusive atmosphere that ran through all levels of the University and from
which another of the group, Louis Hartz, particularly suffered as a student and later as a faculty member. Easton fastened on to the study of elites, and more specifically the elitist strand in British political thought, as a Master’s and dissertation topic. This was in part inspired by curiosity about Elliott’s preoccupation with the critique of authoritarian regimes as well as Elliott’s criticism of what he claimed was the proto-fascism implicit in American pragmatism and progressivism and in the work of Lasswell and the Chicago program. Pendelton Herring was also attached to the Harvard department and engaged in his early study of pluralist politics, which seemed to offer a counterpoint to elitist accounts of politics such as those of Mosca and Michels, but for Easton, neither elite theory nor group theory and their respective conceptions of power seemed to be adequate as a comprehensive account of political life.

He finished his degree in 1947, and considered an inquiry from the CIA as well as offers from the University of Texas and Redlands University in California, but he did not find any of these possibilities attractive. In the meanwhile, Finer, for whom he had been a teaching assistant, had joined the Chicago department and alerted Easton to an opening in the general education program, which carried with it a minor appointment in political science. Elliott, who often seemed more interested in the prospects of Karl Deutsch (at MIT) than in the future of some of his own students, wrote Easton a letter of support for the position at Chicago but in the same letter suggested that the department might instead want to consider a faculty member at Louisiana State named Eric Voegelin. The possibility that Chicago might have chosen Voegelin makes one wonder how different the next half century in the history of American political science might have been. Easton was interviewed at Chicago by Daniel Bell in sociology, Herman
Pritchett in political science, and Saul Tax and Milton Singer in anthropology, and although they were not particularly impressed by his dissertation, he was offered the position, which he accepted with a salary of $5000 after negotiating, with the support of Finer, Leonard White, and Avery Leiserson, a half-time appointment in political science, which was later extended to a full appointment in the graduate division.

After the constraints of Harvard, Easton found Chicago an exciting and vibrant intellectual world with a great deal of emphasis on interdisciplinary work as well as on issues of methodology and the philosophy of science, which provided a basis for self-reflection regarding his own intellectual path. Because he was attached to the graduate program and dealt only with graduate students, there was significant time for research and writing. The theoretical and empirical core of the Chicago school had, however, been diminished in the course of President Robert Maynard Hutchins’ reformation, which turned the university in the direction of the humanities as well as embracing a conservative ideological orientation. Hutchins did, however, sponsor interdisciplinary work, often supported by the Ford Foundation, but much of the effort was under the control of individuals such as Mortimer Adler, Richard McCeon, and those attached to the newly created Committee on Social Thought chaired by John Nef, which had close ties to the theologically oriented Review of Politics and conservative émigré scholars. There was a distinct turn away from the social sciences as well as from the progressive ideology that had been associated with Merriam and his colleagues. Lasswell and Harold Gosnell had not returned to Chicago after their war service, and Merriam’s capacities and participation were in decline. Although Hans Morgenthau had arrived a year earlier and although Quincy Wright was working on a general theory of
international relations, the only other designated political theorist besides Easton in the department, before the arrival of Leo Strauss in 1948, was Jerry Kerwin, a Thomist who had no positive interest in social science.

Easton perceived two distinct trends in the field of political science at Chicago, and these would be prominent in his thoughts about the future of the discipline: data collection and survey research, or what he would later refer to as “hyperfactualism,” and a latent concern with more general theory, which reflected the residue of Merriam’s reign. White, the department head, was still devoted to linking the study of politics to the defense and development of democratic values, and there remained the sense that the social sciences were methodologically a unity, but it would fall to Easton to resurrect the original vision of the Chicago school. When the famous social science building had been constructed, the cornerstone had originally read “social sciences,” but Merriam insisted that it be changed to “social science” in order to reflect the interdisciplinary commitment. There was, however, by this point, a distinct lack of any core curriculum, and Easton experienced his “baptism” in methodology and interdisciplinary work as part of a group devoted to creating a new course on the “Scope and Method of Social Science” that would serve to put “Humpty-Dumpty” together again and remedy the condition of what, many years later, he referred to as “divided knowledge.”

In these early years at Chicago, there were several important influences on Easton. He participated in the Cowles Commission, which was devoted to Keynesian economic research with an emphasis on statistics and mathematics. Milton Friedman had not come into prominence and Frederich Hayek had not yet arrived. Easton became a close friend of Kenneth Arrow (who was the eventual mentor of Anthony
Downs), and he participated in the progressively oriented Committee on Planning which was eventually disbanded because of the pressures of McCarthyism and worries about association with socialist ideas. He joined a group in anthropology that focused on the subject of culture and personality, and he had social and academic ties to sociology and individuals such as Park, William Foote Whyte, W.Y. Thomas, William Ogburn, and Florian Znaniecki. The Ford Foundation sponsored a series of seminars for senior faculty for which Easton served as the rapporteur, and he sat in on some courses in calculus.

There was no question, however, that Easton’s early principal concern was with values and whether they could be treated scientifically, as the pragmatists believed, and in the manner that was reflected in Dewey’s contribution to the *International Encyclopedia of Unified Sciences*. This short-lived publication was edited at Chicago and functioned as an organ for the transplanted logical positivists from the Vienna Circle and their pragmatist allies such as Charles Morris. Rudolf Carnap from Vienna and Morris, along with Otto Neurath (in Europe), edited the *IEUS*, and Charner Perry, in philosophy, was a principal spokesman for the persuasion and its adoption as a vision of science in fields outside philosophy. Up to this point, a commitment to science and the scientific method had been quite uncritically accepted by American political scientists but very porously articulated in the field as well as in the literature of philosophy. Apart from Elliott, few had significantly challenged this commitment, but it involved little more than the use of statistics and quantification, data collection, and a general pragmatic empiricist attitude. The philosophy of science had not really emerged as a distinct field of study, and what was available amounted to little more than a
general image of science as un-dogmatic inquiry, which coincided with the values of liberal democracy. At this point, however, logical positivism was on the eve of gaining ascendancy in philosophy, and by 1950, its doctrinal influence was evident in Lasswell and Abraham Kaplan’s, *Power and Society* as well as in a more general attitude that was becoming prevalent among certain social scientists who sought a more coherent vision of science. Kaplan had been a student of Carnap, and what characterized the book was an attempt to draw upon positivism as an account of pure science that, while cognitively authoritative and divorced from practical goals, would nevertheless provide a footing for the future application of knowledge. This assumption would be a consistent element of Easton’s vision from the point of the *Political System* to his Presidential address to the APSA in 1969.

The basic character of the positivist account of the logic and epistemology of science and the unity of scientific method was clearly the image of science to which Easton came to subscribe, but he was also concerned with finding a rapprochement between fact and value, despite what he took to be their logical difference. The spirit of scientism that informed positivism and what its philosophical proponents from Vienna referred to as, the “scientific vision of the world,” had been the spearhead of a movement for left liberal reform in Europe. In many respects, this ideology corresponded closely to the character of the Chicago school, but in the American

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10 For a discussion of the emigration of positivism to the United States and its relationship to political theory and the philosophy of science, see Gunnell 1975: 1993 and 2009 “Ideology and the Philosophy of Science: An American Misunderstanding,” *Journal of Political Ideologies* 14:
context, the value orientation of positivism receded and became obscured – in part because of the Cold War atmosphere but also because those values were in many respects not far removed from those of American liberalism. Despite the positivist insistence that values lacked determinate meaning and that there was a logical distinction between fact and value, they did not deny either an important relationship between fact and value or the assumption that science itself was a social value.

Nascent positivism, however, conflicted with the view of the new humanists at Chicago and their emphasis on metaphysics, religion, and the great books, and positivism was also at odds with ideas of Leo Strauss, who had been hired by Hutchins without department consultation. Strauss, however, soon became quite independent of both political science and philosophy at Chicago. Despite the common identification of Strauss with natural law, he did not really agree with the Thomist version that was widely embraced at Chicago. Kerwin, however, was the chairman of the Walgren lecture series that supported the more general theological approach of those in philosophy such as Yves Simon but which would also, in the early 1950s, sponsor the work of Voegelin and Strauss. There were, in principle, distinct ideological and philosophical tensions between Easton and Strauss, but Easton attempted to keep them from coming into focus. Although the philosophy department was fractured, logical positivism and pragmatism continued, among both supporters and critics, to largely define the vision of science that informed intellectual life at Chicago. It was the basis of what Easton would later refer to as “pure science.”
Easton’s first major publication was an article in 1949 on Walter Bagehot, which was derived from his dissertation.\textsuperscript{11} A principal focus of the article was the worry among both left and right intellectuals about the fragility of the theory and practice of liberalism. Easton’s specific concern was with the manner in which liberalism, in its more conservative formulation, had ultimately failed to deal, in principle and practice, with economic inequality and the deficiencies in the material conditions of society, but he was also concerned with its failure to test its theories and values against social reality. In the work of Bagehot, as well as that of individuals such as Mosca and Pareto, he saw the possibility of a realist positivist account of social facts and value-testing as well as the need for creative elite leadership, and this view continued to inform his ground-breaking essay on the “Decline of Modern Political Theory.” \textsuperscript{12}

The context of the article was a symposium on “The Relation of Political Theory to Research” which grew out of a 1950 panel of the Midwest Political Science Association and which was devoted to exploring, as stated in the “Preface” by Alfred de Grazia, how political theory might “adjust” and contribute to research methodology rather than focusing on “utopian goodness.” Easton claimed that in the field of academic political theory the dominance of historical commentary on the work of past thinkers had resulted in the contemporary “poverty of political theory” and a failure to produce both creative and relevant value theory as well as “systematic theory about political behavior and the operation of political institutions,” and particularly in a failure to bring these forms of theory together. What he referred to as “historicism” had also led to “a relativistic attitude toward values” which may have suited the complacency of the

late nineteenth and early twentieth century but which poorly served the contemporary period. His use of the word “historicism” was adapted from Strauss, but Easton at this point did not perceive its role in the wider scope of Strauss’s work which included Strauss’s attack on a distinction between fact and value. Easton, however, accepted a definite logical difference between facts and values, but he stressed the need to bring them into conjunction and to realize both that values informed research and that research yielded knowledge about values and their application to public policy. He did not deny the relativity of values as emotive responses to life experience, but this, he argued, did not entail the irrelevance of values as solutions to political problems and as guidance for policy-makers. But if political theory was to serve political action, it was necessary for the reconstitution of value theory to be joined to systematic empirical political theory that would provide general scientific knowledge of political facts. While the older Chicago school had contributed to this goal, much more was needed to move beyond “crude empiricism” and achieve a causal “broad-gauge theory or the conceptual framework within which a whole discipline is cast” and which might “reach the stage of maturity of theory in physics.” He noted that Parsons did not subscribe to the possibility of a distinctly political general theory, but such a theory was Easton’s goal, and the search for an intrinsically political theory would figure very largely in his rejection of political analysis as just a subset of general systems theory. He had given Strauss a draft of the essay, and Strauss had written a number of marginal notes. They certainly both agreed on the fact of “decline” in political theory, a term that Strauss would later appropriate and in which he would implicate the very image of science that Easton championed, but although the realization had not quite come into focus, Strauss’s work
was closely related to the kind of historical commentary that Easton associated with the poverty of the field.

In the same year, Easton also published an article on the work of Lasswell.\textsuperscript{13} He referred obliquely to the Chicago ambience when he noted the “attack” on science that had been mounted by some philosophers as well as the skepticism of many scholars regarding the possibilities of a science of man. Voegelin had given the Walgren lectures the same year, which were published as his \textit{New Science of Politics} (1952), and in the same series, Strauss gave the lectures what would become \textit{Natural Right and History}, which was published in the same year as Easton’s \textit{Political System}. Liberalism, social science, and scientism were all under siege in these lectures and by major elements of the Chicago faculty, and one of the major issues was whether there was any fundamental basis of values or whether relativism had become the major perspective of the age. Easton concluded that while Lasswell had originally appeared to eschew value judgments, he had come to the conclusion that, as a policy science, political science was devoted to a defense of democracy and that this could be achieved by providing democratic values with a scientific foundation and thereby making political science matter politically.

During these years, however, Easton was involved in what would be his most defining experience in formulating an image of systematic political theory. After the war, James Grier Miller, who had been trained at Harvard, became Assistant Professor of Clinical Psychology in the new Department of Social Relations at Harvard, which was chaired by Parsons. In January, 1948, six months after Easton arrived at Chicago, Miller became chairman of the Department of Psychology at the University of Chicago.

\textsuperscript{13} Easton, David. 1951. “Harold Lasswell: Policy Scientist for a Democratic Society.” \textit{Journal of Politics} 13:
In 1949 Easton joined the Innominate Club, a faculty research group, of which the nuclear physicist Enrico Fermi was also a member. Fermi urged Miller to work with other professors at the University to develop an integration of biological and social scientific knowledge that would lead to an understanding of human behavior similar to the manner in which general theories operated in physical science. Such increased understanding, he hoped, would prevent human beings from destroying themselves by nuclear warfare. Fermi’s suggestion echoed that of one of Miller’s mentors at Harvard, Alfred North Whitehead. Miller was somewhat skeptical that the biological and social sciences were ready for such an integration, but Fermi requested Hutchins to provide Miller with funds to finance a group of professors in such an effort. Miller, with the assistance of Dean Ralph W. Tyler of the Social Science Division and Dean Lowell T. Coggeshall of Biological Sciences Division, selected an interdisciplinary group of senior professors who agreed that they would participate in such a project. Miller chaired the group and invented the label “behavioral science” and requested that Hutchins name the interdisciplinary study group The Committee on the Behavioral Sciences. It was from this source that the term “behavioralism” first entered the language of political science. The Committee met for planning sessions and preliminary conversations from 1950 to 1952, and in May 1951, Miller circulated copies of a manuscript entitled “The Behavioral Sciences,” which dealt with the relation between science and the humanities and particularly with scientific method in the behavioral sciences and posited a set of basic principles that were deemed appropriate for the biological and social behavioral sciences. In October, 1952, a Theory Group was organized and began regular meetings. The initial participants were Miller, Donald T. Campbell, David Easton,
Donald W. Fiske, Ward C. Halstead, Henrietta Herbolsheimer, Roger W. Sperry, and Sherwood L. Washburn. These theoretical discussions continued on a weekly basis until 1955.

Easton continued to give papers on subjects that related to his 1951 publications, but the department was somewhat concerned that he did not seem to be doing what had been generally considered to be political theory. At this point he was teaching a course that reflected his emerging views about the field, but he was also in the final stages of formulating a draft of the *Political System*. White read the draft and although he admitted that he did not fully understand it, he believed (correctly) that it tied in with Herbert Simon’s work on administrative beavior. He became very enthusiastic about the manuscript, and contacted the Knopf publishing representative who accepted the manuscript for publication, which led to a favorable decision in 1953 on Easton’s promotion and tenure. Some in the department, such as Finer, said that they did not understand the book, but others such as Strauss all too clearly discerned the implications. Morgenthau claimed in a faculty meeting that the book would undermine all of us. There was, then, limited but grudging support in the department, but organizations such as the Social Science Research Council were committed to transforming the field of political science in a manner that matched Easton’s vision. The *Political System*, however, has now become somewhat a prisoner of the perspectives that have informed its complex interpretive history. After two generations, it is worthwhile attempting to free the book from these interpretations, taking a careful look at the text and the structure of the argument, and determing its place in Easton’s path to the formulation of a systems analysis of political life.
Re-Reading the Book

In the Preface to the *Political System*, Easton claimed that “it has become increasingly difficult to appreciate why political theory should continue to be included as a central part of political science. Theory has become increasingly remote from the mainstream of political research.” Although a bifurcation between political science and political theory had been becoming increasingly evident in the discipline at large, it was nowhere more apparent than at Chicago. Easton defined his task as to “win back for theory its proper and necessary place” (ix). Before the War, there had been no significant tension between the discipline and the subfield of political science, and despite Easton’s characterization of Sabine, it would difficult to distinguish Sabine’s views on values and science from those of Easton. The book was actually instrumental in further driving a wedge between political science and the emerging literature in political theory represented by authors such as Strauss, Voegelin, Hannah Arendt, Herbert Marcuse, and others who were redefining the subfield of political theory. The book marked a crucial point in what, within the next decade, would become largely a contest between the subfield of political theory and the evolving behavioral persuasion, which ultimately led to the officially accepted fracturing of theory into empirical, historical, and normative components. The basic image projected in the book, however, that is, the growing alienation of political theory from political science, was neither at this point self-evident and widely perceived in the field nor persuasively documented in the book.

The first chapter (“Mood and Method”) has surely been the most neglected and the most in need of re-interpretation, but it signalled a change in distribution of

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emphasis from that in the 1951 article. Although Easton repeated much of what he had said in 1951 about the lack of devotion to science, he went further in his diagnosis of the problem and in his attempt to demonstrate, as he indicated in a quote from Charles Beard, that the idea of bringing the phenomena of politics fully within the scope of causal science might be a dream but that it was one that enhanced intellectual imagination and was worth pursuing. Easton claimed that after 2500 years of optimism about the possibility of a science of politics, the current era had, on the whole, yielded only “disappointing results.” This, he argued, must be attributed to the failure of American political science to “teach seriously the standards of valid thinking, observation, and description which today we are prone to associate with something vaguely called the scientific method” and which would demonstrate the “true relationship between facts and political theory” and particularly “the vital role of theory in the partnership” (4). “Systematic theory” was necessary for facts to be “ordered” in a manner that produced “connections” from which empirical “generalizations” could be derived. He noted that his aim at this point was only to “hint” about the construction of such a theory, while his principal purpose was to “probe to the roots” of the growing strength of “dissent against the scientific method” and to demonstrate that despite the continuing practical concerns of the discipline, the result of this failure had been “to imperil its attempt to understand the major problems of political life” (5).

Easton began by asking why political science had never really adopted the “scientific method,” but he went on to claim that the more fundamental source of the problem was not simply disappointment about long-standing unrequited hope for scientific advance but rather that “today we are confronted with a growing
disillusionment about the whole of scientific reasoning as a way of helping us understand social problems” (5). What was wrong with political science, he claimed, “mirrors the mood of the age” – “a mood in Western civilization directed against the use of scientific reason” – which had led to “a movement away from the rational attitude toward life, implicit in the use of scientific method, and towards a greater dependence upon emotion or faith and upon tradition” (6). Although in the nineteenth century and beyond there had been great optimism about the “ability of men to use scientific reason for the common welfare,” this view had begun to decline after the War with the advent of things such as the atomic bomb (15-16). Even in natural science and the philosophy of science, there were doubts about the capacity to provide a determinate account of the facts of the natural world, and in “political philosophy” there was a “ubiquitous” trend toward a loss of faith in social progress in the face of the “humanistic feeling that scientific development, either social or physical, does not always lead to desirable moral results” (17). “Dischantment” with “utilitarian theories of man as an essentially rational creature” had been replaced by a new emphasis on “arousing emotions,” the authority of tradition,” and a “movement back to theology” and to “high spiritual ideals” as well as to “secular emotions” and the assumption that democracy rested on “faith” reinforced by “propaganda” and “traditionalism” as a way to “outbid its competition in appealing to passion.” There was a “rejection of the idea that political order could be based on “rational conformity to social and individual purposes” and that what was required instead was “an order founded on a myth and maintained through the periodic revitalization of this myth.” In despair, people blamed scientific reason and sought to escape from it” (18-21).
There was something odd, if not counterfactual, in the general anti-scientific attitude that Easton attributed to American society. This was, on the whole, an age of enthusiastic support for, and adoption of, technology and of optimism about science as the key to the future. It is difficult to avoid the conclusion that his actual concern was a certain trend of thought in the field of political theory. Easton claimed that the “crisis” was evident in political science where, despite the work of the original Chicago school, most were now “cold and skeptical” when it came to “discussing generalizations about human activity that correspond in universality and durability to the laws of the natural sciences” (23-24). While some believed that such laws were, for a variety of reasons, simply not attainable or that they were only “true for a particular time and place” and while others claimed that although “much of social science,” as Mannheim recognized, was “culture-bound,” it was necessary “to separate the truly universal propositions from the more limited ones” (34). The consequence was that “in political science criticism rather than approval of scientific method and its cognitive objects is almost imperceptibly becoming the criterion in many circles for judging the worth of a study of method” (36). Easton argued that whatever the particular problems that might stand in the way of developing generalizations, the greatest difficulty was a kind of “social pessimism” that emanated from the basic belief that we could not achieve law-like explanations such as those in natural science. This had been principally engendered by “a manifest tendency to turn away from reason to emotion and tradition” and that making this point clear gave “meaning and cogency” to everything in the book (31, 36).

What is most interesting about the argument advanced in this first chapter as a foundation for Easton’s recommendations about how to proceed with a transformation
of political science is what, in retrospect, was the obvious lack of specificity regarding the literature in which this mood was supposedly manifest. Since his characterization did not remotely fit any of the individuals he discussed in the book, the picture that Easton painted of contemporary political science was difficult to locate. His basic claim was in fact largely a reiteration of a position that had been consistently voiced since the creation of the APSA fifty years earlier, and he acknowledged that it had continued to inform the work of individuals such as Merriam, Lasswell, Kaplan, and Simon. The one figure mentioned as a “purveyor of traditionalism and anti-rationalism” was Michael Oakeshott in England (21), but Oakeshott had little to do with either the character of American political science or American intellectual and popular culture. Even Easton’s image of the wider cultural mood was not convincing. Despite the anxieties he pointed out as marking the atomic age and the Cold War and the popularity of philosophies such as Existentialism, the period was one in which, throughout society, there seemed to be great hopes and support for all aspects of science.

The real target of Easton’s criticism, or at least from what his interpretation of the mood of the age was derived, was his residual dissatisfaction with the work of his thesis advisor and others at Harvard and the new font of similar ideas that had sprung from the impact of European émigré scholars and their American fellow-travelers in the field of political theory from the late 1930s through the early post-war years. And nowhere was all this more manifest than in the wake of the Hutchins regime at the University of Chicago. Easton’s reluctance to be specific about the object of his criticism may have been in part strategic, as when in the Preface he acknowledged the “friendly criticism

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and challenging scholarship of Leo Strauss,” but even though at this point Strauss’s true position had not really been clearly articulated and perceived, Easton was reacting to an intellectual climate that was becoming manifest in the political science conferences that he was attending and in the general literature of political theory. Despite the diversity of the émigré scholars, who ranged from the positivists on one side to individuals as philosophically and politically diverse as Strauss and the members of the neo-Marxist Frankfurt school, the latter individuals were united in their attack on liberalism, scientism, relativism, and the idea of historical progress. What had been projected into the American context was a debate between European intellectuals in which Americans now in strange ways attempted to situate themselves without entirely understanding the context but nevertheless trying to relate these ideas to continuing issues in the field of political science. It is very doubtful that Easton had any clear grasp of the arguments of someone such as Strauss, but although Easton judiciously avoided directly confronting Strauss’s arguments in class, Strauss, in his discussions with students, pointedly distinguished his position from that of Easton.

In Chapter Two, Easton lamented the “condition of American political science” which, although, he suggested, contained significant potential and promise, had focused too much on the “accumulation of facts and the premature application of this information to practical solutions” (37); failed to reach the “hard core of political power in society” or what V.O. Key had for the first time defined as the “authoritative allocation of values” (41); not produced an adequate account of political change; and lacked a well-defined terminology. The discipline remained bound to its “hyperfactualist past” (47) because of its primitive methodology, professional inertia, and the “proximity of political research to
the social forces that determine social policy” (50), but above all, the problem was rooted in the “absence of a theoretical orientation” and a “conceptual framework or systematic theory” which, he argued, was at the core of any real science (51-2). Such a “causal theory” would rise above “singular” and “narrow-gauge” or “synthetic” generalizations to a “conceptual framework with which a whole discipline is cast,” which could in turn be tested and adjusted in terms of its ability to account for observed phenomena. It would consist of high-level postulates from which could be derived narrower generalizations that would in turn yield testable propositions in terms of which the whole system could be assessed. Although such a theory might at some point reach the “maturity associated with theory in physics,” it was important not to be premature and embrace a naïve scientism (58-59). He noted that while those such as Parsons had claimed that it was not possible to speak of a “political system” as theoretically autonomous and independent of a general theory of society, he insisted that political science was a distinct area of research that could be analytically specified and that this was essential for coherency in the field.

Just as he was vague about his opponents in the first chapter, a striking feature of this second chapter was that Easton gave no indication of the source of the image of science that he projected with such confidence. This image, however, with its strict distinction between theory and fact, the assumption that theories were instrumental constructs for connecting and ordering facts that were somehow given to immediate experience, and that a mature theory was a deductive system of causal generalizations, were all part of an abbreviated but accurate rendering of the central claims of logical positivism and logical empiricism which by this point had not only given rise to the
philosophy of science as a distinct subfield of philosophy but had gained intellectual hegemony and was particularly prominent at the University of Chicago. This image would continue to inform Easton’s account of theory and science that was the basis of his critique in Chapter Three of past “conceptions of science and theory in empirical research.” He noted that although individuals such as Merriam, Lasswell, Simon, Key, and David Truman had advanced “synthetic theory,” they had stopped short of formulating “a conceptual framework for the whole field” (64-65). This, Easton argued, was because of the heritage of a “depersonalist” image of science. Only Catlin and H.B. Munro had managed to suggest something more comprehensive. But the problem was also rooted in the tendency to seek a “premature policy science” that did not recognize the priority of “pure science” in solving the problem of the relationship between means and ends. To move forward in the theoretical endeavor required confronting a range of problems that were involved in theoretical research, and in the case of political science, it was crucial to vouchsafe the theoretical autonomy of politics.

What Easton took on in Chapter Four was an issue that he claimed had been neglected for a long time, that is, what, exactly, constituted the units and boundaries of politics. This Chapter on the “orientation of political research,” and Chapter Five on a “convenient guide for political inquiry,” were the crux of the book. For the first time in his writing, he claimed that “the idea of a political system proves to be an appropriate and indeed unavoidable starting point.” He suggested that we intuitively know that for the purpose of “analysis and research” we can set politics apart from the “total social system,” because in the “concrete world of reality” there are certain related phenomena that, while part of the whole social system, have a more “determinate relation” to one
another than other phenomena. Because all social phenomena are interdependent, it is ultimately "artificial" to factor out any particular set, but political science, like economics, is "compelled to abstract from the whole social system some variables," and "the mental or analytical tool for this purpose is the theoretical system" (96-99). The question, however, was what to extract and on what basis. Although Easton strongly believed in interdisciplinary research and in the methodological unity of the social sciences, he nevertheless wanted to defend, like so many before him, the theoretical autonomy of political science. This had been a problem for political science ever since it separated from the profession of history and claimed the cognitive authority of science as a way of gaining practical purchase. He suggested that there were pragmatic reasons for differentiation such as the extensive modern accumulation of knowledge about politics, but there was a deeper reason. This was a recognition of functional differences among parts of the social system that dealt with matters of "vital concern" to society. Easton argued that past attempts to find a unifying concept for political science had been inadequate. Much of the history of political science had embraced the concept of the state, but the state was too narrow and only one institution among many. Although individuals such as Merriam, Lasswell, Catlin, and Morgenthau had made an advance by turning to power as the defining variable and "exploring the total configuration of power in relation to values," power was only a means and too broad as a definition of politics. In Chapter Five, Easton presented his "convenient guide for political inquiry" (125), which he claimed was largely a more sophisticated version of our common sense view and derived from our experience of the "empirical or concrete political system." This view was that politics involved making and executing public policy. The concept of
politics advanced by Key as “the authoritative allocation of values” (129) also built upon Lasswell’s discussion of the “distribution and composition of value patterns in society,” but, Easton argued, Lasswell’s mistake was to look at the whole society rather than at what distinguished the political dimension as a functional requisite within the whole social system.

In the next three chapters, Easton focused on how it was necessary to take into account various kinds of “situational data” and how even though various stages in the history of the field had tended to reflect a concern with such data, the field on the whole had been slow in identifying and distinguishing these classes and allocating an appropriate place to each. While an institutional and juristic focus had given way to a form of “realism” that stressed groups as the locus of power, the discipline was still constrained by its conception of the boundaries represented in the legal structure. The emphasis on process in the work of Arthur Bentley and others had added a dynamic dimension of process to political inquiry, and this indicated the need to pay greater attention to a changing variety of “phenomenal” situational elements. This, he claimed, was exemplified by the recent focus on psychology and personality and the introduction of “behavioral” data, which helped to complete a picture of the “total structure of the situation” and to take into consideration not only for the “objective” conditions of political life but “political behavior” and the “subjective” elements represented in attitudes, feelings, and motivations.

Given the contemporary debates in political theory, Easton felt that it was necessary to deal in Chapter Nine with the “moral foundations of theoretical political research.” He claimed that one respect in which values were involved with political
research was that they were themselves part of the data of politics, even though it was doubtful that values could be judged to be either true or false, but his more important point was that although moral judgments and claims were “logically separable from factually oriented knowledge,” “moral premises” conditioned the orientation and conclusions of empirical research. Consequently, what was required was to achieve “moral clarity” regarding these premises and to acknowledge them. In addition, he claimed that although “facts and values are logically heterogeneous,” most claims were in practice a mixture of the two and “no one proposition need express either a pure fact or a pure value.” Although he did not rule out the possibility of a more substantive view of moral claims, he believed that even by tentatively accepting the prevailing positivist view that “values can ultimately be reduced to emotional responses conditioned by the individual’s life-experiences,” it was clear that in many respects fact and value were inextricably linked. It was evident that Easton’s discussion was bound by a Weberian and positivist account of values, but this chapter prepared the way for a reprise in Chapter Ten of his article on the decline of political theory and a reiteration of his point that a recognition of moral relativism did not entail moral indifference. By this point, however, Strauss no longer appeared in the discussion.

Easton’s “critique of a general political theory” in Chapter Eleven was more significant than often recognized. He claimed that what he categorically referred to here as “equilibrium theory” was a nascent or subteranean type of “systematic theory which can be detected in recent empirical research” (266). The concept of “equilibrium as a possible condition of the empirical system” was derived from a general claim about how power groups exercised mutual restraint within a constitutional structure, and it
functioned as both a descriptive and normative proposition. He noted various deficiencies in this formulation, but the principal problems were that of quantifying the claims and the extent to which it did not adequately address the issue of political change. There are several things about this discussion of equilibrium that deserve notice. First of all, even Easton's own account of this idea demonstrated that this “theory” was hardly recent. It encompassed the idea of the balance of power that had dominated the study of international relations for a century and which was still manifest in the work of realists such as his colleague Morgenthau. But more importantly, it was the core of the group theory of politics that had emerged after the turn of the century and had become the meaning of “liberalism” and “democracy” during the 1930s and, through the work of Herring and others, the leading empirical and normative core of American political science. In its most recent articulation, in the work of individuals such as Truman and Dahl, it was in the process of becoming the theory most commonly associated with behavioralism. Second, although Easton implicated the Chicago school in this formulation, it, particularly in its normative sense, had never been the basic theory of democracy embraced by Merriam, Lassewell, and Catlin. But, most importantly, Easton would never subscribe to this account of democracy. His limited intimations of democracy were tied more closely to a progressive holistic image, not unlike that of Dewey in The Public and its Problems (1927). Third, this “theory” was closely linked to what Easton had discussed as the deficiencies of power as an organizing concept for political research, and like power, it bore little resemblance to what Easton had elaborated as the logical features of a conceptual framework and a
systematic theory and what he recommended in Chapter Twelve as the basis for the “rejuvenation of political theory.”

Easton argued that it was necessary to move consciously and explicitly to produce systematic empirical theory that would recapture the unity of political theory that he claimed had been manifest in the great theories of the past and in their attempt to “embrace the whole of political life” (309). Empirical and moral research had not only become divided and at best mutually tolerant but often assumed imperialistic stances devoted to invading and dominating the other. Although it was possible, and necessary, to separate logically factual, evaluative, theoretical, and applied propositions, these were not divisible in practice. The essential task was to produce creative systematic theorizing and to “pursue the logic of a theory for its own sake” without being constrained by “excessive worries of verification” and the operationalization of terminology (315). Easton was certainly setting his future agenda, but he ended by noting that “a preliminary formulation of such a theory is, however, the task for a separate study” (320). Despite this caveat, and although it appeared to some critics that Easton had not really delivered on what was promised in the title of his book, the basic claims had in fact been presented, despite the lack of a detailed model of the political system.

One way to shock us out of the illusion of familiarity many tend to harbor with respect to *The Political System* is to return to the initial reception, which was not perceived through the veil of subsequence assessments of meaning and significance. Although Easton’s basic argument was not entirely clear to his contemporaries, there were also aspects that would be lost to many later readers. The book was, however,
widely and generally positively reviewed, and although most were quite accurate in their accounts of its contents, there were some interesting misunderstandings. William Anderson, who as much as anyone of this period possessed a comprehensive view of the development and current character of political science as well as the subfield of political theory, reviewed the book, as a result of White’s urging, for the *American Political Science Review.*\(^{16}\) His comments were basically favorable, which was not surprising, because Anderson had been among those who in the post-war period who had been vocal about the need to continue moving the discipline in a more scientific direction. He complained, however, that the book was not really about political science as much as the failure of political theorists to contribute to the discipline and that Easton seemed to believe in a golden age of political theory and advocate that political theorists be viewed as an autonomous group. He also noted that the book appeared to be influenced by a gloomy perspective that was characteristic of the time and represented a form of world pessimism that focused on the phenomenon of decline. This attitude emanated, he suggested, from a small group of thinkers who took themselves too seriously and failed to realize that the rest of society was hardly aware of the mental sickness that they believed threatened modernity. It might today seem strange that Anderson actually attributed to Easton some of the very attitudes that Easton was criticizing, but, as I have already noted, Easton had been somewhat circumspect as far as making clear what literature he was confronting and from which he was distancing himself. It is doubtful, however, that he was yet fully aware of the sources of that intellectual climate, even though he sensed that it had some connection to the kinds of ideas that had been espoused by Elliott at Harvard as well as by many at Chicago.

Donald Smithburg, who was more closely attuned than Anderson to the recent movement for scientific reformation in the field, judged the book as “brilliant” and recognized Easton’s divergence from Parsons on the issue of whether there could be a distinctly “political” theory. Oliver Garceau noted that Easton’s characterization of individuals such as Charles Mcllwain as “moral relativists” did not really fit and that maybe he was too harsh in his criticisms of field research, but he recognized that Easton’s critique of the concept of equilibrium was really in part a critique of the dominant group theory of politics. Robert Dahl reviewed the work in tandem with Voegelin’s *New Science of Politics* and perceptively noted that despite the fact that they both criticized the contemporary science of politics, the books represented new fundamental “cleavages” in the discipline. Dahl claimed that the book indicated that a distinct difference between the “political scientist” and the “political theorist” was developing and that each side was promulgating “caricatures” the other. Dahl praised Easton’s critical analysis in which he said that Easton had given “as thorough a drubbing to American political science” as it would be likely to receive for some time, but he claimed that Easton’s preoccupation with the nature of politics and political science indicated a kind of “metaphysical” search for essences that did not square with the wish to emulate the natural sciences. Dahl also said that he did not understand the significance of the “authoritative allocation of values” as a definition of politics. This remark probably reflected Dahl’s awareness that although they were in accord on the ethic of science, the was implicitly a challenge to the group theory of politics. This


reservation about Easton’s definition of politics was repeated in other reviews, which noted that although Easton’s work was “a welcome relief from so much of the moralizing that passes as political theory” and that it urged the creation of systematic theory, the book did not actually present such a theory.

Eulau praised the book highly but found it difficult to identify the anti-scientific “mood” against which Easton claimed to be rebelling, and he could not really discern a basic difference between Easton’s definition of politics and Lasswell’s analysis of power in terms of “who gets what, when, and how.” There were also some reservations about Easton’s treatment of moral theory. The longest review of the book was by Henry Magid who had been a student of Strauss and who presented an extended critique of Easton’s account of values and his emotive account of ethics. It is in some respects possible today to understand the book better than it was understood in the 1950s, and in some respects, it may be possible to understand the author better than he understood himself. One reason why Individuals such as Anderson and Eulau had difficulty recognizing the “mood” to which Easton was responding was in part because that mood had not yet become fully visible in the profession and because they were not at Chicago.

A Science of Politics

It is important at this point to determine exactly what constituted Easton’s vision of theory and science. To some extent it reflected, as he later acknowledged, a


distinctly Weberian and Mannheimian type of perspective in terms of which fact and value and well as theory and fact were viewed as logically homogeneous but linked in various ways and with implications for public policy. Part of this position involved the assumption that values were subjective and relative and always informed empirical research but nevertheless in themselves constituted data and that they, like theoretical claims, were capable in certain respects of being tested against reality. Knowledge was always, as Easton, following Mannheim put it, “relational” but not “relative” in the sense of only true for a particular time and place. For Easton, as for Weber, whatever the value perspective informing research and guiding selection of data, the empirical claims could be assessed in terms of their empirical validity, which was a matter of their usefulness in explaining phenomenal data. There was considerable continuity between certain aspects of this image of social scientific investigation and the emerging logical positivist account of the logic and epistemology of science, which largely was the framework within which Easton later couched his image of the logic and epistemology of science. Later yet, he described much the same position in terms of what he referred to as a “constructivist” view of theory. There were, however, some distinct difficulties with this amalgamated vision of science.

First of all, for somewhat such as Weber, the idea of a unitary general theory of society was not possible, and for Mannheim also, changing forms of society and culture entailed different investigative premises. Theory was more a matter of creating historically and culturally variable ideal typifications, and theory was considered instrumental in that social science as a whole was ultimately devoted to practical problems and in that ideal types were designed to fit particular historical forms of social
phenomena. In the view of Weber and Mannheim, there were also very definite differences between the natural and the social sciences, with the later primarily concerned with eliciting the indigenous meaning of social phenomena. Easton surely brought elements of this image with him to Chicago, but he was also taken with the claim of Merriam, Catlin and others that social science could more strictly emulate the natural sciences, even though exactly what this involved was not very clear.

Catlin had relied heavily on economic theory as a model as well as on the instrumentalist account of scientific theory advanced by Karl Pearson. This was before the advent of logical positivism, but this search for general theory complemented the emphasis on inductive methods in Stuart Rice’s *Quantitative Methods in Politics* (1928) and reflected ideas similar to the operational account of scientific theory and concepts advanced by Percy Bridgman in his *Logic of Modern Physics* (1927). Although Easton’s premises regarding the heterogeneity of fact and value as well of theory and fact fitted the logical positivist account of science and although both were originally tied to ideological and practical social goals, there were some important differences, which Easton never fully reconciled. For example, The positivist model was one of deductive causal explanation in which theories were indeed mental or analytical constructs but universal and nomothetic in character. and could be tested against factual givens and either verified or falsified accordingly. They were instrumental in that they were neither in themselves true or false but more or less useful in terms of their cognitive capacity for organizing and economically describing facts and generating empirical generalizations that could be verified or falsified by the extent to which they corresponded to observable phenomena that were in some sense experientially given.
Easton’s image of the political system as a scientific concept was a complicated and not fully explicated combination of these accounts of science. The theoretical system was not itself true or false but instrumental and constructed and something to be judged by its usefulness in illuminating and forming empirically testable causal propositions about the realities of the empirical system as it existed and operated at any particular time and place. It was not, however, an ideal typification in a Weberian sense, because it was supposed to be universal in scope and applicable to any empirical system. It is also important to specify some internal logical problems involved in Easton’s formulation, which would persist in his later work. It was unclear whether, as he sometimes implied, the political system was a part of a whole in somewhat the same manner that the circulatory system might be distinguished as part of the human body or whether, as he also at times implied, it was merely analytically constructed, like a set in logic. The latter interpretation would seem to be unavoidable in light of his claim that there was a fundamental difference between any empirical political system and the analytical political system and that an analytical system could be constructed from any number of totally different but analytical related variables. It is probable that this difficulty was never resolved in his work and that it was closely tied up with his attempt to meld systems theory with his logical empiricist philosophy of science. One thing that is clear, however, is that he never relinquished the idea that in natural science theory and fact are ontologically distinct and that whether the foundation of scientific knowledge was based on the positivist notion of given observables or some other ground of scientific realism, theory and reality were different realms even though reality claims
were always theory-laden. And the problem of exactly how this applied to the study of social phenomena remained less than fully resolved.

The Final Design

By the point that the book was published, Easton had begun to formulate in more detail the character of a systems analysis of politics, and this continued to parallel his participation in Miller’s committee. By the mid-1950s, the group had added new faculty members in different disciplines, as well as doctoral and post-doctoral students, and concentrated on systems and cybernetics concepts concerning various levels of complexity in biological and social systems. The first publication of the Committee was presented at the Annual Meeting of the American Psychological Association in September, 1953 and titled “Symposium: Profits and Problems of Homeostatic Models in the Behavioral Sciences” and authored by Miller, R. W. Gerard, Anatol Rapoport, David Easton, and Donald T. Campbell. A more advanced article entitled “Toward a General Theory for the Behavioral Sciences” was published by Miller in the September 1955 issue of the American Psychologist and reprinted in 1956 in The State of the Social Sciences, edited by White, and it contained the papers read in 1955 at the 25th anniversary of the opening of the Social Science Research building at the University of Chicago. Among the participants were Walter Lippman, Arnold Toynbee, and three individuals who later became Nobel Laureates: Hayek, Friedman, and Simon.

On this occasion, Simon offered some reservations with respect to the idea of developing a single formal general systems theory, and Easton’s work continued to reflect a similar sentiment. In 1953, the Miller Committee began to raise funds to construct a building and endow the program. Dr. Raymond W. Waggoner, chairman of
the Department of Psychiatry at the University of Michigan, learned of this activity and invited Miller, together with a group of his senior colleagues, to move to Ann Arbor. He obtained from the Michigan Legislature a commitment to construct the sort of building that had once been envisioned for Chicago and to provide a continuing appropriation of sufficient size to operate the planned institute. Chancellor Lawrence Kimpton of the University of Chicago and President Harlan Hatcher of the University of Michigan agreed to this plan, and in July, 1955, Miller arrived as director of the new Michigan institute, accompanied by Ralph W. Gerard and Anatol Rapoport, who were later joined by Robert I. Crane and Richard L. Meiser from Chicago. Easton continued to attend the Staff Theory Seminar of the Mental Health Research Institute to which he acknowledged his debt in the first rendition of his theory of the political system in World Politics in 1957, which he had previewed the previous year at meetings of the New England Political Science Association and the International Political Science Association.

It is important at this juncture to emphasize once more that despite Easton’s long involvement in these discussions of systems theory, he never subscribed to the search for a general systems theory. For Easton, the basic concept of system with its account of outputs and inputs and other features served more as a model or what he consistently referred to as a conceptual framework, which was actually more an ideal typification than a fundamental claim about the nature of what he referred to as an “empirical system” of politics. It was, instead, a way of understanding such an empirical system and presupposed the dichotomy of theory and fact. This assumption, however, still did not sit easily alongside his equally consistent claim that this framework was a
prototype of what counted as causal theoretical explanations in natural science.
Second, although Easton's vision of how to achieve an advance in the evolution of the
science of politics was internally generated and in many respects continued a long
tradition prominently represented in the work of the Chicago school, he readily admitted
that part of the impetus in moving in the direction of abstract theorizing and the
emphasis on the separation of fact and value and the priority of such theorizing over
applied applications was McCarthyism and the general context of the Cold War. The
manner in which the Chicago historian Daniel Boorstein had celebrated the Genius of
American Politics (1953) and caved before the pressures of the Broyles's Commission
in Illinois dealing with unAmerican activities was a visible indication of such pressure and
the manner in which the cultural atmosphere encouraged the behavioral agenda and
particularly Easton's valorization of the priority of pure science.

In the summer of 1953, Easton was asked to participate in an ongoing Brookings
Institute seminar devoted to a discussion of presidential nominating conventions. In
these meetings, there had been considerable tension between those who pursued a
traditional institutionalist approach to political research and those who were at the
forefront of the evolving behavioral persuasion and urged greater attention to
quantitative techniques and approaches. Because of the nature of Easton's book and
its emphasis on a rapprochement between theory and fact, he was invited as someone
who might mediate the situation but yet tilt somewhat toward the position of the
empiricists. At this point, the behavioral perspective had not gained the status of
science, if such status was to be measured in terms of such things as support from the
National Science Foundation, but Easton's involvement at Brookings aided in moving
toward that goal. These meetings, however, not only pointed up the difference between what came to be understood as “traditional” and “behavioral” political science but exposed a certain tension within the behavioral movement itself. As already noted, behavioralism would never be totally unified with respect to the pivotal concept of democracy, but there was also a degree of division between those who were directly engaged in empirical research and those, like Easton, who were concerned with more abstract theorizing.

After the publication of the book, Easton had continued to work consistently on developing and refining his account of the political system, but he had been under considerable pressure to “deliver” on his theoretical promises. This pressure came from both supporters and dissenters. A student of Strauss’s, Walter Berns, who later became a prominent scholar of constitutional law at Cornell, criticized his work at an APSA panel where Easton was in the audience. Berns claimed that the lack of a fuller rendition of the theory simply indicated the underlying poverty of the basic endeavor as well as of behavioralism as a whole and that Easton’s book did not really go beyond a critique of the existing condition of the discipline. But Easton’s work continued to be less than fully accepted by those such as Warren Miller and Austin Ranney who focused on data collection and survey research. The latter criticism, as well as a wish to gain some relief from his intense theoretical endeavor, moved Easton, by the end of the decade, to become more involved in empirical research, such as his work on the role of children in the political system, but although he wished to proceed slowly and believed that it was premature to attempt fully to elaborate his theory, he did feel compelled to make a concrete and visible move in this direction. The result was the
publication of the 1957 article,\textsuperscript{23} which served as a prolegomenon to the more extended treatment in the 1965 publications. The article had been first submitted to the APSR but rejected, which indicated the still tenous position of both the behavioral movement as a whole and the kind of theorizing in which Easton was engaged.

Part of this later period involved visits to the Advanced Center for the Study of Behavioral Science at Stanford. Working in this multi-disciplinary environment, composed of both humanists and social scientists, included contact at times with individuals such as Friedman, Parsons, Howard Mumford Jones, Arrow, and Ralf Darendorf, and it was an important part of the further development of his theoretical work. By the early 1960s, the draft manuscript elaborating his systems theory had, however, become very long, but it still had been given little public exposure. He had reached a tentative agreement with the editor at Wiley, William Gum, to publish the book without engaging an outside review, but James Murray, the editor at Prentice-Hall, also expressed a very keen and persistent interest in his work. Easton eventually decided in favor of Wiley without informing Murray, but in a subsequent discussion involving Easton, Sylvia, and Murray, Sylvia suggested that since the manuscript in its present form was so long, why not allot the first part to Prentice-Hall and the longer and more detailed portion to Wiley. Thus on the basis of this Solomonic decision, there were two publications in 1965: \textit{A Framework for Political Analysis} and \textit{A Systems Analysis of Political Life} which together not only became the most important theoretical contribution to the behavioral movement but served to articulate most fully and thoroughly the conception of science that became central to the theory and practice of the movement as well as the target of many of the critics.