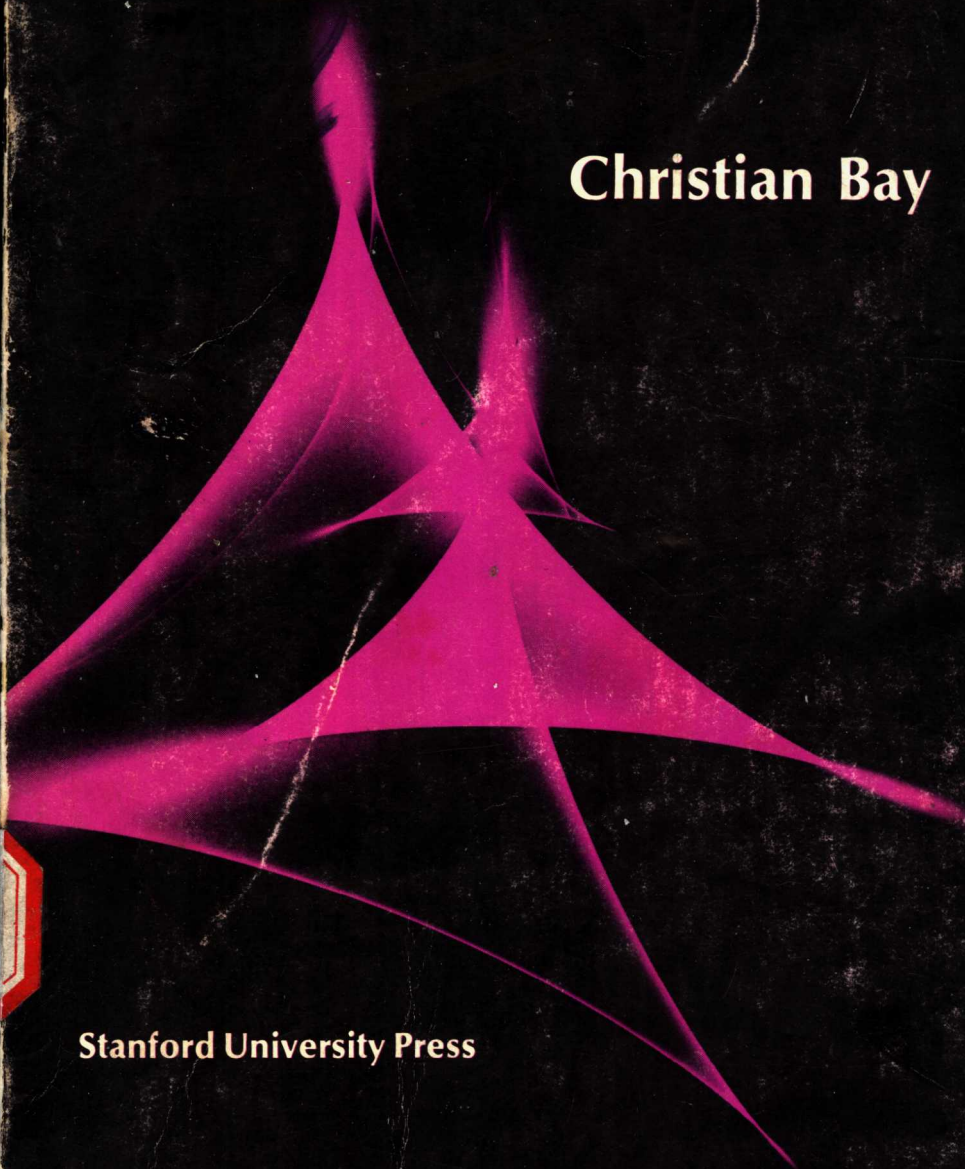


The Structure of Freedom

Christian Bay

An abstract, vibrant magenta graphic dominates the lower half of the cover. It consists of several sharp, overlapping triangular and polygonal shapes that create a sense of depth and movement, resembling a stylized star or a complex geometric pattern. The design is set against a dark, textured background that shows signs of wear and aging.

Stanford University Press

The Structure of Freedom

by Christian Bay

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Til mine foreldre,
JENS OG RUTH BAY,
som har lært meg alt det viktigste

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Preface to the 1970 Printing

It is gratifying to learn that this book is still in demand, and I accept with pleasure the opportunity to write a brief preface to the new printing. A new edition it is not; only a few printing errors that went undetected in the second printing have been corrected.

In the 1965 Preface I recorded a number of relevant respects in which my views had changed in the years following the initial publication of this book in 1958. "Six years older and conceivably six years wiser," I wrote, "I will try to replace some defective parts in the structure of an argument on liberty which on the whole I still consider valid and useful."

After another five years it is a bit unsettling to discover that the 1965 Preface continues to express my present views. At least largely and in substance it does, although I would have changed a few words here and there (for example, I like the word "liberal" less now and prefer, especially when applied to my own stance, words like "humanist" and "radical"). No substantive gains in wisdom in five years?

I still like to distinguish "politics" proper from "pseudopolitics"; I still believe in the crucial importance of new research and theory-building regarding human nature and needs, as an integral part of political inquiry; and, with this proviso, I still consider myself a behaviorist (that is also true, I believe, of a large proportion of the generation that has brought about, in David Easton's not quite appropriate phrase, "the post-behavioral revolution"; see his "The New Revolution in Political Science," *American Political Science Review*, LXIII [1969], 1051-61). I still believe in the vital importance of bridging the gulf that existed between mainly normative political inquiry, too often scholastic or entirely speculative in approach, and mainly empirical inquiry, only fact-oriented and mainly sociological in approach.

But the field has been moving, so much so that by comparison I may seem to have been standing still. The gulf that existed is gone in some areas, and is nowhere as wide and intimidating as it used to be. Political theory is in the ascendancy, and Leo Strauss and those he has influenced are no longer the most influential or even the most visible political philosophers. Indeed, the very terms "normative theory" and "empirical theory" have become a bit disreputable, as indicating a division of labor that makes no sense. Put in Harold D. Lasswell's terms, the separation of "political theory" from "political philosophy" has few spirited defenders today, even though it con-

tinues to make sense for many purposes—also to this writer—to distinguish as carefully as possible between empirical and normative propositions.

Issues of human nature and needs are exceedingly complex with respect to distinguishing propositions that can and cannot, in fact or in principle, be put to empirical test. Yet these are areas of research that can no longer be kept out of systematic political inquiry. And not the least important good news, these last five years, is the real upsurge of interest in the normative and the personality and motivation aspects of political research. Even the better voting studies are taking in increasingly “messy” psychological indicators; more important, in the writings of younger political scientists there is a decreasing tolerance for the conventional behaviorist proneness to reduce political behavior to attitudes (passive) and voting (active). Political activists themselves, many younger colleagues have been cutting down to size the importance of voting and voting research in the practice and study of political behavior.

Among the prime movers in this direction are those who have sponsored or supported or taken an interest in the Caucus for a New Political Science. It is perhaps a hopeful sign of the times that essentially similar groupings have been formed within virtually all the social science professions. While there has been no general stampede to have the young turks elected and anointed as leaders of the traditional professional organizations, the impact of the new breed in the literature and on the teaching of political science in many departments has been considerable already, and shows every sign of still being on the increase.

Many factors no doubt should help account for this healthy trend. Most important, I suspect, have been the increasingly glaring contradictions between the humane and democratic pretensions and the brutal and oppressive realities of the American polity, with respect to its wars of aggression abroad as well as the deepening despair among the underprivileged at home. The utter irrelevancy of perhaps the bulk of traditional behavioral political science to the most pressing problems of public policy of our time has become a scandal to be stomached only by people exceedingly well trained and socialized in the ways of value-blind and need-exempt positivism. Many more among the young political scientists are now apparently becoming educated as well as trained, and learn or find out about war and peace, ecology, mental health, etc., as well as systems theory, voting behavior, comparative political parties, and Guttman scales.

Consequently, political science departments are becoming more exciting places, in which generations discourse and also do battle as never before; and in which politics as well as pseudopolitics is being practiced as well as taught, on a wide scale, with students often as influential in both kinds of dialogue as their professors. This is all to the good. I have come to believe that the most serious problems of an age of rapidly accelerating technological and social developments can be coped with only by a new breed of citizen capable of fresh perspectives as well as having effective access to accumulated knowledge and experience. In other words, I believe that a continuing political dialogue, in which different generations (including students and professors) take part as essentially equals, has become a real prerequisite

for achieving the qualities of responsible citizenship that may yet save our civilization, if we and our children are lucky.

Is it worth saving? I, for one, believe that our freedoms and human rights, for all their underdeveloped states as yet, are supremely worth saving, roughly according to the priorities proposed in this volume. If so, then the same goes for all those institutions that in fact serve these freedoms and rights—and for those institutions only. Which institutions do and which don't are in principle researchable questions. Research and action dictated by these concerns are of course what the problem of National Defense should have been about, and perhaps could have been, had not the vested interests of powerful, overprivileged minorities succeeded, in this as in other countries, in brainwashing large majorities into believing that they live in a democracy and into confusing "defense" with "military ascendancy."

The effective defense of our freedoms and their supporting institutions may conceivably require military equipment, among other resources. But surely a prior requirement is a kind of rationality that insists on first asking exactly what it is that must be defended; and subsequently, in the spirit of scientific inquiry and unencumbered by special pleading, seeks to determine the most likely effective means within the resources now or later available. It may well be found, for example, that the progressive pollution of our environment has created defense problems already far more pressing than even the more alarmist and ethnocentrically lopsided interpretations of, say, Chinese or Soviet behavior can make credible. To speculate a bit further, it may be found that crucial to our defense is a kind of citizenship that makes us insist on vindicating our freedoms in our daily lives and on defending them for all; and that makes us insist on democratic practices to the fullest extent possible in our living and working environments (like universities, or factories, or offices, or schools, or neighborhoods). For what is to be defended surely is not the pretenses of democracy achieved, or the alleged intentions of long-dead Founding Fathers, but the achievements that have been reached in the protection of and respect for human lives, with rights and liberties. While the structures of privilege and power remain as lopsided as they are in this country today, to say nothing of the situation within humanity as a whole, our rights and liberties remain not only precarious but, for some, virtually nonexistent; here, surely, Defense is most badly needed.

My interest in problems of freedom and of advancing and defending freedoms has persisted, and has led me to do subsequent work on the related subjects of citizenship, human needs and wants, civil disobedience, education, and student political activism. Some of my recent papers in these and neighboring areas will be published later this year by Odyssey Press in New York, under the title *Politics for Man*, edited by Susan B. Hendershott.

CHRISTIAN BAY

Oslo, March 25, 1970

Preface to the 1965 Printing

My distinguished friend Dr. Alexander Meiklejohn told me after reading this book that he expects me to keep rewriting it all my life. He did not solely have the book's many weaknesses in mind; his remark also reflected the observation that the subject of liberty is an inexhaustive one as well as an engrossing one to every serious student. He should know.

And he was right. Unlike every other reader of this book, I'm sure, I haven't been able to put it down. There has been no opportunity as yet to attempt a complete restatement of my position on the politics of freedom, however; and a patchwork repair job would not do for this occasion. Save for the correction of a few minor errors, therefore, this edition is unchanged.

My position on liberty and on the uses of social science for libertarian aims is fundamentally the same as it was six years ago. Some of my views on related issues have changed, however, and are perhaps still in the process of further development. It is good to be given the opportunity now to make explicit at what points of substance I today differ with positions adopted in the book. Six years older and conceivably six years wiser, I will try to replace some defective parts in the structure of an argument on liberty which on the whole I still consider valid and useful.

Useful for what? A good portion of this new Preface will examine critically some of the main currents in contemporary political research; in this way I hope to articulate what I take to be some neglected problems and perspectives of inquiry to which the present volume addresses itself. Though in some respects well qualified, if hardly impartial, I shall not attempt a review of my own book, and aside from a passing reference or two there will be no review of the reviews, either. Within the short space available I'd rather speak to the purposes the book is intended to serve, in the context of somewhat modified views on the general objectives of political inquiry.

Let me begin with the term "politics." It so happens that on the issue of how to define this crucial term I do differ significantly with the position adopted in the book. My new perspective on this issue has ramifications for the problem of how to relate value commitments to political inquiry; in a sense it triggers off (or is itself one manifestation of) the whole argument that will be the main theme in this essay: that the prevailing trends in political behavior research, and to a considerably lesser extent research in comparative politics as well, have restricted the role that political inquiry could perform in the service of human needs. (It will be noted in passing that the

most vocal among the contemporary critics of the New Science of Politics, neo-Aristotelian by persuasion, have failed to produce any viable alternative approach.)

"Politics refers to all the processes by which public values are promoted and distributed by means of power and authority." This is the definition adopted in the present volume (p. 21), substantially following David Easton. It is a normatively neutral definition, quite properly; it refers to processes that may or may not spring from acceptable motivations or intentions.

The drawback to this way of defining politics, which has many followers, appears to me now to be the virtual absence of a reference to the relatedness of politics to human needs and problems. In the development of political inquiry as a behavioral science there has been an understandable tendency, as we shall see, to fasten on basic concepts that are operationally useful; with modern research techniques, processes of promotion of values by means of power and influence can be measured, so that purely sociological hypotheses can be put to the test and some behavioral knowledge accumulated. This is true also when what is promoted are schemes for distributing public values, or values "oriented toward the assumed needs or desires or interests of large numbers of people" (p. 13). But the usual approach in the modern behavioral study of politics is to be concerned with desires and *perceived* interests only. To my mind the more crucial reference of political activity in the real world is to the *needs* and to the *real* interests (however they may be conceptualized and studied) of human beings.

To put it more concretely: The mass of behavioral research in political science today deals with voting and with opinions and attitudes on social, political, and economic issues. This literature has filled debilitating gaps in previous knowledge, and we need more studies of voting and of political preferences. But we should not mistake the political horizon we encounter in this research for the whole realm of the political. There is too much that gets lost when attention is focused on what we can readily measure by the standard kinds of sociological techniques—individual meanings of political commitments, for one example.

One sign that all is not well with the current state of political research is the prevailing tendency not to try to relate behavioral data meaningfully to normative theories of democracy or of the good society, whether traditional or new. For example, a prominent team of researchers a decade ago concluded a painstaking analysis of voting behavior with an astonishingly superficial attempt at bringing their data to bear on democratic theory. The American system of democracy, concluded Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, "does meet certain requirements for a going political organization" and, indeed, "it often works with distinction." But these authors apparently saw no need to enlighten their readers on the nature of their criteria for "distinction," or on what works well and what works less well in the American system, according to their normative criteria of democracy—for good reasons, as these criteria were left unanalyzed. And yet we are told that, of the book's two themes, the "confrontation of democratic theory with democratic practice is the second implied theme that runs throughout the book." And this is the final statement in

the book: "Twentieth-century political theory—both analytic and normative—will arise only from hard and long observation of the actual world of politics, closely identified with the deeper problems of practical politics." (*Voting*. Chicago: University of Chicago Press, 1954, pp. 312 and 323.)

Assuredly not—or at any rate not *only*. With a more adequate conception of politics it will become clear, I believe, that what these and many other authors of books on political behavior are looking at is only a limited range of data, which badly needs to be supplemented by more intensive psychological inquiry, and also by a much larger canvas of political theory that includes a place for concepts such as needs, growth, and the common good, to name a few only.

I shall attempt to contribute toward a more adequate definition of politics in a moment, but it will be well first to give one or two more examples of normative imprudence among leading political sociologists. Berelson *et al.* are by no means unrepresentative; some go much further toward ruling out any need for careful articulation of the normative commitments to which their empirical research is related. One highly respected writer has cheerfully claimed that democracy "is the good society itself in operation"; "the give-and-take of a free society's internal struggles" is the best man can hope for on this earth—so good that political ideology is becoming a superfluous commodity, at least in the West, according to S. M. Lipset (*Political Man*. Garden City: Doubleday, 1960, pp. 403 and 415).

For a less extreme example of the same tendency, consider the most recent book by one of the nation's ablest and most versatile political scientists, whose recent death is a great loss. In V. O. Key, Jr.'s *Public Opinion and American Democracy* (New York: Knopf, 1961), we are given an admirably organized survey of current knowledge on the state of American public opinion and on its bearing on governmental decision processes. Yet once again there is hardly a hint of the bearing this knowledge is supposed to have, or could have, on the relevant normative issues of democracy. Only toward the very end there are a few stray remarks on such issues, including the point that political deviants "play a critical role in the preservation of the vitality of a democratic order as they urge alterations and modifications better to achieve the aspirations of men. Hence the fundamental significance of freedom of speech and agitation." There is no attempt to clarify such concepts as "vitality of a democratic order" or "the aspirations of men," or any other aims beyond keeping democracy, in some nebulous sense, going.

For present purposes these examples must suffice. Determined to utilize the available arsenals of sociological techniques, this line of research has stressed the phenomena that can be weighed and counted to the exclusion of more diffuse and elusive aspects of politics. In their desire to be scientific, these investigators have shied away from normative inquiry to such an extent that they unblushingly relate their fine empirical work to the crudest notions of and assumptions about democracy—either as an end in itself or as a means to even vaguer conceptions of human wants.

Now, it is proper to define "politics" in a normatively neutral manner, as I have said. But we are bound to be led astray if we work with a concept of politics which permits us to consider activities of and around voting

and related attitudes as all there is to it. For politics in its more adequate concept must in my opinion be considered an essential dimension of social life, and even of individual man as a social being; and this dimension not only bears on but *is* the individual's relatedness (in whatever role) to perceptions of the welfare of his whole nation, or other largest reference group. And this relatedness, in turn, is in the nature of a recognized or unrecognized common interest in the solution of social problems (or in alleviating them, or in seeking to forestall the development of more distressing human problems).

Social problems are discrepancies between aspirations and realities, or anticipated discrepancies, in so far as the aspirations are shared, and in so far as the discrepancies conceivably can be remedied by governmental or other social action. It may be a matter of deficiencies in security; the problem may be to defend better what we have; what is required may be to think more clearly about what must be defended, to do better research on the nature of the hazards and on the costs of alternate means of defense and to take more effective action on policy decisions arrived at. Or the problem may be a matter of realizing potentialities for improvements; it may be a question of making progress toward a better state of affairs—perhaps more freedom, or more well-being—in some limited respect or more generally; what is required may be clearer thinking about how we ought to live, better research on how we do live and on how we could live, and more effective social and political action.

There is no room here for greater detail or more precision; let us conclude that "politics," whatever else its reference, must also refer to the process of grappling with social problems. I cannot offer any really satisfactory definition as yet; perhaps in another six years a more adequate conception will be at hand. Let me suggest the following working definition, which must do for the present essay: *"Politics" refers to all activity addressing itself toward the solution or alleviation of perceived social problems.*

This formulation leaves open what kinds of problems political actors may perceive, or give priority to. It makes no explicit reference to power, influence, or authority; it leaves open what the categories should be for observing the means employed. It requires the intent of acting toward an objective larger than just a personal or a business corporation's economic stake in a particular policy. According to this definition, activities on behalf of, say, Standard Oil become *political* only to the extent that the actor convinces himself that the policies he promotes would benefit the nation or large communities in it, and not just Standard Oil. I reserve "*pseudo-political*" as the term to refer to private interest-oriented activities that interact with political activities and utilize the political institutions without any intention, or perceived (imagined) intention, of serving any larger social interest or the public interest.

Only if we in some such fashion attempt to distinguish the political from the pseudopolitical can we, in my judgment, hope to achieve a more adequate political theory and research, inconvenient and initially difficult as the distinction may be from the point of view of sociological research at this time.

While normatively neutral, the type of definition of "politics" that I have just proposed does suggest that the student of politics should concern himself more adequately with values than he does now. For behavior-oriented political scientists, "values" have come to refer to preferences that respondents happen to hold, and they are analysed as just another set of facts. In much of this literature it is not determined or even speculated on whether the preferences of respondents have been embedded in a whole framework of commitment to the public good, or are related to business or class interests, or perhaps have no psychological anchorage whatever; indeed, most survey research instruments are in a position to tap only fragmentary responses anyway.

In real life it makes all the difference in the world, of course, whether political opinions are carefully thought out or not; are based on personal anxieties or ambitions, on group loyalties, or on sympathy for others; are strongly felt or superficially held (or simply expressed to get an interviewer off one's neck); and so on. Moreover, it makes a great deal of difference whether or not people think politically as distinct from pseudopolitically, in the sense specified.

It would seem to follow that the political scientist had better try to discover the totality of salient views that political actors hold, if he is to understand the dynamics of their voting behavior and other political behavior. Aside from Harold Lasswell, who has been ahead of his time in several fields of political inquiry, psychologists have been the first to understand this necessity. Among psychologists M. Brewster Smith has been the most influential innovator in this area, and the volume entitled *Opinions and Personality*, which he wrote with Jerome Bruner and Robert White (New York: John Wiley, 1956) is after less than a decade widely considered a classic in this new field. In recent years the first few political scientists have ventured into the intensive study of personalities and political views; notably Robert E. Lane (*Political Ideology*. New York: The Free Press, 1962) and A. F. Davies (*Private Politics*. Mimeo. University of Melbourne, 1962).

Another welcome development is that the study of comparative politics has become broadened to include political cultures as well; leading students of comparative cultures insist on observing political behavior in the framework of the total systems of values, in so far as possible. (There will be more comment on this literature below).

Yet in my opinion the role of values in political inquiry should be expanded even further. The political scientist should feel responsible for articulating his own values, and for structuring as explicitly as possible the totality of his own political commitment. This is not to say that he should be politically active; perhaps so, but that is a choice he must make as a citizen, not as a professional. As a political scientist he merely owes it to himself and to his audience to make it as clear as he can where he stands with respect to those fundamental issues of politics that are relevant to each of his inquiries. If politics once is conceptualized as the activity that addresses itself to the solution of social problems, and if it is desired that political science should have an impact on politics, it not only follows that intelligent political inquiry must evaluate the normative as well as the factual

basis of political institutions, opinions, and behavior. It also would seem to follow that the political scientist must be in a position to examine critically the value assumptions of political actors not merely on an *ad hoc* basis, but on the basis of carefully structured positions of his own on the larger issues of politics.

Every social problem, from the trivial ones to those that bear crucially on human well-being, consists in a discrepancy between what exists and what should be, I have said; and both sides of this relationship are equally in need of careful study. The study of what exists has become the specialty of most modern students of political behavior, and good strides have been made. The trouble is that few behavioralists have taken any interest whatever in the serious study of the "ought-side" of politics. The prevailing view appears to be that the scientist, even the political scientist, not only needs not but should not take a public stand except to report on and to witness to the validity of established facts. (For a recent statement to this effect by an eminent student of political behavior, see Heinz Eulau. *The Behavioral Persuasion in Politics*. New York: Random House, 1963, especially the Epilogue). The unfortunate result is that political *philosophy* (in the now widely accepted sense in which Lasswell distinguishes this discipline from that of political *science*) lately by default has become the almost exclusive domain of a neo-Aristotelian breed of political scientists which has no use at all for such facts as the behavioralists produce!

Not only a bridge but a multilane freeway covering the gulf between factual knowledge and normative study is a necessity, in my opinion, if the rigors of rational inquiry are ever to make a dent in precisely the realm of human behavior in which the spirit of scientific inquiry is least in evidence till now and is most badly needed—the political.

The present volume was above all else intended as a contribution to building such a bridge. It tackles a very broad problem: The general discrepancies between the qualities of liberty that we now enjoy and the qualities that would appear attainable, so far as logical and factual inquiry can ascertain. Such inquiry necessitates a number of normative decisions of priority between freedoms and between types of freedom demands. Many more such decisions would be proper, at least on a tentative basis, if the problem to be studied had been a narrower one—say, the issue of racial discrimination in the rental of houses, or the issue of conscientious objection against military service. But it was hoped that the *general* liberal position advanced in this book would provide a sufficiently viable framework of basic norms to give a common ground to liberal students of more specific political or legal *de lege ferenda* issues. For example, *if* the least free are to be given priority in the advancement of freedom, then it would seem that the interest of the Negro in escaping the ghetto with his family must as a rule take precedence over the desire of some landlords not to rent to Negro tenants. *If* physical violence is the worst evil that our political institutions exist to prevent, or reduce, then conscientious objection would seem an obvious individual right—at least until it can be demonstrated, first, that many will want to exercise it regardless of cost in terms of the fairly mild dysincentives that under the U. S. Constitution are and could be estab-

lished; *and*, secondly, that the irreducible number of insistent objectors, if tolerated, would be large enough to make a significant difference in America's military posture; *and*, thirdly, that this difference would substantially increase the danger of war.

In both examples issues of empirical as well as normative inquiry are involved, even though the former example would seem to permit only one solution, too obvious to require inquiry (as a rule), if one starts out with the present basic liberal commitment. Other issues of competing freedom demands—say, between proponents and opponents of public aid to parochial schools—raise far more complex empirical as well as normative issues. If political scientists are to promote more rational political processes in their professional capacity, they must be ready to study dispassionately the scope and importance of the rival freedoms involved, in this as in other public controversies, and recommend whatever solutions, if any, that in terms of their own carefully developed value positions appear to support the claim of the side for whom the more basic elements of welfare are at stake.

It is time, I submit, that the notion of a value-free political science be abandoned altogether. Every social problem, the proper subject matter of political inquiry, implies one value commitment or another from the moment it is perceived as such; and the clarification of any social problem involves, unless we are deceiving ourselves (for example, by the use of pseudoscientific terms suitable to concealing the value elements involved), a further sharpening of the normative as well as the factual premises from which we wish our inquiry to proceed.

What is important in the scientific study of politics is not to avoid a framework of normative choice—which remains implicitly there if we fail to explicate it—but to separate the operations of factual research from the operations of normative analysis and choice. On this point it is necessary to take issue with the neo-Aristotelians in our midst, who claim that “the political can better be seen by minds that do not draw [the distinction between facts and values] too sharply.” (Cf. Walter Berns. “The Behavioral Sciences and the Study of Political Things: The Case of Christians Bay’s ‘The Structure of Freedom’,” in the *American Political Science Review*, Vol. LV (Sept. 1961), p. 550). On the contrary, it is only if we keep quite clear the difference between statements that can be shown to be (probably) true or false, and statements expressing a value commitment, that we can hope to develop a body of political knowledge that can be communicated among scientists of different political convictions, or who are concerned with different political problems; a knowledge tuned to problems and thus to values, but with clear contours of empirical knowledge which can be utilized for quite different normative purposes as well.

My position on the bearing of political commitment on political research is as sharply at variance with purist logical positivism as it is with anti-positivist neo-Aristotelianism. This is well illustrated by the fact that critics from both extremes have charged that the commitment to freedom advanced in this volume is vacuous. Walter Berns the neo-Aristotelian, being of a mind to “not draw [the fact-value] distinction too sharply,” attributes

to my refusal to claim objective validity for my commitment to freedom the implication that freedom cannot exist. Felix Oppenheim the logical positivist, on the other hand, claims that my commitment to freedom as the supreme value is empty on the ground that I use some terms to which he attributes value implications—such as, for example, “degree of harmony between basic motives and overt behavior” in my definition of psychological freedom. (Cf. Felix E. Oppenheim. *Dimensions of Freedom*. New York: St. Martin's Press, 1961, p. 168). Granted, I value such harmony, as does every clinical psychologist and psychiatrist; but this preference of mine in no way interferes with the task of developing progressively sharper criteria for determining degrees to which such harmony is present or absent.

During the six years since the book was published I have moved a step or two away from Oppenheim's and toward Berns's position, in that I now am willing to affirm a belief in the probability that certain elements of a humanitarian or liberal commitment may eventually be shown to possess a certain kind of objective validity, in the sense that human nature may be shown to make us gravitate toward these tendencies to the extent that we learn to cope with our anxieties. (Cf. my “A Social Theory of Intellectual Development,” in Nevitt Sanford (ed.) *The American College*. New York: John Wiley and Sons, 1961, especially pp. 1000-1005). Perhaps there are some rights that can be considered *natural* after all; not because Aristotle made this claim, but because socio-psychological research may be in the process of establishing a basis for such a claim by way of increasing our knowledge of universal human propensities. The current doctrine of natural law in political science is deficient, asserts James C. Davies, “not in its reasonableness, or even validity, but rather in its relative non-empiricism. It has not been subjected to empirical test and therefore remains more a faith than a tested theory” (*Human Nature in Politics*. New York: John Wiley, 1963, p. 53). Nor is it a testable theory, in most of its formulations so far.

In the meantime I hope that more political scientists, and students of behavior as well as of institutions, will spend less time on debating the merits of parochial approaches, and more time on learning to communicate beyond them. The present state of affairs in political science makes no sense at all, with neo-Aristotelian philosophers disdainful of empirical inquiry on one side of the gulf, confronted with logical positivist behavioralists who shy away from any and all normative commitments on the other side. To make matters worse, communications across the chasm at times suggest the existence of two enemy camps, not two kinds of scholars with complementary contributions to make toward a common objective (cf. especially Leo Strauss. *What is Political Philosophy?* Glencoe: The Free Press, 1959; Strauss's “Epilogue” in Herbert J. Storing (ed.) *Essays on the Scientific Study of Politics*. New York: Holt, Rinehart and Winston, Inc., 1962; and the review article on the latter volume by John H. Schaar and Sheldon S. Wolin, with rejoinders, in the *American Political Science Review*, Vol. LVII (1963) pp. 125-160).

The best hope for a development toward a politically more useful political science, in my opinion, rests with an increasingly close interaction between students of politics and students of psychology. There are dangers,

of course, in making this an exclusive liaison, with temptations to jump from the demonstration of aggressive instincts to assumptions about how war comes about, for example; it is essential that intervening variables are supplied from the coffers of sociology, anthropology, and economics. There is safety in numbers—of variables, of disciplines, and of critics who can move across traditional borders. And yet we get into trouble if numbers in the context of measurement and statistics become all-important. The narrowness of most political behavior literature stems not at all from a disinclination to learn from neighboring disciplines, but from an interest only in the readily quantifiable goods and methodological equipment that they have had to offer. As we have seen, political scientists have eagerly gone to work with tools such as attitude scales, procedures for small group analysis, and indices of social and economical stratification; they have been slow to get involved (excepting an occasional bold pioneer such as Lasswell) with problems of, say, political identity or political motivation.

The stress on sociological variables and scale data in political behavior research has led to a tendency for the lowest common denominator of citizenship—or, more strictly, for the most apolitical or pseudopolitical orientations—to become taken for granted as the mainsprings of our democratic way of political life. Most people vote for reasons of status anxiety, perceived economic interest, or prejudices magnified in election campaigns, we are told; but somehow democracy survives, in some fashion, and individuals adjust to a system in which it appears to be left to chance who will worry about the public interest. Perhaps majorities do tend to be apolitical; but much could be gained by study of those minorities that are not, and also by more study of human and social needs and their relationship to various conceptions of the public interest. Perhaps we might then not only discover how individuals can adjust to ongoing systems and ensure their continued stability, but also develop some knowledge on how social institutions can become better adapted to the service of basic and possibly permanent human needs.

In one area of inquiry it does seem that political scientists are now moving toward a better proportioned interdisciplinary perspective. That is, as already mentioned, in comparative politics, and to some extent also in the study of foreign political systems. In the latter field I consider Lucian W. Pye's study of "Burma's search for identity" in this respect an example of what can and should be achieved (*Politics, Personality, and Nation Building*. New Haven: Yale University Press, 1962). Important landmarks in the new broad-gauged study of comparative politics are Gabriel A. Almond and James S. Coleman (eds.) *The Politics of Developing Areas* (1960), and Almond and Sidney Verba, *The Civic Culture* (1963), both from Princeton University Press.

Yet in this literature, too, there is room for improvement. The theoretical framework and the careful development and integrated study of a broad range of psychological, social and cultural variables are most impressive, but the latter are all cast in the role of independent variables. There appears to be two preponderant aims: To understand the political process in its totality, and to understand how it bears on the prospects for democratic

institutions. A third objective is missing; one which to my way of thinking should take precedence over the second one: To understand how the totality of human welfare is affected by the same political processes, or by feasible modifications in them. *The Civic Culture*, the most recent of the three books just referred to, in the last chapter posits "democratic stability" as the dependent variable, or the overriding goal that political processes should serve, and we are given a summary review of findings pertaining to how particular independent variables, some of them conceptualized or given careful study for the first time in this volume, are likely to bear on the prospects for democratic stability. Yet the latter concept is hardly discussed at all.

The result, probably unintended, is a conservative and at times almost an ethnocentric bias. Now, it may be argued that the present British and American democracies clearly do belong to the most successful experiments so far in political government, and I grant that a conservative acceptance of these particular patterns of political culture might be arrived at by the wisest of men. But it does make a difference how political commitments are arrived at, or at least how they are articulated. Ideally, in my opinion, the commitments of political scientists should reflect a fair amount of careful study and reflection on what are the needs of man that political institutions ought to serve. Perhaps many past and present political scientists have been too cautious or have felt that they knew too little psychology to attempt to develop or explicate their own political commitments in this manner; and anyway, it was not long ago widely and comfortably believed that their own commitments should not be related to their scientific work. But the modern students of comparative political culture are in the avantgarde; they plainly state their commitment to democratic stability; they display all the skills and insights needed for analysing behavioral and institutional variables and sub-variables bearing on democratic stability; and yet, paradoxically, they have felt no urge to go beyond such pat formulations of purpose and ask questions, for example, such as these: What particular aspects of a political order, or what institutions within it, ought to be stable, in what order of priority? What categories of citizens have a particular stake in stability, whether partial or total, and whether democratic or not? What are the costs of stability, in general or, say, in Britain or in the United States, or in parts of either country? How is democratic stability related to liberty? To social justice?

But the work of these investigators has helped substantially to create what is now surely a wide open field for political studies that can more directly and more effectively than hitherto serve the political development of mankind. We now have the theoretical and conceptual equipment, much of which was lacking as late as a decade ago, for beginning to approach empirically, within specified contexts and with more precise definitions, some of the great issues formulated already by Plato and Aristotle: How can men learn to live with each other so as to bring out the best in their nature? What is responsible citizenship? How can justice be made to prevail? What are the limits to political obligation? How much individual freedom is compatible with social justice? And so on.

Students of political behavior have established an increasingly tenacious

body of knowledge on the bearing of various situational and background determinants on political opinions and attitudes; and great strides have been made toward understanding how governmental and other political institutions (including, of course, the courts of law) actually work. Of necessity, perhaps, the study of personalities in politics lags behind; it is perhaps the most difficult area in which to produce cumulative knowledge. More by accident, or if you will by necessity in the particular intellectual climate that has prevailed until recently, the study of political norms and of the implications of commitments has also lagged behind. It is perhaps in this area the real bottleneck has been. Only when the implications and consequences of alternative ideas and policy proposals can be anticipated and evaluated over the long run, as they would affect the welfare and development of various people, will we have a political science capable of improving the dialogue and the political decision processes. Stability is an important achievement of politics, and many countries badly need more of it than they have achieved so far. But it surely is not the only objective of government; a more advanced political science will consider and study a variety of hypothetical improvements in our social order which, if feasible, conceivably could improve the quality of social and individual life.

It may well be asked if this volume and also this new Preface may not rest on overly optimistic assumptions concerning the qualities and potentialities of human nature. In an important sense the basic premise that individual freedom ought to be maximized assumes that, by and large, men are or could become able to live peacefully together under conditions of lessened restraint.

No proof is available for this premise, of course. Neither could its negation be proved. History as well as the contemporary scene are replete with examples of beastliness in men, and also of saintliness, not to mention simple kindness and nobility of spirit. A study of the frequencies of each category of deviance from the "average" would establish no new knowledge on the general goodness or badness of human nature, I submit; at most we could discover some of the extremes, in either direction, toward which human behavior can be pushed, by constellations of external and internal factors.

The difficulty with understanding human nature is at least twofold: it can be studied only by way of indirection—by eventual generalization from unending series of studies of individual behavior in different cultures and under different exigencies. We are in ignorance, so far, about the nature of the attributes we all may have in common as men, for one thing because they are so well camouflaged and so thoroughly transformed by culture and personality.

The other and in some ways more pernicious aspect of our ignorance about human nature stems from the powerful incentives in many quarters toward stereotyping our beliefs about human nature. Every man of power and every conservative (whether in socio-economic or in religious matters) has certain important incentives toward portraying our human nature as weak or bad. In the first place, our human nature is one of the best scape-