

# Culture and the Comparative Study of Politics, or the Constipated Dialectic\*

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It may well be that presidential addresses are read largely, perhaps exclusively, by subsequent presidents when the arts of procrastination finally fail and they in turn are driven to confront the doleful prospect of writing their own address and, in the course of so doing, are moved to seek a feel for what has gone before. At least this has been my circumstance. I have browsed these presidential pastures, usually with profit, frequently with pleasure, and, occasionally, with real delight. On the basis of these forays I would recall to the memories of some of you and commend to the attention of all others at least one such prior performance. Its style is literate, its wit well-honed, and its message compelling if not comforting. It is entitled "An Obituary of a Political Scientist" and was delivered twenty-three years ago to the Southern Political Science Association by our colleague, Jasper Shannon.<sup>1</sup>

Dr. Shannon's address was an allegory—poignant, mocking, bitter and yet funny, exaggerated but possessed of a discomfiting core of truth. It consists of autobiographical fragments from the writings of Caspar Don Quixote Milquetoast, a recently deceased and not altogether mythical professor of political science—writings which, in microcosm, trace Dr. Milquetoast's own career and shifting professional commitments and, in macrocosm, reflect the "development" of the discipline between 1917 and 1950, a dark and pre-scientific age known to most of us only by dim repute.

Professor Milquetoast has an impressive albeit dwindling capacity for enthusiastic and wholehearted commitment to the reigning professional passions of the discipline. The flavor is well conveyed by a passage from his writings dated April 10, 1917:

Political Science is a new study, one whose frontiers are scarcely clear. Reflection and investi-

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<sup>1</sup> *Journal of Politics*, 13 (February 1951), 3-18. Quoted by permission of the author and the *Journal of Politics*.

gations reveal certain laws of politics as clear and definite as those of physics. First of all, it cannot be disputed that the whole trend and tendency of human relations is towards more democracy. Man is an incurable political animal. The evils which have crept into democratic governments are the result of too little democracy. Without question the cure for the evils of democracy is more democracy. The rebirth of American democracy is on its way with the introduction of the initiative and referendum, the development of proportional representation in cities, the movement for the short ballot and the growth of the direct primary, together with federal control of Big Business monopolies. As we look back to the dark days of the last half of the nineteenth century, it is evident that the vision of democracy held by Jefferson and Lincoln was only temporarily obscured. La Follette, Bryan, Theodore Roosevelt, and now one of our own professors of political science, Woodrow Wilson, have made clear the truth which Rousseau undertook to demonstrate a century and a half ago; namely, that human nature is basically and fundamentally good though it has been temporarily corrupted by political institutions. It is true that some critics have wanted to know who created these institutions if not men; and if men were good, why did they create them? This is unworthy of the true student of government for we have simple and conclusive evidence of the contrary in our contemporary world. The present war in which we are now unfortunately engaged is showing once more the inevitable progress towards the perfection of democracy.

When this war is over, undoubtedly some kind of world organization will arise and, with American Democracy in the lead, we will move towards a fuller and more complete freedom. The triumph of democracy is only a question of time.

A marginal notation added in December, 1923, indicates that: "This chapter needs revision in the light of subsequent developments in the Harding Administration."

The entry for 1925 reads:

It is more and more clear that governmental problems must be solved at the grassroots. The principal issues of government are essentially those of administration. Government is not only a police agency, but it is a service agency as well. The trend of the time is towards urbanization and centralization. The census returns show that rural America is passing. We must make our cities more healthful, more beautiful, more livable. This demands the integration of power in the hands of

one man who can give us better city government. The invention of the city manager plan combined with the employment of a merit system in government is what is essential to sound local government. Efficient business practice must be applied in city government even as it has been in our well-run corporations. Businessmen must play a larger role in government so that their organizing and administrative talents may be brought to the solution of political problems, just as they have so competently improved our standard of living.

This, too, bore a subsequent annotation dated November, 1929: "My confidence in business efficiency is somewhat shaken."

A note of doubt and discontent creeps into our now unemployed hero's credo in 1933:

In previous chapters I have neglected the importance of economics for politics. A perusal of Charles A. Beard's *The Economic Interpretation of the Constitution*, his *Economic Basis of Politics*, or his more recent volumes, *The Rise of American Civilization*, will convince the most stubborn mind that economics is the clue to politics. The only trouble is the way economics is taught. Professors of economics are generally as dismal as their subject and are creatures of little imagination who have become slavish idolaters of the capitalist system. They are proof of Plato's contention that we become like that which we study. Congressional investigations have showed that much of the economic order is wrong. It will have to be turned upside down. We need to study the Russian system more closely. They have solved the problem of unemployment while millions here are near starvation.

But then after four years as a public administrator with WPA, Caspar concluded in 1937:

. . . I confess my feelings are somewhat contradictory . . . I have always been an opponent of monopoly, but I had thought only of business monopoly. My experiences in government lead me to question the total monopoly of government as much as the partial monopoly of business. I suspect my senses are now more keenly attuned to the doctrine of the separation of powers than formerly . . . Bureaucracy really exists and is not solely a chimera of the Republican National Committee . . . Why I never saw the close relationship of the welfare and police state I do not know . . . It is really tragic what we political scientists do not know. I think I will turn to psychology for help . . .

Needless to say he did and by 1939 found that all they [the psychologists] have told me is that life is a dream and a bad one at that . . . Psychoanalysis has told me little about political science but much about political scientists.

Thereafter our hero staggered on towards 1950, death, and final repose, insatiable in his

quest for the Holy Grail of professional truth. At points en route he concluded variously. In August, 1945:

. . . My life, everything, is now dependent upon the ability of a Missouri ex-necktie salesman in Washington and an ex-divinity student in Moscow. Not even the gremlins could have created such a condition . . . Honestly, I am beginning to have a nostalgia for Calvin Coolidge.

And, finally, in August, 1950:

Occasionally I feel an urge to be an historian, a mortician of the past, for hindsight is so much more helpful than foresight. Thirty years ago I mounted my good horse and rode off to war against politicians. Now I pray for more and better politicians. I seek not uniformity and efficiency, but cohesion and peace. These can come only through compromise . . . I am tempted to say, "Blessed are the politicians for they are the architects of peace. Oh, Lord, save us from preachers, generals, idealists, educators, and administrators!"

The fragments end with an injunction to his wife: "Please destroy this manuscript . . ."

#### **Thesis and Antithesis: Behavioralism Versus the "Area Approach"**

Dr. Shannon brings perspective and humor as well as irony to bear on a question that too seldom engages our systematic attention: "How over time does the discipline change?"—against such a background, I dare not say "develop." In an immediate sense we are all in varying degrees aware of and sensitive to the disciplinary fads, fancies, and feuds of the moment. But memory is a selective faculty—perhaps mercifully so—and we seldom reflect on these sometimes painful matters in deeper perspective or in much detail. Were we to do so, I suspect we would discover a phenomenon that I hereby denominate as "the constipated dialectic." It has theses and antitheses aplenty, but whether it leads to synthesis remains questionable.

I would speak to you this afternoon about a particular instance of just such a "constipated dialectic" and do so with cathartic ends in mind. The context is that of comparative politics, the general topic is the practical implications and consequences of cultural differences for the comparative study of politics, and the specific thesis and antithesis involved are the oft-alleged incompatibility of what, for shorthand purposes, I will call respectively the area and the behavioral approaches to the comparative study of political systems.

What is the nature of the problem? As best

I can tell and reduced to its simplest and most basic elements, it seems to be as follows.

Many behavioralists allege that something called "the Area Approach" lacks rigor and scientific potentiality. It is viewed as descriptive and relativistic, often historical or institutional rather than behavioral in focus, and normally idiosyncratic in terms of its findings. In current professional parlance these are not terms of praise.

A few so-called "area types" respond with counter-charges of cultural illiteracy, gross ethnocentrism, uncritical scientism, and scornful characterizations of those members of the opposition who venture abroad as "itinerant methodologists" or worse.

And thus, thesis confronts antithesis and battle is joined.

The fray is a strange one, however, which is explicable more in psychoanalytic than in objective terms. To begin with, the area camp is in considerable disarray. Even in a physical sense it is by no means a unitary force. There really is no single area movement, approach, or method comparable in any sense to even so diffuse a phenomenon as the behavioral movement or approach. What actually exist are very loose congeries of academics united only by a common interest in particular areas such as Japan, China, India, Slavic Europe, or Latin America and lacking any common identity or organization that transcends such specific regions or cultural areas. They certainly do not share anything so pretentious as a methodology or a theory.

Second, the area group, despite truly remarkable accomplishments, is not at all complacent about its own scholarly performance. Everywhere in area circles one encounters fervent injunctions about going beyond traditional area studies, of escaping from the "area box," as it is frequently put. Considering that a brief twenty-five years ago the universities of the United States possessed the merest handful of scholars fluent in any non-Western languages or competent in such subject matters as the politics, history, economics, literature, or philosophy of any non-Western society and that today its endowment in these respects is not even remotely rivaled anywhere else in the world, such self-abnegation on the part of those responsible for this achievement is, to say the least, remarkable.

These two characteristics—a lack of unity and appreciable intellectual dissatisfaction with their own performance—combine to produce a third relevant phenomenon: the area group is far more aggressed against than aggressing.

The charges come largely from the behavioral side and are usually justified as necessary to the effort to convert political science into a more scientific discipline. Occasionally an area proponent replies, but, to use the military metaphor, the area tactic has been one of skirmishing rather than pitched battles or frontal attack.

The locus of this anti-area animus in what I have been calling in grossly oversimplified terms "the behavioral camp" is also of interest. In my experience it is not shared by many of the founders or leaders of what has been termed the Behavioral Revolution. Any such bias is notably absent—indeed, often specifically disavowed—in the works of such scholars as Almond, Dahl, Deutsch, Eulau, or Truman. In fact it is difficult to trace the evolution of the animus in specific terms. Like Topsy—it just grew, and eventually became a part of the behavioral view, widely shared in many, although not in all, quarters.

If, for the sake of argument, one grants that all this is true, that I am providing an essentially accurate description of an episode of disagreement and strife within the discipline, the question remains: "Does it really matter? Isn't this just one more tempest in the academic teapot? Why should we care?"

I think that it does matter, and I would like to devote the balance of my remarks to an attempt to explain why I believe this to be so. I have three categories of argumentation in mind:

- (1) The grounds for dissension are mostly spurious;
- (2) The episode provides important and, I hope, constructive insights into the processes of intellectual change and commitment within the discipline; and
- (3) It points the way to catharsis and synthesis. The dialectic need not continue constipated and lacking in synthesis.

Allow me to develop these points in sequence.

### Is There an Area Approach?

My first point was that the grounds for dissension are largely spurious. I would argue that there really is no such thing as an "area approach."

There are undoubtedly individual political scientists of faculty rank who in their teaching or research specialize to varying degrees in the politics of particular areas such as Japan, Nigeria, India, or Brazil. The forthcoming Lambert survey of area programs estimates that

there are probably several thousand of them. As one of their number, I have always found it a bit difficult to understand why this was in any way more remarkable—let alone more questionable—than specialization in the political systems of Great Britain, France, Germany, or the Commonwealth. These latter have been standard parts of the discipline since the nineteenth century. Logic aside, however, it is true that the large-scale extension of systematic professional interest to non-Western political systems that developed after World War II did entail both stress and change within the discipline. Those involved in this intellectual diaspora were faced with at least three critical problems: (1) an unusually formidable language barrier—few American scholars spoke or read such languages as Chinese, Arabic, or Hindi; (2) the manifest inadequacy of the traditional categories of comparative political analysis which were largely legal and institutional in nature and had been developed with only the Western European and American experiences in mind; and (3) the problem of devising alternative categories of analysis sufficiently broad and precise to incorporate both the traditional Western political systems and a variety of quite different non-Western polities.

The shared characteristics of what some have called “the area approach” derive from this particular set of problems which are common to all scholars working in non-Western settings. These characteristics are basically commonsensical. First, such so-called area specialists place great emphasis on the importance of learning at least the major language of the society concerned. Communication is prerequisite to effective study or learning. Our most common languages—English, French, German or Spanish—are usually not adequate for this purpose. Therefore, one must learn to speak and read the languages that are. It is that simple.

Second, area specialists are apt to be unusually sensitive to the impact of culture on politics. The postwar study of comparative politics in non-Western settings began with a recognition of the sterility of trying to analyze these polities in terms of their constitutions, laws, and executive, legislative, and judicial systems. Political institutions that bore familiar Western names simply did not perform functions analogous to those of their Euro-American namesakes. The obvious question was: “Why?”—the equally obvious answer: “Cultural difference.”

From this rather elementary perception

stemmed a much more detailed and sophisticated search for the cultural determinants of particular non-Western political institutions, events, attitudes, and behaviors. The questions asked were not basically different from those involved in a similar analysis of the American or British political systems. They differed primarily with respect to the investigator’s point of departure—and, I might add, that of his audience. One started more or less from scratch. Little or no prior research that was directly relevant existed; the basic data in currently useful forms had literally to be created. Lacking were the enormous advantages of operating in the culturally familiar settings of developed Western societies with scholarly traditions that are both compatible and, in important degree, cumulative. The result was that political scientists working under these circumstances shortly became very sensitive—perhaps excessively so—to the total cultural matrix in which politics is set; and they felt obliged to write and teach about the societal setting and determinants of politics to a degree that is uncommon in professional writings about the American political system where both author and readers usually share so much more of the contextual knowledge concerned, or at least think that they do. It is only in this limited sense that an area specialization may be said to be interdisciplinary. While sensitized to the relevant theories and findings of other disciplines, it is still basically a mono-disciplinary approach.

A final characteristic that area specialists tend to share is a high regard for the importance and relevance of history as a determinant of political outcomes. This is really a subcategory and a consequence of their concern with the cultural context of politics. One does not study culture in an historical vacuum. The same should be true of politics. It is a conceit born of the Enlightenment and reinforced by certain trends in current social science theorizing that political attitudes, behavior, or institutions can be explained or understood in terms that do not involve a substantial historical element. Professional exposure to societies where the so-called premodern stages of history are chronologically so much closer to the present, where nation states have emerged well within the reach of living memories, and where people still live and strive to cope with problems that seem by our standards archaic imbues one with a particularly keen sense of the contemporary relevance of history.

This set of characteristics—a concern with language, with culture, and with history—is

really all the "theoretical" or "methodological" baggage that area specialists possess in common. I personally do not find it a very impressive endowment. As I said earlier, it is basically commonsensical, wholly predictable, and, I should say, inevitable given the nature of their professional subject-matter. It is certainly not of an order of complexity, precision, interrelatedness, or pretentiousness that would justify labeling it as a methodology or even a technique. Where then is "the area approach?"

The matter does not stop at this point. Richard Lambert of the University of Pennsylvania has recently completed—but not yet published—a massive and most impressive review of language and area programs in the United States. In the course of his activities he tried not only to identify area specialists but also to determine what they actually do. The results are interesting. For example, among the more than four thousand college and university teachers with area specializations in Lambert's sample, 73.6 per cent taught at least one course that had no area-specific required reading material.

The same group were asked to rate themselves on a seven-point scale as to the degree to which their teaching, research, and other professional activities were concerned with their area rather than their disciplinary interests, a rating of seven indicating complete area orientation. The mean scores were 4.1 for teaching activities, 5.3 for research, and 4.8 for other professional activities. Only a minority (14.6 per cent for teaching, 33.2 per cent for research, and 24.3 per cent for other professional activities) regarded themselves as completely area rather than disciplinary in their orientations. It should also be noted that these figures represent results for all area specialists regardless of discipline and are seriously skewed toward the area pole by large numbers of historians and humanists whose disciplines are traditionally organized along area lines. They would be far lower for political scientists with area specializations.

In the light of these findings, I would submit that not only is there no shared body of theory or methodology that can legitimately be labeled an "area approach," but also there are relatively few political scientists who see themselves as more area than disciplinary in their professional activities and orientations. It is in this sense that I view the grounds for dissension between the behavioral and area camps as well as the concomitant charges against area specialists within the discipline as largely spurious. To revert to the symbolism implicit in the

name of Professor Shannon's antihero, they remind one of Don Quixote's famous tilt with the windmills—sincere, energetic, and zestful to be sure, but sadly ill-formed.

#### **Why the Tensions between Area Specialists and Behaviorists?**

But the windmills were there! The valiant but myopic Don was right in at least this respect. And in a sense the windmills of the mind at which some of our colleagues have been tilting are also there. Which brings me to my second point. This episode that pits some behaviorally inclined members of the profession against some area-inclined members provides potentially important insights into the processes of intellectual change and commitment within the discipline.

What has actually been going on? In my estimation we have here a case of parallel development leading to intradisciplinary hostilities. Permit me to explain. In doing so I will ignore the pre-1945 antecedents of both area studies and behavioralism. They are significant in both cases but isolated and small in scale and disciplinary prominence.

In their early modern stages, area studies were a newborn phenomenon in the sense that our vastly expanded national relations and concern with Japan, China, and the U.S.S.R. in particular—although not necessarily friendly—produced a widespread awareness of our abysmal ignorance of these major societies and their cultures. The postwar world was really new in this sense. It expanded enormously our national spheres of interest, concern, involvement, and responsibility. A sort of communications revolution ensued and brought with it a demand for specialized higher education in the languages, histories, cultures, and politics of the non-Western peoples involved. In any event, the interest and response generated in academic circles was genuinely academic and professional in nature. The early area programs were financed by the universities themselves with substantial assistance from the Rockefeller Foundation and the Carnegie Corporation of New York. Government funds were not involved.

The oldest organized area programs date from about 1947 and relate primarily to Japan, the U.S.S.R., and China. For the most part, programs specializing in other areas—South or Southeast Asia, the Near and Middle East, Latin America, Africa, or Eastern Europe are of considerably later vintage, most having been established since 1959. The ages of area programs range, therefore, from about

twenty-six years down to ten or eleven years. The average is probably around fourteen.

Their organizational form varies widely from the loosest type of committee structure to full-fledged centers and institutes and occasional departments. So does their leadership. Most directors are historians or humanists. Practically all area programs are interdisciplinary in the sense that their staff includes representatives of two or more—occasionally as many as ten or twelve—different academic disciplines ranging widely over the humanities and social sciences. Relatively few of them, however, have separate staffs. The individuals concerned are mostly on loan from departments and thus professionally dependent primarily on their disciplinary rather than their area connection for such basic matters as appointment, pay, and promotion. This is important.

Important also are certain aspects of the developmental history of area programs. There was a certain mystique about them in the early days and some rather ill-considered and exaggerated claims were made about areas as a basic mode or unit of scholarly organization alternative to the standard disciplines. In practice, such claims were seldom taken very seriously, however, and far more significant was the sense of excitement aroused by being involved in an activity that was simultaneously pioneering in character, calculated to redress the older ethnocentric biases of the profession, and doing so along lines that promised substantial achievement in both humane and disciplinary terms.

Such circumstances inevitably fostered group loyalties and a mild sort of academic nationalism. It is noteworthy, however, that these loyalties were parochial and focus not on the area approach or movement as such but on the particular country or region with which a given scholar was involved, to wit, Japan, China, India, etc. What bound these groups together was not shared theory or methodology but a common interest and stake in studying and interpreting the language, literature, history, economy, or politics of the area concerned. Note also that in the early days at least this specific area focus tended to rule out any more general comparative interests. The tendency was to study and explicate the political systems of these non-Western areas for their own sake as major and neglected institutions about which very little was known. For the most part these early investigations were not guided by theory partially because there were then—as now—no very persuasive universal models of

political systems and, partially, because from a scholarly standpoint the most impressive and demanding aspect of the societies concerned was our almost total ignorance of their workings. Practically speaking, it was felt that some measure of basic information gathering had to precede the construction of grand theory.

This did not, however, connote a total absence of theoretical or methodological concerns. The political scientists involved tended to share a profound dissatisfaction with the traditional forms of political analysis developed in Western settings. Their search for alternative modes of analysis had a great deal to do with the emergence of the structural-functional approach and the new emphasis on process rather than form in the study of foreign political systems, as well as with the renaming and restructuring of the field that we now call comparative politics rather than comparative government. They were also among the first to use systematic interview techniques in societies other than our own and thus to add a new dimension of life and realism to what had been a decidedly documentary if not dreary tradition.

What I would like particularly to emphasize, however, is not so much the state of the area art in its earliest stages—exciting, innovative, parochial, mildly xenophobic, descriptive, and but marginally concerned with theory and methodology—as the subsequent processes of change which are common to its later stages and are in a number of cases still emergent. I will do so with specific reference to political scientists who have area specializations and largely ignore the other disciplines and specialists involved in the area endeavor.

The important thing to note is that nothing stays the same; everything changes. If one's prime concern is with the scholarly quality of this area-oriented product in terms of teaching and research—as I believe it should be—a number of factors besides the competence of the individual scholar must be taken into account. Among them are the age and stage of scholarly development of the particular area group concerned, the state of indigenous scholarship in that area and thus of the native resources upon which the area specialist may draw, the ease or difficulty of access to the area for purposes of study or research, and the availability of funds for research and training purposes. All of these factors are subject to change and, as they change, so does the area field concerned.

Several aspects of this process of change are of particular importance. First, the professional loyalties of the political scientist who is also

an area specialist have always been bifurcated. He may participate actively in an area program, but usually his career interests lie basically with the discipline. He is, therefore, impelled to keep abreast of changes in the discipline and is apt to be affected by them in very much the same fashion as are his nonarea-oriented colleagues. Second, the mild xenophobia that characterizes the early stages of area organization does not normally persist. With the passage of time, the initial mystique of both the language and the culture diminishes, particularly for the social science members of an area program. They tend to become less absorbed with the particularities of the area and progressively more concerned with how their area knowledge fits into such more general frameworks as political development or comparative politics. Finally, and most important of all, the process of generational change has time to operate. New cohorts of political scientists trained along newer disciplinary lines infiltrate the ranks of the older first-generation area specialists and bring with them all of the theoretical and methodological apparatus of their day in graduate school.

The result of this joint process of attrition and development is an area field that differs greatly from what it was five, ten, or twenty years ago. Its practitioners are trained primarily as political scientists. Their professional interests and skills reflect quite faithfully those of the discipline at large. They differ mostly in the sense that they have spent an additional year or two in graduate school or abroad acquiring language skills and general area competence.

This then is the first of the two processes of development that I set out to explain. It is also the thesis in the dialectic that I am positing. The parallel development of the behavioral movement constitutes the antithesis.

This audience does not need still another account of the rise within the profession of that composite of theories, methods, and techniques that is loosely subsumed under the title of behavioralism. You have lived with the experience and are familiar with the history involved. Permit me, however, to dwell briefly on those aspects of the movement that are in my opinion responsible for engendering its antipathy to area studies and area specialists.

First, the life spans of the two movements—area studies and behavioralism—largely coincide, with area studies being somewhat the older of the two, having started in organized fashion about 1947 rather than in the early 1950s. This overlap facilitated several kinds of

rivalry, both real and fancied. To begin with there was obviously a potential for conflict between a creed that proclaimed "The root is man," to borrow Eulau's famous phrase and one that at least seemed to be saying "The root is area cum culture." That in fact few area specialists in social science fields really upheld such a tenet did not much matter. They could with some superficial validity be portrayed as doing so.

At a more mundane level some behavioralists also felt that they were in competition with area specialists for scarce foundation funds. In the 1950s and for much of the 1960s behavioralism was a young and struggling movement. Some of its most characteristic techniques, in particular survey research, were uniquely expensive, and the dollars were hard to come by. This was precisely the period when the major foundations were providing relatively handsome support for a selection of area programs, and given the adverse opinion of area studies held by many behavioralists, it was natural that they should both envy and resent what they perceived as a regrettable misallocation of scarce funds.

There was also some rivalry at the campus level. At the major universities in particular, area specialists were usually organized in centers or programs which were externally funded in substantial part and which enjoyed a peripheral status in the contentious hierarchy of their colleges of liberal arts. The behavioralists, on the other hand, were apt to be based either in the Department of Political Science or in research institutes. In either event they tended to view the area centers and personnel as rivals for both support and status within the university community. This, too, was not conducive to amiable relationships.

In the 1950s and early '60s, in particular, there were also strong contrasts between the professional working styles, methods, and products of behavioralists and area specialists—contrasts that could only accentuate the other causes for dissension. The behavioralists in those days worked almost exclusively in the field of domestic American politics. By so doing they inherited the fruits of the richest storehouse of social science data and findings in the world. To it they brought new approaches, greater rigor, important discoveries, and, ultimately, an impressive increment of new knowledge. Their work was characterized by a predominant concern with methodology, theory building, and canons of proof, by the emphasis placed on mathematical and statistical techniques, and, ultimately, by their at-

tempts to achieve for the discipline a more truly scientific status.

The area specialists on the other hand were at this time contemplating the legacy of social science data and findings that awaited them in such places as China, India, Bolivia, or Kenya. It varied enormously in quantity, quality, and accessibility. In some cases—China and Russia for example—the borders were physically closed to meaningful social science research, and it was necessary to observe from a distance and to develop the black arts of Kremlinology and Pekingology. In others the borders were open but the social science cupboard was bare. Practically no one had ever seriously studied the political systems concerned in terms that had contemporary utility. In still others one encountered rich and ancient traditions of indigenous scholarship that had in varying degree been modernized. But the prime emphasis of such scholarly communities was apt to be historical, literary, or philosophical; and, where they addressed themselves to contemporary politics, there was a high probability that they did so in descriptive, legal, and institutional terms. In few of these cases was there much cause for rejoicing among American-trained political scientists and area specialists confronting these scenes with the sorts of questions engendered by their own recent revolt against legal and institutional analysis at home.

The professional consequences of circumstances such as these are not well understood by those who have not confronted them in person. Practically speaking, one never really starts quite from scratch. There is always some sort of pre-existing literature or lore to be discovered and assayed for its ore content. Thereafter, if one has any sound sense of the impact of cultural difference on the shape, content, and interrelations of political institutions, attitudes, and behavior, it is not wise to plunge right into the sort of rigorously and compatibly defined microstudies of political attitudes and behavior that are the norm at home. You may not be measuring the same thing at all when dealing with concepts so shifting and elusive as party identification, leadership styles, or sense of personal political competence or efficacy. Some prior sense of overall cultural form and style and of the interrelations of particular political institutions, attitudes, and behaviors therewith is a functional prerequisite to more refined ventures. So are the collecting and evaluating of basic political data that are simply taken for granted in this country.

This type of scholarly inquiry has, in my

opinion, very appreciable value in its own right. Ideally, it need not preclude a growing attention to more scientific modes of inquiry; but, in fact, in the early years of an area venture it often tends to do so. For the most part this has been the predominant style in what I characterize as the initial stage of the development of a given area field. This has been particularly true of the older area fields such as the Japanese or Russian. It is probably less so in the case of the newer fields such as Africa where the relative youth and recency of academic training of the individuals involved act as a countervailing variable. In any event it is easy to see how in the 1950s and early '60s the American-oriented, scientifically fixated behaviorist element in the discipline came to look askance at their foreign-oriented, culturally fixated area colleagues on grounds of incompatibility of working styles, methods, and scholarly products. The practice of the time largely substantiated such a judgment, and few on either side really understood or took the trouble to investigate what the other group actually did and why they did so.

More fundamental, and also more lopsided, than this, however, is what I can only define as the ideological element in the behavioral versus area confrontation. I have earlier made the point that the level of ideological—or, if you will, methodological and theoretical—commitment to an “area approach” was never very high in area circles and that it has diminished in later years, especially for the social science contingent involved. The typical attitude is one of concern about the so-called “area box” and whether or not the work of area specialists is contributing appropriately to something often described as the mainstream of innovation in the discipline. Very little ideological ardor or arrogance is involved.

The situation is different with many of the behaviorist group—with numerous notable exceptions, particularly among the intellectual leaders of the movement. I find the phenomenon fascinating, especially against the background so admirably depicted in Dr. Shannon’s “Obituary of a Political Scientist.” Like Caspar Milquetoast, the discipline has to an extent seldom acknowledged lurched from one ruling obsession to another. In the space of thirty-three years (1917–1950)—slightly more than the classic definition of a single generation—Caspar’s career embodies at least five such commitments, impressive in zeal if not in duration: to evolutionary democracy, to efficiency in government via city managers and business methods, to economic determinism and socialist

politics, to the merit system, and to psychological determinism. But whereas Caspar's capacities for enthusiastic dedication dwindled with time and progressive disillusionment, those of the profession show little sign of doing likewise. They are more reminiscent of the valiant Don Quixote, untouched by repeated rebuffs and ever ready to pick himself up from the dust, mount his staunch Rosinante, and ride off to new and more glorious quests and combats.

It is not that the discipline lacks a solid underpinning of impressive accomplishments that have added enormously and cumulatively to its sophistication and understanding of political phenomena since the early years of the century. It is rather the undiminished ardor with which, against such a background, it embraces new causes and panaceas. What a truly stupendous "forgettery" must be involved! What a magnificent, if short-sighted, capacity for emotional as well as for intellectual commitment! How remote from an attitude of cultivated and cautious skepticism toward all professional nostrums! Perhaps this is a subconscious reason for our recent aversion to historical inquiry?

It must be added that if Caspar Milquet's successive intellectual idols have been able to mobilize even briefly an impressive following and degree of commitment within the profession, their potency pales before the totems of behavioralism. No magic—unless that of religion or of nationalism—has proven as strong or as energizing as that of science. A social scientist who is really convinced of the scientific quality of his methodology and findings is reminiscent in several respects of the legendary Presbyterian rising from his knees to do the will of God. Not infrequently such apocalyptic zeal carries over into professional relations with colleagues of less faith or different persuasion. This would seem sometimes to have been the case with attitudes toward area specialists, inevitably rendering more difficult their relations with behavioralists.

It is sad to think that the process I have described may be all that lends substance to this particular controversy within the discipline. If the diagnosis is accurate, the estrangement is compounded largely of ignorance, misunderstanding, and misplaced zeal. Insofar as it has substance, this is found in circumstances which prevailed in the earlier stages of development of both area specialization and behavioralism and then probably only in marginal degree. Nevertheless, the animosity has been real and has had unfortunate conse-

quences in terms of the training, staffing, and funding of political scientists. Most important of all—fruitful professional relations between behavioralists and area specialists have been impeded and rendered unnecessarily difficult.

### The Path to Synthesis

The third and final point that I want to address relates to the possibility of synthesis and catharsis. Must the relation between behavioralists and area specialists continue to be estranged and barren? I think not—and I would like to offer a few observations and speculations on this score.

Fundamentally, two emerging and—it is hoped—reinforcing trends are involved in this possibility of synthesis. On the one hand, the area specialists in political science have for the reasons stated earlier long been subject to professional influences that have moved them steadily away from idiosyncratic area orientations in the direction of far greater interest in the sorts of methodological, theoretical, and comparative concerns that are central in the discipline. Even a cursory examination of the research designs of work in progress in most area fields will demonstrate the extent to which this is true in practice. The trend is, of course, not universal; but it is certainly impressive and growing.

On the other hand, as you are all aware, the interests of many behavioralists have been shifting to the international and comparative scenes in recent years. This is a development of major significance. In its origins and for most of its brief history, behavioralism in the discipline of political science was almost wholly an American movement. Although gradually it has acquired foreign converts, including some very distinguished ones, it began in this country, its leaders have almost always been Americans, and its data base and findings were long geared almost exclusively to the American scene. The results were, of course, impressive; but the degree to which they may in fact be culture-bound has never been adequately explored.

The problems involved are numerous and serious. For example, no behavioral field is better developed, more sophisticated, or more mature than that of electoral attitudes and behavior. And, I would assume, since the American Voter series, no concept has been more seminal or central to the conduct of scholarly research in this field than that of "party identification." But unless I misread him completely, Donald Stokes, a principal author of the American Voter series, was suggesting in his

1972 paper before this Annual Meeting that the role of party identification or partisan self-images in electoral change is significantly different in Great Britain than in the United States.<sup>2</sup> I am practically certain on the basis of my own experience in Japan that the concept lacks any very close cognitive equivalent in that quite different cultural milieu. One does not have to go far to uncover comparable problems of equivalence in such popular fields as elite studies or political socialization. Apparently, cultural variations can make a critical difference with respect to the validity or, perhaps, even the relevance of some of our more cherished conceptual units of analysis and comparison. If so, this cuts very close to the heart of the entire comparative endeavor insofar as it is couched in cross-cultural terms.

Lest anyone think that the problem is limited to this particular level, let me recall to your memories Frederick Frey's splendid essay on "Cross-Cultural Survey Research in Political Science."<sup>3</sup> There he sets forth in impressive and appalling detail the sorts of problems that await the scholar who seeks to optimize the accuracy and reliability of data obtained through a given survey instrument administered in several different cultures. If not sufficiently dismayed by Frey's account, you might go on to peruse in the same volume Dell Hymes' equally prescient and pioneering but even more basic essay entitled "Linguistic Aspects of Comparative Political Research."<sup>4</sup>

I point out these difficulties not to discourage further cross-cultural and comparative research by political scientists but to suggest that the profession has yet to face effectively the attendant problems of training and research strategy. If it is true that cultural difference is this critical a variable in the entire comparative enterprise and if the problems can be so severe between cultures as similar as those of Great Britain and the United States, we are in very serious trouble. If our research abroad, particularly in non-Western settings, is to acquire a respectable behavioral dimension, distressingly few of our behaviorally inclined colleagues have been adequately trained for this purpose. For the most part, they really don't know enough about the languages and cultures involved to be able to cope effectively with the scholarly problems posed by cultural

difference. Methodology alone is not enough.

I see two long-term solutions to this problem. The first lies in a synthesis of behavioral and area training. Fortunately, this is already under way; but the initiative comes more from the area than from the behavioral side. Increasingly, at a number of our major universities a Ph.D. in political science with area specialization differs in only one significant respect from a straight disciplinary doctorate. The individual concerned meets the same requirements, receives the same training in statistics, methodology, and theory, and is distinguishable from his other colleagues only by reason of the fact that he has invested an additional eighteen months to two years in language and area work in order to secure his degree. As the entry requirements become more difficult and the better universities cut down still further on the size of their entering graduate classes, this tendency becomes even more pronounced. From the profession's standpoint this is a most satisfactory solution. It results in the implantation of what a colleague of mine used to refer to as "two skills in one skull"—an ideal synthesis.

The only real problem with this solution is cultural lag within the discipline. Both graduate students and faculty with area specializations are still viewed with suspicion in many behavioral quarters. They are not infrequently regarded as a different breed, inherently inferior in quality, and of dubious loyalty to the cause—not true believers. The result is a subtle but effective form of discrimination which tends to steer individuals who are not really dedicated to an area interest along other and more orthodox career paths.

I submit that this is not in the best long-term interests of either the profession as a whole or, for that matter, the future development and enrichment of the behavioral approach. We have spoken for years of the comparative mode as the only truly scientific approach to the study of politics. If we really believe this, we must equip ourselves and train our successors to understand and deal far more effectively with the problem that is fundamental to all cross-national and much domestic research—cultural difference. One does not do this by exalting methodology at the expense of language and area training. Both are essential to the future of comparative politics.

I indicated a moment ago that I could envisage two long-term solutions to this problem. Actually they are complementary, not mutually exclusive. One is the "two skills in one skull" technique just described. The second has a

<sup>2</sup> "Some Comparative Reflections on Electoral Cleavages in Britain," unpublished paper, pp. 2-10.

<sup>3</sup> In *The Methodology of Comparative Research*, ed.: Robert T. Holt and John E. Turner, New York, The Free Press, 1970, pp. 173-294.

<sup>4</sup> *Ibid.*, pp. 295-341.

better-known rubric—"the internationalization of social science."

It sometimes seems to me that there are curious and little-remarked parallels between the postwar course of world politics on the one hand and the global social science scene on the other. I would not press this analogy too far, but let me mention a few of the similarities I see.

In both spheres the period from 1945 to about the mid-1960s is at least mildly reminiscent of the attitudes, values, and performance characteristics implicit in the phrase, "The American Century." If American foreign policy may be said to have been characterized during this period by global pre-eminence; by either monopoly or supremacy in nuclear weaponry; by worldwide involvements and engagements; by a more or less general acceptance of the superior wisdom, virtue, and efficacy of its own motivations, policies, and acts; by what on balance can only be termed its "good intentions"; and by enough explicit or tacit acceptance of these views in some foreign circles to facilitate their acceptance at home, do not certain analogies suggest themselves with respect to the postwar record of American social science in general and political science in particular?

It was not only the cities and the political and economic institutions of the ancient civilizations of Europe and Asia that were decimated by the war. Their scholarly institutions suffered equally, thus creating what—to pursue the initial metaphor—we may call a "vacuum" into which American intellectual and academic aid, prestige, ideas, training, and influence flowed or pushed. The domestic immunity of the United States to the physical consequences of the war plus its unprecedented affluence and the fervor and energy of some of its social scientists resulted in a species of worldwide scholarly penetration that was certainly unprecedented in so short a space of historical time. This was particularly the case in the social sciences.

The reception of this enhanced American presence abroad varied, of course, but the aid was accepted and in time enough converts were made to the views and methods of American social science so that new currents of scholarship were set in motion throughout the world. The entire process was undoubtedly much helped by the fact that during most of this period the United States was in the throes of the so-called Behavioral Revolution and that this seemed to many at home and not a few abroad the newest, most vital, exciting, and

promising development in the field of the social sciences to be found anywhere. The result has certainly not been a mass capitulation of foreign systems of higher education in the social sciences to the American persuasion, but it has resulted in a vastly expanded American presence, and a degree of activity, ferment, and influence that has never before characterized the external reach of American social science.

The result is that in postwar times American social scientists have been concerned, involved, and active abroad in both the West and the non-West to a completely unprecedented extent. Whatever the underlying assumptions and short-term consequences of this American social science diaspora, a great deal of contact of academic subcultures across national boundaries ensued; and one began to hear talk of something called rather vaguely "the internationalization of social science."

This is not the occasion to discuss the manifold problems and pitfalls of such an endeavor. I merely want to point out the physical and intellectual improbability of long-term progress in the study and understanding of comparative politics so long as the endeavor is based largely on American money, personnel, methods, and theories. The cultural and intellectual arrogance and the degree of ethnocentrism that underlie any such assumption should be obvious. However, such can be our passion for the cause of social science and our unquestioning acceptance of the rectitude and value of our professional endeavors that the question may never be raised in these terms. It should be routine! There is no day-in and day-out equivalent for the cultural sensitivity and understanding of a talented scholar native to the society concerned who has also been trained in a social science tradition that is in working terms at least compatible, if not identical, with our own. There is also a real function and need for the perspective and objectivity of the foreign observer, particularly if he adds to his linguistic and area skills the perceptiveness of a de Toqueville or a Bryce; but such individuals cannot really replace native scholarship. Among other things, there is simply too much to be done and far too few qualified foreign scholars to do it.

It is curious how seldom we discuss this question of the role of non-Americans in the advancement of comparative social science. If we did, it would never occur to most of us to think that this was really an American monopoly. Yet we often act as if this were the case, especially where the other cultures concerned are non-Western. In any long-run sense

it should be obvious that the bulk of the data gathering and of the scholarship with respect to any society is going to be done by its own scholars, if it is done at all. There is a role for Americans, as there is for all other foreign scholars of comparative bent and appropriate training; but this is likely to be supplementary and, eventually, derivative in the sense that it builds upon and criticizes the products of local scholarship.

In the interim pending the advent of a time when particular societies have produced conditions in which such fruitful interaction across national lines by social scientists is possible, there are problems of a scope, complexity, and virulence that sometimes surpass belief. I do not underestimate these. There will undoubtedly be more instances of governments that choose to regard the raw data of social science as national possessions akin to oil deposits or classified military intelligence. In such aggravated cases the only remedy is time; the only effective prescription—continued effort plus patience. This type of denial of access, while worrisome and inconvenient, is not fatal and is often partial or temporary. The Chinese and Russian cases demonstrate what can be accomplished even in the face of extreme difficulties.

Granting the problems involved, our long-term interest clearly lies in supporting more general freedom of access to the data of social science by all responsible and objective scholars, providing training and other forms of professional assistance to foreign communities of

political scientists where this is both feasible and desired, and seeking to extend opportunities for collaborative research with foreign political scientists both at home and abroad where the nature of the project involved renders this both efficient and practicable. A program of this sort lends substance to that otherwise undefined phrase "the internationalization of social science." It is also calculated to add an invaluable second dimension to the profession's capacity to carry out effective cross-cultural research in comparative politics—the first dimension being the aforementioned combination of behavioral and area training that I have referred to as "two skills in one skull."

This is the type of synthesis that, in my opinion, the discipline of comparative politics really needs. Given the developments already in process in both the behavioral and area camps, it seems to me that the time is ripe for its realization. By this I mean that in an increasing number of individual cases behavioral training and skills have already been combined with their area counterparts. All that remains to be done is to acknowledge openly the utility, indeed the essentiality, of both the area and behavioral traditions to the further progress of the discipline and to expand the numbers receiving such dual training. At that point synthesis will at last have been achieved and the dialectic consummated. I venture to predict that so glorious a *dénouement* would rejoice the shade of the late Professor Caspar Don Quixote Milquetoast.