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THE SEARCH FOR PARADIGMS AS A HINDRANCE TO UNDERSTANDING

By ALBERT O. HIRSCHMAN*

IN a recent issue of this journal, Oran Young argued forcefully against the “collection of empirical materials as an end in itself and without sufficient theoretical analysis to determine appropriate criteria of selection.”¹ The present paper issues a complementary critique of the opposite failing. Its target is the tendency toward *compulsive and mindless theorizing*—a disease at least as prevalent and debilitating, so it seems to me, as the one described by Oran Young.

While the spread of mindless number-work in the social sciences has been caused largely by the availability of the computer, several factors are responsible for the compulsion to theorize, which is often so strong as to induce mindlessness. In the academy, the prestige of the theorist is towering. Further, extravagant use of language intimates that theorizing can rival sensuous delights: what used to be called an interesting or valuable theoretical point is commonly referred to today as a “stimulating” or even “exciting” theoretical “insight.” Moreover, in so far as the social sciences in the United States are concerned, an important role has no doubt been played by the desperate need, on the part of the hegemonic power, for shortcuts to the understanding of multifarious reality that must be coped with and controlled and therefore be understood *at once*. Interestingly enough, revolutionaries experience the same compulsion: while they are fond of quoting Marx to the approximate effect that interpreting the world is not nearly as important as changing it, they are well aware of the enormous strength that is imparted to revolutionary determination by the conviction that one has indeed fully understood social reality and its “laws of change.” As a result of these various factors, the quick theoretical fix has taken its place in our culture alongside the quick technical fix.

In the following pages, I do not have a central epistemological theorem to offer that would permit us to differentiate between good and

* The substance of this paper was originally presented at a Conference on Social Science Research and Political Change in Latin America organized by the Centro de investigaciones sociopolíticas para América Latina (CISAL) at Pacific Grove, California, May 9-11, 1969. It was written while the author was a Fellow at the Center for Advanced Study in the Behavioral Sciences, Stanford, California. David Riesman contributed extensive comments.

¹ Oran R. Young, “Professor Russett: Industrious Tailor to a Naked Emperor,” *World Politics*, xxi (April 1969), 489-90.

bad theorizing, or between fruitful and sterile paradigmatic thinking. My accent throughout is on the kind of *cognitive style* that hinders, or promotes, understanding. I introduce the topic by a critical look at two books that exemplify opposite styles. Subsequently, I make an attempt to delineate various areas in which an impatience for theoretical formulation leads to serious pitfalls. Theorizing about Latin American society and economy, on the part of both Latin Americans and outside observers, receives special attention because it has been particularly marked by the cognitive style I find unfortunate.

I

John Womack's *Zapata and the Mexican Revolution*² and James L. Payne's *Patterns of Conflict in Colombia*³ are the two books I shall use to open the argument. They have in common that they are both by young North American scholars; both, in fact, were originally written as doctoral dissertations; and they were both published early in 1969. But this is where any possible resemblance ends. At this point I should state that both books aroused in me unusually strong feelings: I found Womack's way of telling the Zapata story extraordinarily congenial, while I was strongly repelled by Payne's book in spite of its crispness, cleverness, and occasional flashes of wit. There are of course many striking contrasts between the two books that can account for these opposite reactions, not the least perhaps being that Womack obviously fell in love with revolutionary Mexico and the Zapatistas whereas Payne's treatment exudes dislike and contempt for Colombians in general, and for Colombian politicians in particular. But the more important, and not necessarily related, difference is in the cognitive styles of the two authors. Within the first few pages of his book Payne presents us triumphantly with the key to the full and complete understanding of the Colombian political system. The rest of the book is a demonstration that the key indeed unlocks all conceivable doors of Colombian political life, past, present, and future. Womack, on the other hand, abjures any pretense at full understanding right in the Preface, where he says that his book "is not an analysis but a story because the truth of the revolution in Morelos is in the feeling of it which I could not convey through defining its factors but only through telling of it." "The analysis that I could do," so he continues, "and that I thought pertinent I have tried to weave into the narrative, so that it would issue at the moment right for understanding it" (p. x).

² New York.

³ New Haven and London.

And indeed what is remarkable about the book is the continuity of the narrative and the almost complete, one might say Flaubertian, absence from its pages of the author who could have explained, commented, moralized, or drawn conclusions. Yet whoever reads through the book will have gained immeasurably in his understanding not only of the Mexican Revolution, but of peasant revolutions everywhere, and Womack's very reticence and self-effacement stimulate the reader's curiosity and imagination. Payne's book, on the contrary, obviously explains far too much and thereby succeeds only in provoking the reader's resistance and incredulity; the only curiosity it provokes is about the kind of social science that made an obviously gifted young man go so wrong.

Here, then, is the experience behind the title of this paper: understanding as a result of one book without the shadow of a paradigm; and frustration as a result of another in which one paradigm is made to spawn 34 hypotheses (reproduced, for the convenience of the reader, in the book's appendix) covering all aspects of political behavior in Colombia and, incidentally, the United States as well.

Perhaps I should explain briefly what Mr. Payne's basic "insight" or paradigm consists in: politicians in Colombia, he has found out through questionnaires, interviews, and similar devices, are motivated primarily by status considerations rather than by genuine interest in programs and policies, as is predominantly and fortunately the case in the United States. He uses the neutral-sounding terms "status incentive" and "program incentive"; the former characteristically motivates Colombian political leaders whereas the latter animates their North American counterparts. In plain language, occasionally used by the author, Colombian politicians are selfish (p. 70), ambitious, unscrupulous, unprincipled, exceedingly demagogic—interested exclusively in increasing their own power, always ready to betray yesterday's friends and allies, and, to top it all, incapable of having friendly personal relations with anyone because they feel comfortable only with abject supplicants (p. 12). On the other hand, there is the politician with a program incentive whose preferred habitat is the United States of America. *He* enjoys working on concrete policies and achieving a stated goal; hence he is principled, willing to defend unpopular causes, always ready to come to constructive agreements, hard-working, and generally lovable.

For a North American to contrast Colombian and United States politicians in terms of such invidious stereotypes is, to say the least, a distasteful spectacle. We must of course allow for the possibility that truth, as unearthed by the scholar, turns out to be distasteful. But

Payne does not betray any sense of realizing the unpleasantness of his discovery. On the contrary, he evidently draws much satisfaction from the edifice he has built and takes good care to make sure that there will be no escape from it. At various points he assures us that Colombians are like that; that, as he put it in a subtitle, they are not "on the brink of anything"; that it is futile to expect any change in the pattern of Colombian politics from such incidental happenings as industrialization or urbanization or agrarian reform: like the three characters in Sartre's *Huis Clos*, the 20 million Colombians will just have to go on living in their self-made hell while Mr. Payne, after his seven-month diagnostic visit (from February to September, 1965, as he informs us in the preface), has returned to his own, so much more fortunate section of the hemisphere.

It is easy to show that the Payne model is as wrong as it is outrageous. In the first place, it is unable to explain the very wide swings of Colombian politics; after all, during almost all of the first half of the twentieth century Colombia stood out as a "stable" democracy with peaceful transfers of power from one party to another; throughout the Great Depression of the thirties when almost all other Latin American countries experienced violent political convulsions, constitutional government continued in spite of much social unrest.

This experience is hard to explain by a theory that holds that vicious political in-fighting, untrammelled by any concern with programs or loyalty, holds continuous sway throughout the body politic. Moreover, such a theory ought to take a good look at—and give a special weight to—the body's head: if Payne had done that he might have noticed that his stereotype, the politician with a status incentive, simply does not apply to a number of the most outstanding leaders and recent presidents of Colombia—there is no need to mention names, but it is amusing to quote, in contrast, from a recent portrait of a contemporary President of the United States: "His preoccupation seems to have been success—in this case the achievement of power rather than its use for political purposes."⁴

Supposing even that the diagnosis is essentially correct and that politicians in Colombia are more interested in the quest for power *per se* than in the use of this power for the carrying out of specific programs—what does this "insight" explain? Suppose that we find, as Payne indeed does, that those self-seeking politicians frequently switched sides or vote for demagogic measures, does this finding teach us anything fundamental about the political system, its ability to ac-

⁴Nora Beloff and Michael Davie, "Getting to Know Mr. Nixon," *The Observer*, February 23, 1969.

commodate change, to solve newly arising problems, to assure peace, justice, and development? It does nothing of the sort, but at best leaves us with the proposition, which incidentally is both platitudinous and wrong, that if the politicians are vicious, the ensuing politics are likely to be vicious too!

Let us pass now from the paradigms of James Payne to John Womack, who has rigorously excluded from his universe any semblance of a paradigm. It is of course impossible to do justice to his narrative. I shall refer here only to one particular turn of the events he describes in order to show how he invites speculation and thereby contributes to the possibility of understanding.

It has perhaps not been sufficiently remarked that the book has *two* protagonists: Zapata dominates the action during the first nine chapters, but in the important last two chapters (80 pages) the leading figure is Gildardo Magaña who became Zapata's ranking secretary after mid-1917 and, after a brief fight for the succession, the chief of the Zapatista movement following Zapata's death in April, 1919. Womack honors Magaña with one of his too-rare character portraits: "From these stresses [of his youth] Gildardo Magaña somehow emerged strong and whole. What he had learned was to mediate: not to compromise, to surrender principle and to trade concessions, but to detect reason in all claims in conflict, to recognize the particular legitimacy of each, to sense where the grounds of concord were, and to bring contestants into harmony there. Instinctively he thrived on arguments, which he entered not to win but to conciliate" (p. 290).

Womack then relates the exploits of Magaña as a resourceful negotiator of ever new alliances and contrasts him with the rigid and sectarian Palafox, Zapata's earlier principal secretary, who "seemed in retrospect the individual responsible for the Zapatistas' present plight—the man they could blame for their disastrous involvement with Villa in 1914, their alienation of worthy chiefs in the constitutionalist party, and their abiding reputation as the most intransigent group in the revolutionary movement" (p. 306).

After the murder of Zapata, Magaña maneuvered tactfully and successfully among the various chiefs. After six months, the succession crisis was over and Magaña was recognized as commander-in-chief, with the movement virtually intact. Womack then traces the complex events through which the Zapatistas, as he puts it in the title of his last chapter, "Inherit Morelos"—that is manage, by alternately fighting and negotiating and by backing Obregón at the right moment, to pass from outlaws into local administrators and members of a national coalition. "So ended the year 1920, in peace, with populist agrarian

reform instituted as a national policy, and with the Zapatista movement established in Morelos politics. In the future through thick and thin these achievements would last. This was the claim Zapata, his chiefs, and their volunteers had forced, *and Magaña had won and secured*" (p. 369; italics added).

Twice Womack implies that this outcome was due not only to the presence of Magaña, but perhaps also to the absence of Zapata from the scene. There is first the "extraordinary maneuver" by which Magaña offered the Carranza government the Zapatistas' support when United States intervention threatened in the Jenkins case in 1919. Womack says here flatly, "Had Zapata lived, Zapatista strategy could not have been so flexible" (p. 348). Then again at the celebration of Obregón's victory, on June 2, 1920, "twenty thousand Agua Prieta partisans marched in review through the Zócalo, among them the forces from Morelos. And watching with the honored new leaders from a balcony of the Palacio Nacional . . . stood the squat, swarthy de la O, frowning into the sun. From an angle he looked almost like Zapata, dead now for over a year. (If de la O had been killed and Zapata had lived, Zapata would probably have been there in his place, with the same uncomfortable frown, persuaded by Magaña to join the boom for Obregón but probably worrying, as Magaña was not, about when he might have to revolt again.)" (p. 365).

Out of these bits and pieces, there emerges a proposition or hypothesis that must have been on Womack's mind, but that he allows the reader to formulate: did the comparative success of the Morelos uprising within the Mexican Revolution rest on the *alternating* leadership, first of the charismatic, revolutionary Zapata and then of the skillful, though highly principled, negotiator Magaña? And what are the "lessons" of this story for other revolutions and, in particular, for revolutionary movements that are confined to a limited portion or sector of a nation-state?

The historian is probably ambivalent about such questions. He revels in the uniqueness of the historical event, yet he constantly intimates that history holds the most precious lessons. And I believe he is right on both counts! Perhaps the rest of this paper will show why this is not a self-contradictory position.

II

First let me return briefly to the comparison of Payne and Womack. What strikes the reader of the two books most is, as I said before, the difference in cognitive style: Payne, from the first page to the last,

breathes brash confidence that he has achieved complete understanding of his subject, whereas Womack draws conclusions with the utmost diffidence and circumspection. His respect for the autonomy of the actors whose deeds he recounts is what gives his book its special appeal and probably contributed to the spectacular accolade he received from Carlos Fuentes in the *New York Review of Books*.⁵ For it is today a most unusual restraint. I believe that the countries of the Third World have become fair game for the model-builders and paradigm-molders, to an intolerable degree. During the nineteenth century several "laws" were laid down for the leading industrial countries whose rapid development was disconcerting to numerous thinkers who were strongly affected by what Flaubert called "la rage de vouloir conclure."⁶ Having been proven wrong by the unfolding events in almost every instance, the law-makers then migrated to warmer climes, that is, to the less developed countries. And here they really came into their own. For the less developed, dependent countries had long been objects of history—so that to treat them as objects of iron laws or rigid models from whose working there is no escape came naturally to scholars who turned their attention to them. Soon we were witnesses to a veritable deluge of paradigms and models, from the vicious circle of poverty, low-level equilibrium traps, and uniform stage sequences of the economist, to the traditional or non-achievement-oriented or status-hungry personality of the sociologist, psychologist, or political scientist. A psychologist may find it interesting some day to inquire whether these theories were inspired primarily by compassion or by contempt for the underdeveloped world. The result, in any case, is that the countries of Latin America, for example, appear to any contemporary, well-read observer far more constrained than, say, the United States or France or the USSR. Latin American societies seem somehow less complex and their "laws of movement" more intelligible, their medium-term future more predictable or at least formulable in terms of clearcut simple alternatives (such as "reform or revolution?"), and their average citizens more reducible to one or a very few stereotypes. Of course, all of this is so exclusively because our paradigmatic thinking makes it so. Mr. Payne is merely the latest in a long line of "law"-makers, model-builders, and paradigm-molders who have vied with one another in getting an iron grip on Latin American reality. And it must now be

⁵ March 13, 1969.

⁶ I have long looked for a good translation of this key concept into English. It now strikes me that an apt, if free, rendering of Flaubert's meaning would be "the compulsion to theorize"—which is the subject and might have been the title of the present paper.

said that Latin American social scientists have themselves made an important contribution to this headlong rush toward the all-revealing paradigm.

Elsewhere I have described as "the age of self-incrimination" one phase of the efforts of Latin Americans at understanding their own reality and the lag of their countries behind Europe and the United States. Incidentally, traces of this phase can be found in a few contemporary Latin American intellectuals, and they, jointly with their bygone confrères, provide Payne with some telling quotations about the despicable character of Colombian politicians and politics. By and large, the phase has fortunately passed; it has, however, been replaced by a somewhat related phase that might be called the age of the *action-arousing gloomy vision*: on the basis of some model or paradigm, the economic and social reality of Latin America is explained and the laws of movement of economy and society are formulated in such a way that current trends (of terms of trade, or of income distribution, or of population growth) are shown to produce either stagnation or, more usually, deterioration and disaster. The art of statistical projection has made a potent contribution to this type of forecast, which is then supposed to galvanize men into action designed to avert the threatened disaster through some fairly fundamental "structural changes."

Now I believe that this strategy for socioeconomic change has sometimes been and can on occasion again be extremely useful in just this way. But for several reasons I would caution against the exclusive reliance on it that has recently characterized Latin American social and economic thought.

There is a world of difference, by the way, between this action-arousing gloomy vision and the Marxian perspective on capitalist evolution. In the Marxian perspective, events in the absence of revolution were not at all supposed to move steadily downhill. On the contrary, capitalist development, while punctuated by crises and accompanied by increasing misery of the proletariat, was nevertheless expected to be going forward apace. It was in fact the genius of Marxism—which explains a large part of its appeal—that it was able to view both the advances and the setbacks of economic development under the capitalist system as helping toward its eventual overthrow.

My first criticism of the vision ties in directly with my dislike of paradigms laying down excessive constraints on the conceivable moves of individuals and societies. Why should all of Latin America find itself constantly impaled on the horns of some fateful and unescapable dilemma? Even if one is prepared to accept Goldenweiser's "principle

of limited possibilities" in a given environment, any theory or model or paradigm propounding that there are only two possibilities—disaster or one particular road to salvation—should be *prima facie* suspect. After all, there *is*, at least temporarily, such a place as purgatory!

The second reason for which I would advocate a de-emphasis of the action-arousing gloomy vision is that it creates more gloom than action. The spread of gloom is certain and pervasive, but the call to action may or may not be heard. And since the theory teaches that in the normal course of events things will be increasingly unsatisfactory, it is an invitation *not* to watch out for possible positive developments. On the contrary, those imbued with the gloomy vision will attempt to prove year by year that Latin America is going from bad to worse; a year like 1968—and this may hold for 1969 as well—when the economic performance of the three large and of several small countries was little short of brilliant, will come as a distinct embarrassment.

Frequently, of course, the theories I am criticizing are the result of wishful thinking: wouldn't it be reassuring if a society that has been unable to meet some standard of social justice or if an oppressive political regime were *ipso facto* condemned to economic stagnation and deterioration? For that very reason we should be rather on our guard against any theory purporting to *prove* what would be so reassuring.

But the propensity to see gloom and failure everywhere is not engendered only by the desire to reprove further an oppressive regime or an unjust society. It may also be rooted in the fact that one has come to expect his country to perform poorly because of its long history of backwardness and dependence; hence any evidence that the country may possibly be doing better or may be emerging from its backwardness is going to be dissonant with previous cognitions and is therefore likely to be suppressed; on the contrary, evidence that nothing at all has changed will be picked up, underlined, and even greeted, for it does not necessitate any change in the preexisting cognitions to which one has become comfortably adjusted. This is so because people who have a low self-concept and expect failure apparently feel some discomfort when they suddenly perform well, as psychologists have shown.⁷ In this manner, social psychology provides a clue to a Latin American phenomenon that has long puzzled me, yet has struck me with such force that I have invented a name for it—the "failure complex" or "fracasomania."

Finally the paradigm-based gloomy vision can be positively harmful.

⁷ Elliott Aronson, "Dissonance Theory: Progress and Problems," in R. P. Abelson and others, eds., *Theories of Cognitive Consistency: A Source Book* (Chicago 1968), 24.

When it prevails, hopeful developments either will be not perceived at all or will be considered exceptional and purely temporary. In these circumstances, they will not be taken advantage of as elements on which to build. To give an example: the rise of the fishmeal industry in Peru and the similarly spectacular growth of banana planting in Ecuador from about 1950 to the mid-sixties contradicted the doctrine that the era of export-promoted growth had ended in Latin America. As a result, economists first ignored these booms, then from year to year predicted their imminent collapse. It is quite possible that particularly the latter attitude held down the portion of the bonanza that the two countries might otherwise have set aside for longer-term economic and social capital formation; for why bother to exert oneself and, in the process, antagonize powerful interests if the payoff is expected to be so limited and short-lived? More recently, another theory of gloom has been widely propagated: it seems that now the opportunities for import-substituting industrialization have also become "exhausted" even though it can be argued that, just as earlier in the case of *desarrollo hacia afuera*, there is still much life left in *desarrollo hacia adentro*.⁸ Again, if the exhaustion thesis is wholly accepted it may weaken the search for and prevent the discovery of new industrial opportunities.

In all these matters I would suggest a little more "reverence for life," a little less straitjacketing of the future, a little more allowance for the unexpected—and a little less wishful thinking. This is simply a matter, once again, of cognitive style. With respect to actual socioeconomic analysis, I am of course not unaware that without models, paradigms, ideal types, and similar abstractions we cannot even start to think. But cognitive style, that is, the kind of paradigms we search out, the way we put them together, and the ambitions we nurture for their powers—all this can make a great deal of difference.

III

In trying to spell out these notions in greater detail I shall now make three principal points. In the first place, I shall explain why the gloomy vision is in a sense the first stage of any reflections about a backward reality and shall make a plea for not getting stuck in that stage. I shall then attempt to show that in evaluating the broader

⁸ See my article, "The Political Economy of Import-Substituting Industrialization in Latin America," *Quarterly Journal of Economics*, LXXXII (February 1968), 1-32. The Spanish terms *desarrollo hacia afuera* and *desarrollo hacia adentro* are convenient shorthand expressions for growth through the expansion of exports and of the domestic market, respectively.

social and political consequences of some ongoing event we must be suspicious of paradigms that pretend to give a clearcut answer about the desirable or undesirable nature of these consequences. And finally I shall suggest that large-scale social change typically occurs as a result of a unique constellation of highly disparate events and is therefore amenable to paradigmatic thinking only in a very special sense.

The initial effort to understand reality will almost inevitably make it appear more solidly entrenched than before. The immediate effect of social analysis is therefore to convert the real into the rational or the contingent into the necessary. Herein, rather than in any conservatism of "bourgeois" social scientists, probably lies the principal explanation of that much commented-upon phenomenon—the conservative bias of social science in general, and of functional analysis in particular. This very conservatism takes, however, a strange turn when the target of the social scientist is a society that is viewed *from the outset* as backward or unjust or oppressive. For analysis will then make it appear, at least to start with, that the backwardness, injustice, and oppression are in reality far more deep-rooted than had been suspected. This is the origin of all the vicious-circle and vicious-personality theories that seem to make any change impossible in the absence of either revolution, highly competent central planning with massive injection of foreign aid, or massive abduction of the young generation so that it may be steeped elsewhere in creativity and achievement motivation.⁹ Interestingly enough, then, the same analytical turn of mind that leads to a conservative bias in the case of a society that we approach *without* a strong initial commitment to change, leads to a revolutionary or quasi-revolutionary stance in the case of societies that are viewed from the outset as unsatisfactory. In the case of the former, the analyst, like the ecologist, often becomes enamored of all the fine latent functions he uncovers, whereas in the latter case he despairs of the possibility of change (except for the most massive and revolutionary varieties) because of all the interlocking vicious circles he has come upon.

Fortunately these initial effects of social science analysis wear off after a while. In the case of the backward countries, the realization will dawn that certain so-called attributes of backwardness are not necessarily obstacles, but can be lived with and sometimes can be turned

⁹ It is only fair to note that, in his more recent work on achievement motivation, David McClelland has changed his earlier views on these matters. Thus he writes (after having given cogent reasons for doing so): "To us it is no longer a self-evident truth that it is easier to produce long-range personality transformations in young children than it is in adults." David C. McClelland and David G. Winter, *Motivating Economic Achievement* (New York 1969), 356.

into positive assets. I have elsewhere attempted to bring together the accumulating evidence for this sort of phenomenon.¹⁰ This evidence, then, should make us a bit wary when *new* vicious circles or *new* development-obstructing personality types or *new* deadends are being discovered. Though such discoveries are bound to occur and can be real contributions to understanding, they carry an obligation to look for ways in which they may play not a reinforcing but a neutral or debilitating role in so far as system maintenance is concerned. Perhaps social scientists could pass a rule, such as has long existed in the British Parliament, by which an M.P. proposing a new item of public expenditure must also indicate the additional revenue through which he expects the nation to finance it. Similarly it might be legislated by an assembly of social scientists that anyone who believes he has discovered a new obstacle to development is under an obligation to look for ways in which this obstacle can be overcome or can possibly be lived with or can, in certain circumstances, be transformed into a blessing in disguise.

IV

A related element of the cognitive style I am advocating derives from the recognition of one aspect of the unfolding of social events that makes prediction exceedingly difficult and contributes to that peculiar *open-endedness* of history that is the despair of the paradigm-obsessed social scientist. Situations in which the expertise of the social scientist is solicited frequently have the following structure: some new event or bundle of events such as industrialization, urbanization, rapid population growth, etc., has happened or is happening before our eyes, and we would like to know what its consequences are for a number of social and political system characteristics, such as integration of marginal or oppressed groups, loss of authority on the part of traditional elites, political stability or crisis, likely level of violence or of cultural achievement, and so on. Faced with the seemingly reasonable demand for enlightenment on the part of the layman and the policy-maker, and propelled also by his own curiosity, the social scientist now opens his paradigm-box to see how best to handle the job at hand. To his dismay, he then finds, *provided he looks carefully*, that he is faced with an embarrassment of riches: various available paradigms will produce radically different answers. The situation can be compared, in a rough way, with the quandary the forecasting economist has long experienced:

¹⁰ "Obstacles to Development: A Classification and a Quasi-Vanishing Act," *Economic Development and Cultural Change*, xiii (July 1965), 385-93.

the magnitudes that are of most interest to the policy-makers, such as the prospective deficit or surplus in the balance of payments or the budget, or the inflationary or deflationary gap, or the rate of unemployment, are usually—and maddeningly—*differences* between gross magnitudes. Hence even if the gross magnitudes are estimated with an acceptable margin of error, the estimate of the difference may be off by a very large percentage and may easily be even of the wrong sign. The hazards in forecasting qualitative social events on the basis of perfectly respectable and reliable paradigms can be rather similar. Take the question: what is the effect of industrialization and economic development on a society's propensity for civil war, or for external adventure, or for genocide, or for democracy? As with the effect, say, of accelerated growth on the balance of payments, the answer must be: it depends on the *balance* of the contending forces that are set in motion. Industrialization creates new tensions, but may allay old ones; it may divert the minds of the elite from external adventure while creating new capabilities for such adventure, and so forth. Thus the outcome is here also a *difference* whose estimate is necessarily subject to a particularly high degree of error. This ambiguous situation, incidentally, characterizes also less crucial, more “middle-range” causal relationships. An example is the effect of bigness and diversity of an organization on innovation. As James Q. Wilson has argued, bigness and diversity increase the probability that members will conceive of and propose major innovations; but they also increase the probability that any one innovation that is proposed will be turned down. Again the net effect is in doubt.¹¹

Wilson's dilemma is the sort of cognitive style in paradigmatic thinking that is not often met with; ordinarily social scientists are happy enough when they have gotten hold of *one* paradigm or line of causation. As a result, their guesses are often farther off the mark than those of the experienced politician whose intuition is more likely to take a variety of forces into account.

V

Finally, the ability of paradigmatic thinking to illuminate the paths of change is limited in yet another, perhaps more fundamental way. In the context of most Latin American societies, many of us are concerned with the bringing about of *large-scale* change to be carried

¹¹ James Q. Wilson, “Innovation in Organization: Notes Toward a Theory,” in James D. Thompson, ed., *Approaches to Organizational Design* (Pittsburgh 1966), 193-218.

through in a fairly brief period of time. But ordinarily the cards are stacked so much against the accomplishment of large-scale change that when it happens, be it a result of revolution or reform or some intermediate process, it is bound to be an unpredictable and nonrepeatable event, unpredictable because it took the very actors by surprise and non-repeatable because once the event has happened everybody is put on notice and precautions will be taken by various parties so that it won't happen again. The uniqueness and scientific opaqueness of the large-scale changes that occur when history "suddenly accelerates" have often been remarked upon. Womack brings them out as well as anyone in his narrative of the Mexican Revolution. I shall invoke the authority of two recent commentators belonging to rather different camps. The first is the anthropologist Max Gluckman, who addresses himself to "radical change" after having defended anthropology against the charge that it is not interested in change. He writes, "The source of radical change escapes these analyses [of other kinds of change]. Perhaps this is inevitable because social anthropology aims to be scientific. Scientific method cannot deal with unique complexes of many events. The accounts of the actual course of events which produce change therefore necessarily remain historical narratives. . . ." ¹²

Perhaps a more significant witness, because as a Marxist he should be an inveterate paradigm-lover, is Louis Althusser. In his remarkable essay, "Contradiction and Over-determination," Althusser makes much of some striking statements of Lenin's about the unique constellation of events that made possible the Russian Revolution of 1917. The key passage from Lenin is: "If the revolution has triumphed so rapidly it is exclusively because, as a result of a historical situation of extreme originality, a number of completely distinct currents, a number of totally heterogeneous class interests, and a number of completely opposite social and political tendencies have become fused with remarkable coherence." ¹³

On the basis of Lenin's testimony Althusser then proceeds to explain that revolutions never arise purely out of the basic economic contradictions that Marx stressed, but only when these contradictions are "fused" in some unique manner with a number of other determinants. This fusion or embedding is the phenomenon he calls "overdetermination" of revolutions. Actually this is a poor term (as he himself recognizes) for it could imply that, had one of the many circumstantial factors not been present, the revolution would still have taken place.

¹² *Politics, Law and Ritual in Tribal Society* (Oxford 1965), 286.

¹³ As quoted in Althusser, *Pour Marx* (Paris 1967), 98.

But the whole context of the essay, and certainly the quotations from Lenin, exclude this interpretation. On the contrary, it is quite clear that even with all these converging elements the revolution won by an exceedingly narrow margin. *Thus, while a surprising number of heterogeneous elements almost miraculously conspired to bring the revolution about, every single one of them was still absolutely indispensable to its success.* Uniqueness seems a better term for this phenomenon than overdetermination.

Incidentally, this interpretation of revolutions undermines the revolutionary's usual critique of the advocacy of reform. This critique is generally based on the *high degree of improbability* that a ruling group will ever tolerate or even connive at the elimination or destruction of its own privileges; the only way to achieve this end is by smashing the "system" through revolutionary assault. But with the view of revolutions as overdetermined or unique events, it turns out to be a toss-up which form of large-scale change is more unlikely—so we may as well be on the lookout for whatever rare openings in either direction appear on the horizon.

In sum, he who looks for large-scale social change must be possessed, with Kierkegaard, by "the passion for what is possible" rather than rely on what has been certified as probable by factor analysis.

This view of large-scale social change as a unique, nonrepeatable, and *ex ante* highly improbable complex of events is obviously damaging to the aspirations of anyone who would explain and predict these events through "laws of change." Once again, there is no denying that such "laws" or paradigms can have considerable utility. They are useful for the apprehending of many elements of the complex and often are stimuli to action before the event and indispensable devices for achieving a beginning of understanding after the event has happened. That is much but that is all. The architect of social change can never have a reliable blueprint. Not only is each house he builds different from any other that was built before, but it also necessarily uses new construction materials and even experiments with untested principles of stress and structure. Therefore what can be most usefully conveyed by the builders of one house is an understanding of the experience that made it at all possible to build under these trying circumstances. It is, I believe, in this spirit that Womack makes that, at first sight rather shocking, statement, "the truth of the revolution in Morelos is in the feeling of it." Perhaps he means not only the truth, but also the principal lesson.