

CHAPTER ONE  
CONCEPTUAL ANALYSIS AND RESEARCH DESIGN  
FOR POLITOLOGISTS

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First, an apology for yet another neologism. Political science and political scientists have long suffered from a misnomer. People (and even some mis-guided practitioners) assume when faced with the *appellation* (*non-controlée*) of political science that this discipline must be based upon the sort of formal reasoning, deductive logic, precise measurement, and reliable prediction that is characteristic of the physical sciences. It is not and can never be – given the nature of its subject matter. Anthropologists, sociologists and psychologists have opted for a more modest title that implies the objective of providing an understanding of the ‘*logos*’ of their field of inquiry, i.e. by reasoned reflexion and trying to persuade others of the value of their accumulated body of knowledge. Economists sound even more modest – even if they are often more pretentious about the ‘scientific’ nature of their endeavors.

It is time for English language political scientists to adopt a different label. Their French (*politologues*), Italian (*politologi*), Spanish and Portuguese (*políticos*) and Serbian (*politikolog*) colleagues have already shown the way. The psephologists and transitologists among them have also done so, why not the entire discipline?

In the following chapters, I have assembled a set of essays dealing with various dimensions of politology: a definition of the discipline, its micro-foundations, its use of the comparative method, its diverse research designs, its trans-Atlantic nature, its changing sources of inspiration and my own status as a conceptualist.

There is a fair amount of repetition across these essays and I have made no attempt to remove it.

And I have chosen to make them available gratis on the Internet to anyone who is interested. For corrections and suggestions, please contact me at [philippe.schmitter@eui.eu](mailto:philippe.schmitter@eui.eu).

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## Chapter Two

### The Discipline of Politology

Political science is the social science devoted to the systematic accumulation of knowledge about the exercise of power and its consequences. This capacity to get others to do what they would not prefer or be incapable of doing – even to make them want to do something else -- serves to justify its claim to possessing distinctive foundations as an academic discipline. But power is an intrinsically ambivalent phenomenon. It can be used to resolve or to exacerbate conflicts, to benefit society as a whole or a narrow group – even a single individual. It can be compelled to follow legally established and predictable channels or it can flow through illegal and informal, even unprecedented, ones. It can be exercised overtly and publicly through the institutions of government or covertly and privately underneath or alongside these institutions. Political scientists are supposed to contribute to an understanding of any and all of these dimensions. Their observations and findings are always potentially falsifiable by the work of other scholars and vulnerable to the indisputable fact that power is always embedded in diverse and changing social, temporal, and spatial contexts. Even its basic concepts of authority, legitimacy, influence, domination, distortion, manipulation, control, coercion, force, conflict, threat, collusion, hegemony, antagonism e così via can and have been challenged when applied by different scholars in different places and to different time periods.

Aristotle, a founder of political science, observed that human beings are natural "*Zoon Politikons*" (political beasts). By this he meant that they are destined to live and work together in communities that have to sustain a common identity and resolve common problems. In his time, these communities were small city-states. Since then, the context of political life and work has expanded enormously to encompass large nation-states, not to mention even larger multinational empires. With this dramatic change in scale has also come a shift in core assumptions. Most political scientists today would reject or, at least question, the singular emphasis on common identity and problem solving and insert an innate propensity for individuals and groups to compete with one another in efforts to assert their domination or to avoid exploitation by others. In other words, the modern *polis* rests on much less consensual and benevolent foundations.

The study of politics is a very ancient activity; the academic discipline of political science is much more recent. The ancient Greeks—Plato, Aristotle, Herodotus, and Thucydides—established its foundations more than twenty-five hundred years ago. They

were followed by the Romans—Polybius, Livy, Plutarch, and Cicero—who were particularly concerned with explaining the historical rise of the Roman Republic and Empire, as well as the characteristics of the proto-states that competed with them. Christian philosophers—such as Augustine, Marsilius of Padua, Nicholas of Cusa, and Althusius—explored issues involving the internal governance of the Roman Catholic Church and the moral relationship between religion and politics. The breakthrough to a more modern conception came with Niccolò Machiavelli. An acute and realistic observer of the turbulent politics of his time, he introduced a crucial element that persists to this day, namely, the notion of “agency.” Human political animals were credited (or debited, some would say) with the capacity to act not just in conformity to societal norms, religious conviction, or natural law, but also autonomously in efforts to change these norms, convictions, and laws—even to create a new political unit, which Machiavelli, for the first time, called “*uno stato*.” Moreover, in his new science of politics, necessity and fortune frequently required actors to cope with novel situations and to react in innovative and sometimes unethical ways—hence, his enduring reputation for advocating duplicitous behavior. Jean Bodin took this conception of the state one step further by inserting the element of sovereignty—namely, that this political unit should be capable of acting autonomously from all other sources of authority. Since then, most political scientists have treated the state as their primary unit of analysis.

During the seventeenth and eighteenth centuries, the Machiavellian notion of agency was taken for granted and expanded. Not only could states be founded by divine authority and embodied in a king or by the effort of an ambitious and ‘virtuous’ prince, but also, they could come about collectively by a “social contract” between rulers and ruled. Thomas Hobbes, John Locke, and Jean-Jacques Rousseau all contributed to this notion in different ways. Montesquieu revived the ancient effort to classify and compare political regimes and introduced the concept of separation of powers.

During the nineteenth century, treatises on politics proliferated as scholars converged on the topic from philosophy, law, history, and the new disciplines of anthropology and sociology. Most of these were of sweeping scope and had a strong historical-evolutionary bias: Benjamin Constant, Georg-Friedrich Hegel, Auguste Comte, Karl Marx, Herbert Spencer, J. S. Maine, Ferdinand Tönnies, Moises Ostrogorski, Gaetano Mosca, Vilfredo Pareto, Emile Durkheim, and Max Weber. They all contributed to forming the eventual discipline of political science, even if they were not aware of it at the time.

With the partial exception of Plato and Aristotle, with their respective academy and lyceum in Athens, none of the above would be called political scientists as the term is

applied today. They wrote as individual scholars trained in a variety of disciplines, not as members of specialized units dedicated professionally to writing and research about politics, and they did not teach students in this subject, although many of them played an active role in advising politicians, and some of them, most notably Machiavelli, were directly engaged in political negotiations.

In the early 1600s, Leiden University in the Netherlands, Uppsala University in Sweden, and Åbo Akademi, first in Sweden and later in Finland, created the first academic chairs specifically dedicated to politics, although these and other pioneering efforts were always coupled with another subject matter: history, philosophy, “eloquence/retorica” or law. In the German tradition, the emerging discipline was closely associated with the administrative, legal and economic demands of an emerging national state, hence, the appellations of *Staats-*, *Cameral-*, or *Policey-wissenschaft*. This remained the pattern for some time. The study of politics, or government, initially gained academic respectability by penetrating more-established scholarly disciplines. Even by the mid-nineteenth century, political science had yet to attain the status of an autonomous faculty or department with its own students and degrees anywhere in Europe—presumably, the reflection of a more formalized method for determining curriculum and greater resistance from a more rigid professorial hierarchy. An alternative route was to establish institutions for professional training outside the universities. The École Libre des Sciences Politiques in Paris, in 1871, seems to have been the precursor, followed by the London School of Economics and Political Science, in 1895; the École des Sciences Sociales et Politiques in Lausanne, in 1902; and the Deutsche Hochschule für Politik in Berlin, in 1920.

The roots of what became political science grew in Europe through centuries of cumulative effort by scholars and protagonists to make sense of the conflicts that surrounded them and the results that they experienced. It was neither a happy story nor an easy task. Then came a unique and more-promising opportunity: the American Revolution and the creation [I like the *ex novo* since it implies foundation not just newness] of a new democracy. Its founding fathers were probably the first to consider themselves students of “the science of politics,” as Alexander Hamilton claimed in *The Federalist Papers*, No. 9, and they were certainly the first to attempt to put it into practice. Thomas Jefferson even tried to establish a chair in this discipline at his newly created University of Virginia, but he failed to find a suitable occupant. Alexis de Tocqueville was strongly influenced by this tradition and famously proclaimed in the introduction to Volume One of his *De la Démocratie en Amérique* that he was going to produce “a new political science for a world itself quite

new." He did play an active role in the revived Academy of Moral and Political Sciences in Paris, in 1832, but his disciplinary contribution was recognized only much later.

A Chair of (History and) Political Science was first established at Columbia University in 1857, followed by the creation of departments at Cornell, in 1868; Johns Hopkins, in 1876; and Columbia, in 1880. By the turn of the century, the discipline of political science, without other accompanying disciplines, had spread to more universities in the East and Midwest of the United States. Many of its earliest practitioners had been trained in Europe, especially Germany and France, but very quickly its professors were receiving advanced "domestic" degrees. Many were actively engaged in the politics of the times, especially in the so-called Progressive Movement. One of them, Woodrow Wilson, even became the president of the country, in 1912. By the 1930s, the discipline had achieved full academic status in virtually all major American universities. The University of Chicago, under the leadership of Charles Merriam, became an especially prominent department during this period and played a role in the subsequent transformation of political science at other institutions by introducing elements from social psychology and methods for directly observing political behavior. Chicago and other American universities also benefited greatly from the influx of refugees from Naziism, mainly Jewish, who brought with them the academic and intellectual ferment of Weimar Germany and the new Austrian republic. Out of this combination grew the discipline's most enduring contribution to the practice of politics—the study of political opinion by random surveys of mass publics. There is hardly a polity today whose politicians do not use this as an instrument of governing. In some, it is even used by their opponents.

Some of the newly established faculties—e.g. Harvard, Cornell, Texas, Georgetown and Maryland—however, called themselves departments of government or of government and public administration, presumably because they wished to advertise themselves as professional schools preparing their graduates for employment in the civil service. This also reflected a strong ideological bias in liberalism toward channeling and containing all political activity within a set of formal public institutions—that is, a government. Today, political scientists routinely study the exercise of power in "non-governmental" and "non-public" institutions, even within churches, firms, families and, most recently, terrorist cells.

The study of relations between, rather than within, sovereign national states followed a different pattern of development. The first institution explicitly and entirely dedicated to international relations (IR) was the Institut de Hautes Études Internationales in Geneva, established in 1927 to train civil servants for the newly created League of Nations.

Chairs in this subject had been created earlier in Great Britain (University of Wales, London School of Economics) and in the United States (Georgetown University and the University of Chicago). Despite their common origin in the writings of Thucydides, IR has often claimed to be a discipline separate from political science – usually on the grounds of its concentration on the dichotomous extremes of the exercise of power, i.e. war and peace, and its interdisciplinary nature. This has led to a proliferation of specialized institutes, schools and academies, many of which are involved in professional training for national diplomats and policy advice to foreign offices. Elsewhere, IR was gradually inserted into the normal political science curriculum.

Not surprisingly, the United States, in 1903, was the first to create a professional association for the new discipline: the American Political Science Association. Its journal, the *American Political Science Review*, came shortly thereafter, in 1906. It took three decades before equivalent institutions were established in Australia and China, in 1932; Finland, in 1935; and India, in 1938. Only after World War II did other countries in Europe, Asia, and Latin America follow suit, creating national associations and journals on the American model. The capstone to this process of professionalization was the creation of the International Political Science Association in 1952, patronized by UNESCO with strong support from American practitioners. Today, IPSA has more than forty national associational members.

Of all of the social sciences that emerged into academic prominence in the early twentieth century, political science was the most Americanized. Bernard Crick was not exaggerating when he referred to it as *The American Science of Politics*. This has had its advantages and disadvantages. On the one hand, it served to anchor the discipline in an expanding system of higher education that produced some of the world's most respected universities, each of which had a department of political science. Competition among these units for faculty encouraged theoretical and methodological innovation and ensured their rapid diffusion throughout the discipline. Stimulated by the Cold War, which became the dominant feature of international politics after the Second World War, the American government, through the Fulbright Program and the private Ford and Rockefeller Foundations through their grants-in-aid, actively recruited foreigners to study politics in the United States and supported Americans studying it abroad. The explosion of "area studies" programs for Eastern Europe, the Soviet Union, Latin America, Africa, South Asia, East Asia, etc., during this period would have been unimaginable without the public funds provided by



the National Defense Education Act (NDEA), as well as dissertation grants from the Ford Foundation.

On the other hand, it was difficult to avoid the impression that political science had become a weapon of American academic imperialism and a threat to pre-existing programs in Western Europe, India, and Latin America that were anchored in the more traditional disciplines of moral philosophy, civil law, administrative law, or national history. Right from the start, Americans interpreted their discipline as “the science of democracy,” which meant that they took for granted many dimensions of politics that had long preoccupied scholars elsewhere. They initially had little to contribute to understanding dictatorship, coercion, illegitimate rule, absence of consensus, class conflict, foreign domination, or even the nature of the state itself. Moreover, most of them were convinced that liberal, representative, constitutionalized democracy as practiced in the United States, while it might have some minor imperfections, was the best regime in existence and the one that all others aspired (or should aspire) to imitate.

It took some time for scholars and students elsewhere to escape from the hegemony of this Americanized discipline (and for Americans to recognize the valid contributions of foreign practitioners), but today, political science has become a genuinely global discipline with theories, concepts, hypotheses, methods, and findings coming from all directions. A crucial intermediate step in this transformation was the creation of “regional” consortia whose purpose was often to bring together a critical mass of scholars to develop alternative approaches. The European Consortium for Political Research (ECPR) was founded in 1970, but it was preceded by efforts in Latin America: the Facultad Latinoamericana de Ciencias Sociales (FLACSO), in 1958, and the Consejo Latinoamericano de Ciencias Sociales (CLACSO), in 1967. Most recently, the Association Latinoamericana de Ciencia Política (ALACIP) was formed to emancipate political from the other social sciences in this world region. There also exists an African Association of Political Science (AASPS), since 1973, and an Asian Political & International Studies Association (APISA), since 2001. The government of the People’s Republic of China has indicated its intention to make political science a priority in university education, which probably means that by 2020, there will be more Chinese than American political scientists.

Micro-Foundations. Every science, whether physical or human,[this is an expression used to indicate a group of disciplines that includes and goes beyond the social science to include e.g. philosophy. I prefer to use it here] rests on a distinctive set of assumptions, and political science is no exception. These micro-foundations shape the way in which its

practitioners identify topics and transform them into subjects worthy of being taught, researched, and published. Normally, they are invisible and accepted without controversy. Political scientists, however, have persistently disagreed on what these are, even though these foundations all rest on the same accumulated wisdom of centuries of scholarship. The core of the problem rests with the very nature of its subject matter—power. Its exercise is elusive, but omnipresent, obvious when it involves force or coercion but much less visible when it focuses on manipulating preferences or invoking conformity to norms. Actors often pretend that they are not acting politically, and virtually everyone has an incentive not to admit what their true objectives are. Maddeningly, the most powerful actors often have to do nothing, because subordinates already have been either programmed to obey or convinced that it is in their best interest to do so – a condition known as hegemony. Power's consequences are always risky, but they are usually calculable. During periods of rapid change, however, they are fundamentally uncertain and, hence, incalculable. They are usually bundled together with allegedly “natural” social, cultural, or economic phenomena from which they are exceedingly difficult to disentangle. Of decisive importance as the discipline became more self-proclaimedly “scientific” is the fact that power is singularly difficult to measure, especially quantitatively. Experimentation, controlling for existing conditions and measuring precisely for the effect of deliberately introduced ones, is usually not possible, and even when it has been used, the results can be misleading. One of the most salient features of politics involves so-called “fallacies of composition.” What is true or workable at one level of aggregation produces very different results when practiced on a larger—or smaller—scale. Democratic individuals do not necessarily produce democratic regimes, and the inverse can be the case for autocratic ones. If, as Aristotle noted, a science should only “look for precision in each class of things just so far as the nature of the subject admits,” then political scientists face a more daunting task of being precise than any of the other social scientists.

Their science rests on five components, all of which are disputed: agents, motives, mechanisms, regimes, and units.

Agents. This is the most distinctive feature of a human, as opposed to a natural or physical, science. It begins with the assumption that the objects of research are also its subjects. In the case of politics, this means that agents can make relevant choices that are not completely determined by the conditions in which they find themselves. It also implies that the subjects have the capacity for reflexivity. They are historical in the sense that their present actions are influenced by reflections (“memories”) from the past, and, hence, by

learning that they may alter their responses when faced with similar situations in the present. Inversely, they can be anchored in habits (“standard operating procedures”) that can be difficult to break when new opportunities appear. Moreover, the very process of researching the power relations among actors—past or present—can produce changes in the behavior or expectations of the agents being studied.

The vast majority of political science researchers presume that these agents are individual and autonomous human beings faced with and capable of making choices among alternative and consequential actions. They may agree that these actors are uniquely capable of exerting political agency, but they differ considerably about the properties that humans are capable of bringing to bear on their choices. Recently, thanks to the wholesale importation of assumptions from neo-liberal economics, these individuals are supposed to have pre-established and relatively fixed preferences, to be able to rank these preferences consistently, to possess adequate information about alternative courses of action and theories about their effects, to predictably choose the ones that they think best realize those preferences at the lowest cost, and to still have the same preferences once the consequences of their choices have been experienced. This generic conception also reflects the much deeper ideological commitment of modern social and political thought to liberal individualism and rational progress. Shifting to a different micro-foundation would seem to be declaring that politics is a “passionate” activity rooted in raw emotion, blind faith, mindless imitation, instinctual tradition, collective stupidity and/or random events—and, hence, incapable of collectively improving the world that we live in.

Without going so far, there are two grounds for calling this individualistic foundation into question. The first has to do with the sheer complexity and contingency that surrounds contemporary human beings. They cannot possibly know what are the “real” (or, even less likely, all of the available) alternatives and what all of their eventual consequences will be. Moreover, individuals are very likely to discover upon reflection that they have many conflicting interests or passions—especially over different time horizons— and, hence, cannot rank them consistently. If those reasons were not enough, they typically are acting within a multilayered and polycentric set of institutions capable of making binding collective decisions, some public and some private. All of which implies that agent preferences cannot be fixed but are always contingent on which policies are being proposed and by whom, and they will probably change during the course of political exchange among the various layers and centers of power.

The second reason for resetting the micro-foundations of political science is even more subversive of the prevailing orthodoxy. What if most of the significant actors were permanent organizations, not individuals? Granted that these organizations comprise individuals, and some of them may depend very closely on the contributions and allegiance of these persons, but many do not and have developed elaborate rules and sources of support that cannot be reduced to such individual actions. They embody collective choices made long ago and have acquired a reputation and legitimacy of their own. And, not infrequently, these political parties, interest associations, social movements, non-governmental organizations, business firms, government agencies, and private foundations are in the business of teaching individuals what their preferences should be and committing individuals to obeying policies made in their name.

Motives. Establishing who the agents are does not tell us what is driving their political actions. Again, contemporary political science has its orthodox response: self-interest. Individual political agents can invariably be relied upon to choose that alternative that best satisfies their own and highest-ranked preference at the lowest cost and without reference to anyone outside of themselves or their immediate family. Historically, analysts stressed more noble and other-regarding motives, such as the esteem of one's peers, the honor of the community, ethical commitment, personal glory, religious conviction, conformity to tradition, and even justice and fairness. The emergence and eventual dominance of capitalism demonstrated the enormous advantage in pursuing one's own interest in economic transactions, so why not the same for political transactions? The stage was set for the wholesale and uncritical importation of the rhetoric and logic of market competition into the analysis of modern politics.

The most obvious objection to this assumption is that human beings also have passions, and these have not been completely eliminated by capitalism. Individuals still can care about others and even about the whole society or political unit in which they live. Without belonging to some sort of meaningful collective identity, they would find it impossible to identify or act upon their individual interests, because most political actions affect not just a person but also a social, economic, or cultural group. In order to belong to that group, and especially to convince others to act with them, individuals have to appear "reasonable," that is, to be capable of justifying their preferences. They have to behave in an "appropriate" fashion—to conform to pre-established collective identities and norms that may seem highly routinized and codified by tradition but that nonetheless are rooted in

the omnipresent passion for expressing oneself, for belonging, and for being admired, and these institutions can severely limit the unbridled pursuit of self-interest.

If one switches to organizations as the principal actors, at least in the more developed polities of Western Europe and North America, the political scientist's task is greatly facilitated. By their very nature, these more or less permanent collectivities have internal processes for dealing with the diverse motives of their members and followers, and for coming up with a mediated expression of their interests and passions that is publicly justifiable and normatively appropriate. In other words, they have become institutions, valued in their own right. Granted, there is plenty of room for dissimulation, strategic action, gender discrimination and outright hypocrisy on their part, but revealing these will be facilitated by the more abundant and public nature of the information that such institutions are compelled to provide.

<H2>Mechanisms.</H2> By and large, the mantra of the discipline is *competition*. Agents exercise their relative power by competing with one another in order to better satisfy their respective interests or passions. This usually presumes the existence of a pre-established institutional context in which conflicting motives are channeled by mutually respected rules into a process that limits the use of power resources and the range of possible outcomes—that is, the existence of a government and state. Otherwise, the agents would engage in unruly conflict, not bound by such constraints, and they would exercise their power primarily by threatening or exercising violence to impose their interests or passions. The American science of politics was literally built upon the presumption that the rules would be constitutional in form and democratic in process. After World War II, these rules were consolidated in the polities of continental Europe, but only as late as the mid-1970s in Portugal, Spain and Greece. Elsewhere—in Latin America, Africa, and Asia—only recently and selectively has democratization domesticated and pacified the exercise of power to the mutual benefit of the agents involved.

The major distortion within the discipline comes when political scientists assume that electoral competition is the major, or even exclusive, expression of this process. The fact that political parties compete with one another for the representation of territorial constituencies and the right to form governments—even when these elections are freely and fairly conducted, and their outcomes are uncertain—does not exhaust the mechanisms whereby political agents compete with one another over “the authoritative allocation of values” (David Easton if you need the cite: *A Framework for Political Analysis* (Englewood Cliffs: Prentice-Hall, 1965)). Not surprisingly, these other mechanisms are populated less with

individuals than with organizations: competition among interest associations to influence public policy, prosecution of politicians for violating legal norms by law firms or public-interest groups, demonstrations by social movements to set the public agenda or to block the implementation of policies, and revelations by rival media companies to discredit or support the reputation of rulers. All of these are important (and often highly institutionalized) features of modern politics that deserve at least as much attention as the more sporadic and routinized conduct of electoral competition.

Another mechanism also deserves a more prominent place in the micro-foundations of political science—namely, cooperation. If competition is not to degenerate into conflict, political agents have first to cooperate by agreeing upon the rules, formal or informal, that limit and channel their use of power. Many of these are habits or strictures inherited from previous generations (“path dependence”), but they are continuously subject to challenges as power relations and the identity of actors change and therefore require re-affirmation by contemporary agents. Moreover, politicians also cooperate in order to ally with one another, both to modify the pre-existing rules of engagement and to affect present policy outcomes. While it is understandable that political science should privilege competition, if only because its presence is much more visible and consequential, cooperation deserves more status and attention than it usually receives. So does its perverse form, collusion, when inside agents act in agreement to prevent outsiders from competing through the usual mechanisms.

Regimes. Most professors and students of contemporary politics assume that the unit they are analyzing has a relatively stable configuration of institutions that are complementary to each other—that is, a regime, presumably as the result of a prior historical experience of searching among alternatives and eliminating incompatible ones through competition. The actions produced by its agents, motives, and mechanisms are somehow—functionally, ideationally, intentionally, or constitutionally—related to one another at a higher level, such that their nature or importance cannot just be assessed alone. They are embedded in an institutionalized whole that conditions what roles can be played by individuals or organizations, self- or other-regarding interests, and competitive or cooperative mechanisms. These regimes are given labels, and it is presumed that those in the same generic category will share many foundational elements. At one time, there were three such labels: democratic, totalitarian, and authoritarian or autocratic. More recently, the middle one has dropped out, thanks to the collapse of the Soviet Union, and been replaced with “*hybrid*” or some other diminutive version of democracy or augmentative version of autocracy. Needless to say, each of these can be broken down further by the

analyst into subtypes when exploring the performance of more specific agents, motives, or mechanisms.

The implications of this intrusion of “regimes” into the micro-foundations of the discipline are considerable, if still debatable. For one thing, the recognition of such diversity means giving up the quest for universalistic “covering laws” that can be applied to any agent, motive, or mechanism. Individuals or organizations do not behave the same way in democracies and autocracies; the “reasonableness” and “appropriateness” of interests or passions depends on the institutions to which they are addressed; mechanisms such as competitive elections or cooperative multiparty alliances can take on different meanings depending on their complimentary relationships with other arrangements for competition/conflict or cooperation/collusion.

Units. Ever since Aristotle collected the constitutions of one hundred fifty-eight Greek city-states, the privileged unit in political science for both observation and analysis was supposed to have a relatively autonomous economy, a self-governing polity, and a distinctive collective identity—all institutionalized and coinciding with one another in a given territory. Eventually, thanks to the evolution of European polities and their overseas empires, this unit became the sovereign national state. It is usually presumed that only within it are agents capable of making choices and implementing them effectively, individuals or organizations capable of calculating their interests and passions, mechanisms of competition and cooperation capable of operating, and most regimes capable of developing their stable and complimentary institutions. Nothing is more firmly rooted in the micro-foundations of political science than this assumption. Virtually every existing hypothesis or observation in the discipline should be prefaced with the phrase: “Take an existing national state and, only then will X be related to Y in the following manner.” What if this unit of analysis can no longer be taken for granted, however? What if that presumed coincidence among autonomy, capacity, and identity has been disrupted beyond repair? In the contemporary world, no political unit can realistically connect cause and effect and produce intended results, without regard for the actions of others. Virtually all of these units have persons and organizations within their borders that have identities, loyalties, and interests that overlap with persons and organizations in other polities. Nor can one be assured that polities with the same formal political status or level of aggregation will have the same capacity for agency. Depending on their insertion into multilayered systems of production, distribution, and governance, their capacity to act or react independently to any specific opportunity or challenge can vary enormously. This is most obviously the case for

those national states that have entered into supra-national arrangements, such as the European Union (EU), or signed binding international treaties, such as those of the International Monetary Fund (IMF) or the World Trade Organization (WTO). Not only do they occasionally find themselves publicly shamed or found guilty by such organizations, but also they regularly anticipate such constraints and alter their behavior accordingly. If that were not enough, many national polities have recently granted or been forced to concede extensive powers to their sub-national units and, in some cases, these provinces, cantons, *regioni*, *estados autónomos*, or *départments* have even entered into cooperative arrangements with equivalent units in neighboring national states. Political scientists need to dedicate much more thought to the units they choose and the properties these units supposedly share with regard to the specific agent, motive, mechanism, or regime that they are examining. There still remains a great deal of variation that can be explained only by conditions prevailing at the national level, but exorcising or ignoring the increasingly complex external context in which these units are embedded can result in a serious analytical distortion.

Conclusion. Aristotle famously argued that political science was the “master science,” because all of the other human sciences depend upon the order or disorder produced by politics. Ironically, this assertion of its superiority has also been a source of weakness. Political science is condemned to be an “open science.” It reaches into and affects crucial aspects of other realms of human behavior and is, therefore, bound to be penetrated by assumptions and concepts coming from them. Law, philosophy, sociology, psychology, and, especially, economics all have claimed to be more closed and, therefore, self-referential sciences. Each of them has attempted to penetrate the deepest micro-foundations of political science. At times, this has threatened to deprive the discipline of its distinctive focus on the use of power and its conversion (sometimes) into legitimate authority to resolve conflicts and achieve collective purposes. In the twentieth and early twenty-first centuries, attempts were made to reduce the study of politics to the voluntary exchange of information, the joint product of individual opinions, the rational search for optimal institutions, or the deliberative discourse among consenting persons or organizations. This may have served to illuminate some of its peripheral aspects and to expand its scope of inquiry, but these efforts have all floundered when trying to explain situations in which an actor or actors can “get others to do what they would otherwise not do”-- [Robert Dahl, *Who Governs?* (New Haven: Yale University Press) whether by deploying superior resources or by manipulating the preferences of subordinates. When some person (and, even more, when some established public or private organization) can alter the



distribution of information, manipulate the attitudes of individuals, restrict the range of “acceptable” institutions, or distort the course of deliberations, the outcome becomes different—often radically different—from the one envisaged by any of these models. If political scientists were to narrow their research agendas to situations in which these conditions were absent, not only would they be depriving their discipline of the cornerstone of its foundations, but they would also become incapable of providing useful knowledge to the politicians, representatives, citizens, and subjects who have to cope with the very real existence of power and its consequences.

The science of politics has had to track and incorporate continuous changes in the practice of politics, usually in response to successive crises and always with some delay. Its foundations are contestable, its methods are varied, and its findings are contingent, but it has never completely lost its scientific aspiration. It seeks to conceptualize its subjects in explicit terms drawn from centuries of theorizing; it attempts to systematically gather the required data, quantitatively or qualitatively, as the case may be; it tries to examine these data for patterns of association; and it draws reasoned inferences from them about causality, co-dependence, or just plain coincidence. As messy and tentative (not to mention controversial) as this effort may be, it has produced a dynamic academic discipline capable of attracting more and more students, professors, researchers and—yes—critics. The skills it teaches have also proven useful in an expanding variety of public and private occupations, but the greatest mark of its success has been its globalization. Innovations in concepts, methods, and even basic assumptions now come from the South and the East and not just the North and the West. Political science has yet to acquire recognition as the “master” science, but it has definitely become a global science.

## CHAPTER THREE

### The Design of Social and Political Research

Let us assume that you have an **idea** that has led you to identify a **topic** that you believe to be of sufficient importance and of feasible execution to conduct research on it. It may be a doctoral dissertation, or just a seminar exercise, but regardless of length and complexity no topic can ‘research itself.’ You will have to translate it – *via* a series of strategic choices – into a **project**. It is this process of translation from something problematic or puzzling into something that you can gather valid data on and make compelling inferences about that constitutes your **research design**.

Granted, much social scientific research is not self-consciously designed – it is not subject to a deliberate and critical process of choosing its components and defending its overall configuration. In many areas of inquiry, the design is literally given along with the topic. So much research has already been conducted on it that adding yet another case or extending it to yet another time period does not seem to require a novel effort of translation. Indeed, the universal desire of all sciences to produce cumulative knowledge seems to militate against continuously challenging and changing the standard way of doing research. If you do propose a change in design – say, a re-conceptualization of the topic, a revised instrument for measuring variation, a different way of selecting relevant cases, or a novel method for testing for association – you will risk confusing your reader-*cum*-critic. He or she may find it difficult to distinguish whether eventual differences in data or inference are ‘really’ due to the topic itself or ‘merely’ to your meddling with the established way of researching it.

Most young social scientists, however, will not be choosing topics whose research design is given. They will have to find or invent an apposite design – and they should be prepared to understand and defend the choices involved. Moreover, if their immediate or eventual intent is comparative – if they anticipate including more than one case or set of observations and drawing inferences across them – then their choice of design will be even more crucial. Making the right strategic choices will greatly enhance the value of the data they gather and the inferences they can draw from them; neglecting these choices or taking them for granted could result in idiosyncratic scraps of information and inferences rooted in exceptional circumstances that make no reliable or cumulative contribution to scientific knowledge.

[Figure 14.1 about here]

Figure 14.1 is a schematic and idealized representation of the complete ‘social-political research cycle’. Each of its boxes involves an important set of interrelated strategic choices, and its implication is that these should be made in the displayed sequence, beginning with an idea that defines a topic at 12 noon and proceeding clockwise until the researcher arrives at an evaluation of his or her findings that may or may not redefine the original topic at midnight. Inside the boxes lie a number of alternative courses of action. Choose among them wisely, and you will do better research. Ignore them or fail to grasp their significance, and you risk committing serious fallacies at each stage.

The most important message to keep in mind while proceeding through the entire cycle is that there is no single best strategy or set of strategies for researching all topics. Everything depends on your point of departure, the initial substance you

have decided to research. At the beginning of the cycle in Figure 14.1, the range of options tends to be most extensive – and, hence, most confusing. Interesting topics clamour for equal attention; different theories and concepts can seem equally compelling. As one proceeds clockwise, the successive choices are increasingly related to each other and the options become more limited. At some point, you may well adopt or fall into an ‘established disciplinary routine’. You can save yourself a lot of time and worry by doing this but this will only benefit you if your topic and, especially, its conceptualization is sufficiently isomorphic with the original – that is, it conforms to the basic characteristics of the topic that has already been successfully researched by others. Applying even the best established and/or most fashionable design to the wrong topic can be a formula for disaster, especially when it comes to drawing inferences.

Very few researchers really enter Figure 14.1 at Noon and leave at Midnight. Most take shortcuts to get started in the process. Many social scientists begin their research careers already knowing which case or cases they intend to work on. Not infrequently, it happens to be the country they come from or are trained in. So-called ‘area specialists’ usually have some prior personal commitment involving their knowledge of history, culture or language, and this tends to affect the topics they select. Others may have picked up some novel statistical technique or measurement device that they wish to show off – and they search about for an apposite topic to which to apply it. Perhaps the most common (and, in my view, pernicious) point of departure concerns theories or approaches that are currently fashionable in sociology or political science. Imbued with the conviction that only those espousing such a ‘paradigm’ will find eventual employment, young researchers are prepared to take up

any topic – no matter how trivial or obscure – if only to demonstrate their fidelity to its assumptions and postulates.

Do not presume that, once in the cycle, you will have to go all the way around. As we shall see in the conclusion, there are many points of exit that will still permit you to make an original and significant contribution to knowledge.

Wherever you have really begun your research and whatever your motives for doing so, I recommend that you at least pretend that you are beginning at the top of Figure 14.1, if only to help you clarify *ex post* the design choices you should have taken deliberately or have already taken implicitly. Try to imagine that it all began with an idea about a substantive topic that is important to you and that only later did you place it in an explicitly conceptual context, elaborate specific hypotheses about its occurrence, choose the cases to study, and so forth.

**Now, we can proceed to look sequentially into the ‘little black boxes’ in Figure 14.1. Attached to each of them the reader will also find a list of ‘possibly optimizing’ choices and ‘potentially damaging’ fallacies.**

**Choice of Topic:** No one can predict where and when ideas will appear. With some knowledge of the researcher’s personal and professional background, however, it may be a bit easier to predict the conditions under which an idea becomes a topic – that is, when someone will attach sufficient importance to a given thought and place significant boundaries around it to make it worthy of investing his or her energy to explain how it came about or what its consequences might be. This highly personal effort at selection can be an important source of distortion throughout the rest of the design and, especially, when it comes to drawing inferences from whatever data distributions or associations are generated. The very fact that you care enough to select some topic probably means that you also value what it contributes or the effect that it has. However subliminal the

thought may be, your values become embedded in the topic and can exert a persistent influence on your choices as you make your way around the rest of the research cycle. They may have an even greater impact when you decide to make a ‘premature’ exit from the cycle.

It is often the case that one is attracted to a topic because the society or polity also cares about it. Never is this more evident than when the subject matter is in crisis or in fashion. As social scientists, we are attracted to phenomena that call attention to themselves – whether they do so by creating further problems or by providing novel solutions. Which is another way of saying that our topics tend to be either failed experiences at the end of their useful existence or recent successes that have yet to reveal their complete impact. Rarely does one come across designs explicitly focused on explaining social or political phenomena that are mediocre or inconsequential.

*Grosso modo*, topics of research come in two guises: (1) **projections**, where the researcher is confident that the existing approach and methods are adequate and deserve to be applied to units or time-periods that have not already been covered or with greater precision to cases that only seem to be exceptional; and (2) **puzzles**, where the researcher begins with the assumption that something is deficient in the way that the topic has been previously handled and that the units or time-periods to be examined will demonstrate the existence of anomalies. Both projections and puzzles should be approached in the same ‘critically rational’ manner, but the perspective of the researcher differs. If the topic selected is regarded as a projection, he or she has the intent (at least, initially) of confirming established wisdom and will take more seriously the obligation to make a cumulative contribution to knowledge within a specific discipline or paradigm. The perspective when tackling a puzzle leads one to seize on anomalies that seem to expose deficiencies in how the topic has been

conceptualized, measured or reported, and that is more likely to lead the researcher to alternative concepts and methods – frequently by drawing on other disciplines.

Needless to say, both are capable of making valid contributions; both are needed by all social science disciplines.

### *Possibly Optimizing Choices*

1. Choose a topic that you care enough about to be willing to spend the time to complete the project.
2. Choose a topic (and make an argument) that interests other social scientists (even those outside your field); the better it is, the more it will interest those working in adjacent fields and disciplines.
3. Specify the temporal, spatial and, if necessary, cultural boundaries of the topic in a way that makes the research feasible, but does not make it trivial or ‘unique’.
4. Acknowledge your initial source of inspiration for the topic and your personal preference about its outcome, without apologizing for them.
5. Never justify your selection only on the grounds that it has been ‘under-explored’, and, especially, do not ignore, trivialize or dismiss what has already been written on the topic.
6. Try to reach as far back as possible in social and political theory to find grounds for the relevance of your topic and avoid being manipulated by academic fad and fashion.
7. By all means, listen to your advisor and your peers, but be absolutely certain that, regardless of who first suggested it, the topic ‘belongs’ to you.

### *Potentially Dangerous Fallacies:*

1. 'Fad-ism': Your topic (or method or theory) is being very much and very favourably discussed right now in your field, so that if you adopt it your work will be less criticized and you will be more likely to find a job.
2. 'Wishful Thinking': Your topic has already produced well-publicized and promising results for the society or polity; therefore, if you conduct research on it, your findings will be taken more seriously and favourably.
3. 'Ambulance-Chasing': Because the topic of your research is presently in crisis, you will have greater access to data and the public will be more interested in whatever you find out.
4. 'Presentism': Is the wrong assumption that whatever you find associated with some topic in the present must have been there in the past and will probably remain there in the future.
5. 'Standing on the Shoulders of the Past Giants': This might apparently allow you to see further and to avoid being distracted by the squabbles among contemporary pygmies – yet those Giants might not have been looking at the same thing or in the same direction.

### **Conceptualization**

Almost all substantive matters emerge 'pre-conceptualized' in the strict sense that they can only be recognized by the potential researcher and shared with others if they are expressed in some intelligible language. The idea may come initially as a shape or a colour or an emotion, but **words** are the indispensable way in which it acquires factual specificity and shared significance. The complication for research resides in the high probability that the words initially involved will be those of the social or



political actors involved – which implies that their words could bear many different meanings and be attached to a wide range of contrasting assumptions.

Conceptualizing a topic involves translating the words that surround it in ‘real-existing’ societies or polities into **variables** (although see Della Porta’s and Bray’s chapters). These are not just fancy academic labels applied to a specific event or process. They should identify analogies, generic conditions that are shared by a distinctive set of events or processes and can take on different values over time – whether these are quantitatively or qualitatively observed. They acquire their peculiar status as causes or effects according to the way they are connected to other variables by theories. Once these variables have been assembled, whether from the same or varying theories, they constitute your provisional **argument** concerning the topic you have chosen to explain.

Which brings us to ‘the Elephant-in-the-Room’ that is so rarely mentioned but so frequently the source of confusion at each stage of designing research. Even the most elementary and frequently used concepts – such class, status, gender, age, region and religiosity for explaining voting behaviour – derive their meanings from being inserted into a more comprehensive (and presumably coherent) matrix of concepts (see Kratochwil’s and Mair’s chapters). Their definitions may sound the same and, as we shall see later, operationalization of these variables may even be identical, but their role depends on prior assumptions and contingent relations that differ according to the theory, paradigm, approach or framework that is being applied. And no single piece of research can possibly specify what these are. If you tried to do this, there would be no time or space left for your analysis. In other words, all social and political research is part and parcel of ‘the state of theory’ prevailing at the moment it is conducted. No research can be conceptualized *ex novo* without reference to what

has been produced already on that and related topics. This applies just as much to those who are trying to solve puzzles as to those who are ‘merely’ trying to make projections.

Choosing one’s concepts is only the first step. Making them into variables means assigning a status to them, and this is where their embeddedness in theory most saliently enters into the research design. The most important task is to distinguish between those that are regarded as **operative** with regard to the chosen topic and those that are **inoperative**. The former are expected to play some discernable role in the explanation of outcomes – either as an *explicans* (that which does the explaining) or as an *explicandum* (that which is to be explained). The more elaborate the prior theory and, hence, the conceptualization derived from it, the more it may be possible to assign different statuses to the operative variables, for example, by distinguishing between primary and secondary ones (according to their explanatory power), direct and intervening ones (according to how near the effect is to the cause), continuous and episodic ones (according to how constant in time their effect is), and so forth. Needless to say, all these initially assigned roles can be inverted, especially where and when the objective is to explain a relatively long-term sequence of social or political processes. **Inoperatives** are variables that are present and can be expected to take on different values during the subject matter being researched, but whose effect is not expected to produce a discernable or significant difference. Of course, when it comes to making eventual inferences, allegedly inoperative variables may turn out to be an important potential source of spuriousness. Even **constants**, variables that were present but not thought to vary during the research period and, hence, *a priori* considered not capable of contributing to variation in the outcome, may gain eventually in importance – especially when it becomes evident that the impact of

operative variables was contingent on slight modifications or even simple reinterpretations of such background factors. Hopefully, **irrelevant** variables – those whose variation cannot conceivably be logically or empirically associated with the topic under investigation – will remain that way.

### *Possibly Optimizing Choices*

1. As much as possible, avoid references to specific persons, countries or cultures with ‘Upper-Case’ names by using only ‘Lower-Case’ variables to describe them and their prospective effects in your argument.
2. There is nothing wrong with using a ‘hunch’ as your starting point in conceptualization – the world surrounding most interesting topics is usually full of them – but try as soon as possible to identify the more generic theory in which this hunch is embedded, switch to its language, and explore its axioms or presumptions before going further.
3. Try to avoid ‘multi-colinearity’ – clusters of variables that are closely associated with each other – and simplify by only using the dominant variable in such clusters or providing it with an ideal-type connotation that captures as precisely as possible the nature of the cluster.
4. Make as explicit as possible not only the operative but also the inoperative variables and the constants, those characteristics that do not vary, in your argument – and be prepared to change their status in the course of conducting the research.
5. When using classification systems (see Mair’s chapter), make sure that the categories are both inclusive of all observations and exclusive in their assignment of every single observation – and that all of them are potentially

relevant to explaining outcomes, including those that are vacant for the moment.

6. Specify as soon and as explicitly as possible the universe to which your conceptualization is intended to apply in both time and space.
7. Exercise caution when using concepts and variables across long periods of time or different cultural contexts, since their meaning to actors and, hence, their effect may change.
8. Strive for parsimony by eliminating double-dealing or superfluous variables, but without resorting to excessive simplification. One way of doing this is to restate your argument several times and to make it more concise each time.

#### *Potentially Dangerous Fallacies*

1. ‘Obscurantism’: If you cloak your conceptualization in highly abstract terms or fit all of your observations into some complicated classification scheme, no one will notice that all you are doing is describing what happened.
2. ‘Attribute-ism’: The more definitional attributes or analogous properties you attach to a given concept, the more significant it is likely to be in explaining the outcomes you want to understand.
3. ‘Concept Stretching’: A concept used successfully to identify an analogy among events in one time and place must be equally valid when applied to other times or places.
4. ‘Isolation’: Your preferred variable plays such an important role in explaining your topic that it can be conceptualized, measured and manipulated alone, without regard for the network of other variables surrounding it and the prior axioms upon which it rests.

5. 'Novelty at any Price': Because existing concepts are so embedded in (old) theories, by inventing and using novel ones, you will be credited with greater originality in your research.
6. 'Arbitrariness': Since all concepts are basically arbitrary – a function of unpredictable practical uses and/or theoretical fashions – it will make no difference which ones you use, provided that your public and peers come to accept them.
7. 'Consensual-ism': If everyone in your discipline is using some concept and seems to agree on its meaning, as well as its explanatory relevance, you should feel safe to do so.

### **Formation of Hypotheses**

Not all research designs involve the formation (or the testing) of explicit hypotheses. There exists a very broad range of social and political topics for which it is possible to conceptualize the variables that may contribute to an explication, but not to assign any sort of provisional 'IF ... THEN ...' status to their relationships. For these topics, the apposite research logic is one of **discovery** and not of **proof**. The purpose is to improve one's conceptualization of a topic, probe its plausibility against a range of data and eventually generate hypotheses among its conclusions, but it would be premature to expect them as a pre-condition for conducting the research itself.

The determining factor is again that 'Elephant-in-the-Room', the prevailing state of theory on a given topic. Substantive matters that are of recent occurrence, that are only characteristic of a small number of cases, that incite strong emotions or political controversies, or that fall between different social science disciplines are obvious candidates for 'discovery' status. The potential researcher is reminded that

this should not be taken as a sign of inferiority. Somewhere behind all social scientific research that today routinely follows the logic of proof, there must have been a glorious moment in the past when someone launched a voyage of discovery. Unfortunately, behind the façade of increased professionalism and standardization of techniques, this message has been suppressed. Only the most intrepid of young scholars will accept the challenge of trying to make sense out of alternative conceptualizations of the same topic; or piecing together potentially coherent and general arguments by ‘process tracing’ on the basis of specific cases; or admitting that, in instances of highly interdependent and complex social or political systems, it may never be possible to distinguish between independent and dependent variables, much less to express them in terms of a finite set of bi-variant relationships.

#### *Possibly Optimizing Choices*

1. Ensure that the assumption of any ‘IF ...THEN ...’ relationship is sufficiently precise so that it specifies its ‘micro-foundation,’ the functional dependence, structural mechanism or intentional logic that is supposed to connect its variables and, where possible, introduce an independent measure of its presence.
2. Do not assume *ex ante* that only individual human beings are capable of laying ‘micro-foundations’, when the ‘real-existing’, historical world is chock full of social and political units that have acquired the capacity to act collectively in ways that cannot be reduced to individual intentions and choices.
3. Ensure that the presumed cause is independent of the presumed effect, and not parallel or convergent manifestations of the same social or political process.

4. Where possible, specify explicitly the existence of intervening conditions or prevailing constants that must be always present for the hypothesized relation to produce its effect – even if these contextual factors do not vary during the research.
5. An ideal research situation can emerge when you find yourself in a ‘two-ring circus’ – when two rival versions of the same hypothesized relation are plausible and would explain diametrically different outcomes based on different theoretical assumptions.
6. Be prepared to recognize and deal with ‘equifinalities’, similar outcomes that are produced by different sequences or mechanisms, when they emerge, and therefore to test different sets or, better, ‘strings’ of hypotheses – not just isolated ones.
7. Remember that you always have three hypotheses to test, namely, the ones that suggest a positive or a negative relation, and the null hypothesis that no ‘IF ...THEN ...’ relationship exists. The latter should be regarded as the most probable in occurrence. Everything may be related to everything else in our complex environment, but not always in a predictable direction or to a significant degree.
8. Try *ex ante* when elaborating hypotheses to differentiate between variables that you think are ‘necessary’ (always likely to be present when the outcome is present), ‘sufficient’ (always and only present) and merely ‘helpful’ (sometimes present; sometimes not). Never assume that your set of variables is going to be both ‘necessary and sufficient’ and, therefore, make space for the inevitable ‘error term’.

9. Since most research projects consist of ‘clusters’ and ‘chains’ of related hypotheses that contribute to explaining a selected outcome, it is often useful to draft a ‘model’ of these simultaneous and sequential relations using time and space as co-ordinates.

### *Potentially Dangerous Fallacies*

1. ‘Scientism’: If your variables are not organized into hypotheses with clearly differentiated independent and dependent variables, your research will not be scientific.
2. ‘Fear of Failure’: If your hypothesis or hypotheses are disproved, you will have made no contribution to knowledge.
3. ‘Infinite Regress’: All hypotheses about variable relationships in the social sciences are preceded by a potentially infinite historical chain of causality and consequence, therefore, it makes no difference when you choose to break into that chain.

### **Selection of Cases**

For all but a few projects, the potential number of societies or polities affected by the chosen topic will exceed the researchers’ capability for gathering data, testing for associations and drawing inferences. It is, therefore, normal that only some subset of these units will enter into your analysis. One of the most prominent of the strategic choices you will have to make involves the number and the identity of those to be included and the criteria you impose to select them. This can vary from one unit (the single case or person) to as many as are apposite (the universe of those affected); but there is a fairly unescapable trade-off between the quantity of variables that have been included in your initial conception of the topic and the number of units for which you



will be able to gather data. Including more cases probably also means poorer quality data, more missing observations and greater problems of conceptual equivalence. Inversely, the more narrowly you have defined and operationalized those variables – that is, the lower they are on the ladder of abstraction – the less likely they are to be relevant in a wide range of cases.

Case selection may have its practical side when it comes to gathering data and, especially, making one's own detailed observations; but its real payoff is analytical. Manipulating the identity of cases provides most sociologists and political scientists with their closest equivalent to experimentation. It 'simulates' the introduction of control variables. By 'holding constant' across the sample such potentially relevant conditions as cultural identity, geographic location, level of development and temporal proximity, the researcher can at least pretend that variation in them is unlikely to have produced the outcome one is looking at. Granted that the controls can be a bit approximate and that there still will remain many potential sources of 'contaminating' differentiation among units in the sample – still, this is the best design instrument that he or she has available. It should, therefore, be wielded with deliberation – and caution.

Strictly speaking, the researcher does not select individual cases but 'configurations of variables' that co-habit the same unit and may even co-vary in a unique or distinctive fashion within that unit. But one cannot analyze 'France' as such and compare it with, say, 'Spain' or 'Italy.' There are simply too many different (and potentially relevant) conditions within each of these countries with regard to almost any topic you chose to work on. This holds even when comparing micro-units within the same country, where the number of variables can be more reliably controlled due to common constraints at the nation-state level. So-called 'holistic' research is,

therefore, largely an illusion in social and political research and, when tried, it usually amounts to little more than a detailed or ‘thick’ description of one case (or of parallel ones if more units are covered).

Which is not to say that there are not significant differences between designs that are driven by the effort to isolate a small number of variables and test exclusively for their association with other variables across a larger number of units, and designs that begin with a large number of interrelated variables (often combined *via* ideal type constructs) within one country and then seek to find significant and persistent connections across a few, carefully selected units of an allegedly comparable nature. But in either strategy, what you are usually comparing are variables – one or many, alone or in clusters – not units.

Which bring us to the second aspect of case selection, which has long been taken for granted and yet has recently become of growing concern. For a unit of observation to be a valid case for analysis, it must possess identical or, at least, comparable degrees of freedom with regard to the topic under investigation. A design that drew inferences – descriptive or causal – from a sample of units composed of Brazilian municipalities, Mongolian provinces, Spanish *comunidades autónomas* and the permanent members of the United Nations Security Council about the efficacy of taxation systems would not attract much attention. Much as its author might (correctly) protest that this ‘sample’ embodies a ‘most different systems design’, critics would (rightly) object that actors in these units did not have remotely equivalent powers to make or enforce their decisions about taxation.

The usual formula for getting around this problem was to select only units that were at the same level of aggregation and enjoyed the same formal status within the world social and political system. This presumably explains why so many

comparative research projects have been based on nation-state units or, to a lesser degree, on relatively autonomous sub-national units within federal or confederal systems. The *reductio ad absurdum* of this strategy has been reached with large-N comparisons containing all the members of the UN for which data can be obtained – despite the blatant fact that these so-called sovereign states have radically divergent capabilities for governing their respective populations or even satisfying their most elementary needs.

I have little more to add. I will, however, insert a pedagogic device that I have found usefully in explaining to students what their options are at this point in the research cycle.

[Figure 14.2 about here]

Where researchers are committed to producing scientific knowledge (defined here as causal inference), the preferred case selection strategy should usually be the experimental one, choosing the units of observation randomly and introducing some element of change in a subset of them while holding variation constant for the others. Unfortunately, most social and political scientists have to operate in ‘real-existing’ settings, where this is not possible. And even when they are permitted to engage in experimentation, the topics tend to be so trivial and the settings so artificial that projecting inferences based on such findings to more ‘realistic’ contexts is very hazardous.

Quasi-experiments may be second best, but they offer some interesting advantages, with regard to both the efficiency of research and the credibility of inferences. The case-base can be as low as one, although it is better to replicate the quasi-experiment in several other settings, if possible, within the same time frame. They are, however, limited to real world situations where the independent variable is highly discrete and temporally circumscribed and where data-gathering over a sufficient period of time has been consistent and reliable. Assessing the effect of a new public policy or the impact of some unexpected social or natural event tends to fit this narrow bill of particulars, but only if nothing else is happening to the unit or units at the same time. This is also a strategy of case selection that is especially vulnerable to diffusion or contagion effects, if the units involved know of each other’s behaviour.

Most sociologists and political scientists will have to settle for the study of variations in their subject matter that appear ‘naturally’, whether within a single case or across different numbers of them..

There are, I would add, a number of other alternative strategies that are not usually included in texts on research methods or design – presumably because their

scientific status is dubious. They typically arise in contexts in which it is risky or impossible to observe and record the behaviour of ‘real-existing’ social or political units. All involve what Max Weber once called ‘thought experiments’. The best known goes under the rubric of **counter-factualism** and involves the researcher in an effort to imagine what would have happened to the topic if some condition, person, event or process had not been present. Usually, this focuses on a single country – for example, How would Germany have evolved politically if Hitler had not been ‘available’ in the early 1930s. It can also be applied to a sample or even to the universe of cases – for example, What would be today the level of international insecurity in Europe if the EU did not exist? Or, how many people in the world would know how to speak English if the Americans had lost their Revolutionary War? If this sounds ‘exotic’ and a bit ‘flaky’, you should remember that every time that you invoke the famous and indispensable Latin phrase, *ceteris paribus*, before advancing a hypothesis, you are being a counter-factualist.

Moving even further from social scientific orthodoxy, one finds a vast number of seminar exercises, MA papers and PhD dissertations that are essentially rhetorical, theoretical or normative. These certainly deal with topics – often more important ones for ‘real-existing’ societies and polities than those chosen by empiricists-*cum*-positivists – but their purpose is to follow the development of concepts or discourses over time, or to examine the logical consistency of particular arguments, or to promote the ethical acceptance of specific forms of human behaviour. Such projects do indeed involve research. In a ‘heuristic’ and ‘nomothetic’ way they have influenced empirical inquiry. Just think of the impact of recent works by John Rawls, Jürgen Habermas and Jon Elster upon how even the most ‘hardcore’ empiricists select and conceptualize a wide range of topics.

### *Possibly Optimizing Choices*

1. If you are not trying to cover the entire universe, consider the possibility of selecting a sample of cases randomly and how that would affect your project.
2. As a rule, when randomization is excluded and you have to be purposively selective, choose your cases based on their relation to the independent variable or variables rather than the dependent variable or variables.
3. And when you make this choice, try to ensure that the cases chosen ‘represent’ as wide a range as possible of scores on those independent variables.
4. When your topic will not permit this, when you are motivated to research something precisely because it involves a compelling, arresting or extreme outcome and you therefore have to select on the dependent variable, remember this as a potential source of bias when it comes to drawing inferences.
5. Keep in mind that you do not have to use only one strategy of case selection and that so-called ‘Nested Strategies’, where you start with a large N of cases and relatively few crudely measured variables and, subsequently, shift to a small N with a much more detailed ‘battery’ of variables – can give you the advantages of both strategies when it comes to drawing inferences.
6. Always prefer the lowest level of spatial or functional aggregation that is compatible with the actor behaviour presumed by your conceptualization, since you can subsequently reassemble your research upwards – but not downwards – in scale.

7. No matter which or how many cases you initially select, some may prove to be ‘decomposable’, in that you may be able to generate additional cases by dividing up the initial ones, but only provided these subunits possess some and the same degree of autonomy.
8. Before selecting the number and identity of the cases for which you intend to gather data, make sure that you are aware of the criteria that you originally used for classifying your topic and ask first: ‘What is this a case of?’ Only after satisfying that demand will you know what units are ‘eligible’ for inclusion, and you can proceed to exclude some of them for good reason.

#### *Potentially Dangerous Fallacies*

1. ‘Notoriety’: Just because a given case has been prominent in public discussion, it will be more interesting to research, and others will pay more attention to your research.
2. ‘Numbers’: It is always advantageous to have a larger number of cases, even when, by adding them, you are compelled to attenuate their relation to the topic or to use less valid indicators.
3. ‘Cruciality’: Because a given unit is an outlier according to your criteria of case selection, it will be a crucial case whose conformity or non-conformity provides a definitively significant test of causal association.
4. ‘The Illusion of Control’: Selecting cases because they seem to share certain general cultural, locational or structural characteristics necessarily controls for their relevance – when it is still possible that minor or qualitative variations in ‘controlled variables’ could be affecting variation in what you are trying to explain.

5. 'Contemporaneousness': In units chosen for comparison within the same time frame, the actors must have similar (or at least sufficient) awareness of the relevance of common variables and be capable of acting upon them simultaneously – when these units may be at different points in longer cycles or simply on different time schedules.
6. 'Imitation': When actors in the selected units are acutely aware of having to deal with some topic within the same time frame, they will also be sensitive to what others are doing about it and will learn from each other's successes and failures – in fact they may be quite ignorant of what each other is doing.

### **Writing the Proposal**

This stage in the research cycle is 'optional,' although highly desirable. Different graduate programs place greatly different emphasis on the importance of defending a formal proposal. Some require it before allowing the candidate to 'go into the field'. My personal experience suggests that the greater the plurality of approaches or paradigms surrounding a given topic and present in a particular institution, the greater will be the emphasis on writing and defending your proposal. In scholastic contexts dominated by a single theoretical or disciplinary orientation, the effort may be eschewed completely. The reigning orthodoxy favours problems rather than puzzles and may even dictate in considerable detail how topics should be conceptualized and operationalized. At the extreme, there is no 'field' to go to, no specific cases to select and no measurement details to discuss. What matters at this stage is the normative or logical consistency of the 'argument', of one's conceptualization of the topic and how well it conforms to prevailing orthodoxy. The number and identity of cases are relatively unimportant, if not irrelevant, to the extent that both prior axioms and



subsequent expectations are believed to be universal. The data can be simulated or assembled from the usual sources for illustrative purposes. The eventual inferences are usually predictable and in line with original expectations. The fellow members of your 'research club' will enthusiastically congratulate you on your cumulative contribution to knowledge. Practitioners of other disciplines and members of other clubs within your discipline will yawn and tell you that you have 'rediscovered the wheel' or produced something utterly trivial. In other words, there are costs as well as benefits in belonging to an established research tradition.

Another condition affecting the utility of proposal writing is its potentially critical role in obtaining research funding. Where such support is assured or not subject to competitive pressures, the researcher may content him- or herself with a brief statement of intention. Otherwise, your ability to summarize coherently and justify convincingly the design choices that you have made up to this point could make all of the difference in determining whether you will be able to carry out your project at all. Although it is not frequently discussed openly, this 'commercial' aspect of proposal writing can also be a source of distortion when the preferences of the sponsor come to be anticipated in the proposal itself and the researcher finds him- or herself pandering to them by modifying the topic, changing its conceptualization, restricting the range of hypotheses and even selecting different cases in an effort to please the prospective sponsor. More experienced researchers soon learn how to 'fine tune' their proposals to get support from donors and then go on to follow the course of inquiry they think will lead to the most compelling inferences. Fortunately, national or supra-national sponsors rarely control for conformity between proposals and the research actually carried out. At most, they may be interested in whether or not the policy implications drawn from such research conform to their preferences.

The ‘real’ purpose of writing a proposal should be give the researcher a chance to sit back and reflect critically on the strategic choices he or she has made – and to exchange these reflections with supervisors and peers before plunging into the inevitably messy and absorbing process of gathering data and trying to make sense out of them. There may be subsequent moments for self-criticism and changes – see the remarks below on the importance of **serendipity** – but writing and defending a proposal at this stage offers a unique opportunity to ‘rewrite’ and ‘resubmit’ before becoming irrevocably locked into a course of action.

### **Operationalization of Variables**

In principle, the conceptualization of variables should be carried out beforehand and without regard for how they will be converted into indicators and eventually measured. There is a good reason for this. What is of paramount theoretical importance is to specify clearly the condition or factor that is supposed to be present in order to produce some anticipated effect – alone or in conjunction with other variables. Having previously and independently conceptualized the projection or puzzle in such a fashion should provide a strong incentive subsequently to specify the observations that need to be made in order to verify the presence, magnitude, direction, or persistence of that variable. During the early stages of research, this means that you should adopt the attitude that all social and political variables can potentially be operationalized – and later be prepared to compromise when you start looking for indicators in the real world.

In practice, unfortunately, anticipations of such difficulties do tend to intrude and can even inhibit scholars from using concepts that are known to be ‘impossible’ to operationalize. Just think of such indispensable political properties as power,

authority and legitimacy; or of such social ones as esteem, respect and trust. For none of them is there a standard and easily accessible set of measures. Even elaborate (and expensive) attempts to operationalize them based on ‘reputational’ criteria from public opinion surveys have been problematic. And criticisms of these efforts become more insistent the more such indicators are stretched across countries and over time.

Another way of putting this dilemma is that there are bound to be **trade-offs** that have to be made at this stage in the research cycle. The higher one’s concepts are on the ladder of abstraction – and, presumably, the wider their prospective range of application – the more difficult it is going to be to make convincing observations about their presence in a specific case or set of cases. Increase the number of units in your study – either of persons or of organizations – and you are almost bound to run into problems with missing data and misleading indicators. Do not be afraid to make these trade-offs, but do so self-consciously. Tell your reader-*cum*-critic when you are settling for a less satisfactory indicator or a less specific level of observation. Be prepared when necessary even to eliminate cases, but also be sensitive to how this may distort your eventual capacity to draw inferences. Those research sites where operational requirements are most difficult to satisfy are usually places where social and political behaviour is the least ‘normal’ and their exclusion from the design will probably narrow the range of variation and reduce the eventual strength of association.

The theme that haunts all aspects of this stage of the research cycle is **validity**. Do the observations you propose to make accurately reflect and, hence, capture the meaning of the concepts you have chosen to bear the burden of explanation? No matter how accurate the observations, how comparable they are across units, how replicable they turn out to be when another scholar makes them; if they are not valid,

your research will have broken down at one of its most vulnerable points. You may well have discovered something important and the associations revealed by your indicators might be incontrovertible, but you have not proved (or disproved) what you started out with. Your findings are irrelevant in the strictest sense. They have told you nothing about the topic you announced that you intended to work on – unless you are prepared to rely on serendipity (see below) and re-conceptualize your entire project from its very origins.

### *Possibly Optimizing Choices*

1. Pay close and critical attention to the correspondence between your initial concepts and their proposed indicators or assessments by comparing them to research by others on the same or related topics.
2. Be wary of variable specifications and of empirical indicators that have been applied routinely over time and across units to measure different concepts.
3. Make sure that the concept and its indicator(s) are applied to the same level of analysis and are as close as possible in level of abstraction.
4. When available, use alternative operationalizations and multiple potential indicators and, where necessary, rely on ‘triangulation’ among them to resolve disparities and to improve validity.
5. All things being equal (and they never are), you are better off using unobtrusive rather than obtrusive indicators, since the actors whose behaviour is being observed will have less of an opportunity to respond strategically, ethically or emotionally to your request for information.
6. Remember that there are various ways of assessing the validity of indicators, ranging from consensus among independent respondents to co-

variation between different ‘internal’ measures and, least reliably, correlation with other hypothesized ‘external’ outcomes.

### *Potentially Dangerous Fallacies*

1. ‘Availability’: This indicator exists and has been used successfully by others; therefore, it must be valid when applied to your topic.
2. ‘Operationalism’: You decide to include in your analysis only variables for which you know that a valid (or consensually accepted) indicator already exists.
3. ‘Mimetism’: ‘X’ got away with using data on this to indicate a concept similar to yours, even when drawing upon a different theory; therefore, you can safely use it for the same purpose.
4. ‘Ignorance of the Uncertainty Principle’: If you operationalize a variable by intruding on the ‘real-existing’ world of your respondent, you can nonetheless ignore the possibility that his or her answer will be contaminated by prevailing norms of correctness or strategic calculations of interest, or that you will be creating rather than measuring variation.

### **Measurement**

At this point in the cycle, your choices will be more-or-less dictated by the ones you have already made – whether you did so consciously in relation to the specificity of your problem or puzzle (as I hope was the case) or whether you settled into an established research tradition – whether quantitative or qualitative – and obediently followed its dictates. Moreover, there is a good reason why you should let yourself ‘go with the flow’ at this point. Using existing techniques of observation and indicators for variables not only saves you a lot of time and anxiety, but it also can

provide you with an element of internal ‘quality control’ – provided that the measures used are valid, that is they capture the characteristics of the variable that you are relying upon for an eventual explanation. When it occurs, successful replication of previous research is a very desirable result – and one that can be personally very reassuring. Should you decide to invent and apply a new indicator or, worse, battery of indicators – especially to measure some frequently used variable – you will have to make an especially strong effort at justification. Otherwise, you will run the risk at the inference stage of confounding the reader: Is this seemingly compelling finding really novel, or is it only due to some change in measurement?

The discussion on measurement tends to be dominated by the distinction between quantitative and qualitative indicators – with a marked bias in favour of the former. There is no reason to be surprised by this, since most methods texts are written by quantifiers and they have convincing arguments in their favour. Numerical data are said to be more **reliable**, more likely to provide agreement among independent observers, more **accurate**, more likely to produce agreement across units, and more **useful**, more compatible with different ways of testing for association. Certainly, the social science disciplines have tended to assign greater ‘scientific status’ to quantitative over qualitative research – and to reward its practitioners accordingly.

This is unfortunate for at least three good reasons: (1) it has encouraged researchers to attach numbers to variables when the validity of their connection with the designated concept was dubious; (2) it has resulted in the exploitation of standard numerical indicators whose multiple components are often theoretically disputable and whose weighted combinations are poorly understood by those who use them; and (3) it has discouraged the innovative use of more direct and imaginative techniques of

observation – precisely to capture qualities inherent in complex and contingent relations. You can assign a number to anyone and anything; but nothing guarantees that the assignment will produce relevant information. If these qualities are differences in kind (nominal) rather than in magnitude (cardinal or ordinal), then – whatever the rule of their assignment – the number could well be a worthless piece of dis-information. What matters is how you have conceptualized your topic, not the allegedly superior virtues of one over another form of measurement.

Of all of the stages in the cycle, this is probably the one that is best suited for **serendipity**, for learning from the research process itself in ways that can feed back to your previous choices and lead you to introduce improvements in them before ‘path dependence’ has completely taken over. At last, you are back in touch with the ‘real-existing’ subjects/agents of your topic – having spent much time wandering around making abstract ‘disciplinary’ decisions. If you are lucky, they will talk to you directly about their intentions and perceptions, and they may even have some opinions about what you are asking them and intend to do with their answers. Even if your research relies exclusively on secondary or publicly available sources, there can be ‘voices’ in such documents that can speak in ways you have not anticipated. Of course, there will be a lot of sheer ‘noise’ generated by the data you are gathering, and that can be very confusing when juxtaposed to the relatively parsimonious approach you have been applying to the topic. Nevertheless, keep your eyes, ears and mind open for subtleties and surprises, and be amenable to introducing ‘course corrections’ – even some that go all the way back to the boundaries you initially placed around the topic or key aspects of your original argument.

### *Potentially Optimizing Choices*

1. Routinely test for the reliability of indicators, if possible by using alternative sources of data and/or alternative persons to score the data.
2. If validity requirements can be satisfied, opt for quantitative over qualitative measurement, since the technical advantages are considerable and because you can more easily move from the former to the latter.
3. Always opt for the highest, most informative level of measurement possible (given the nature of the variable), since it will later be possible to shift to a lower level. Cardinal data can always be made ordinal, and virtually anything can later be dichotomized or filed away in nominal categories – but you cannot move in the inverse direction.
4. Make your instructions – even if only for your own use – concerning the assignment of quantitative scores or qualitative labels as transparent and complete as possible so that the measurement operations can be replicated by you or someone else in the future.
5. Especially when working on the macro-level of a complex society or polity, most variables will contain multiple components and be indicated by composite measures – which should obligate the researcher to devote concerted attention to how such ‘scales’ are aggregated.
6. Especially when gathering information over time about social or political processes, make sure to check that changes are not due to modifications of the instruments of observation rather than to changes in actual behaviour.
7. Many measurement devices are calibrated to pick up only relatively large-scale and consequential changes in variables, which means that they may systematically fail to capture more modest and gradual ones. Social and



political ‘revolutions’ are always recognized; ‘reforms’ are more often under-reported, until their effects have accumulated sufficiently to draw attention to them.

8. Try to estimate before actually gathering the data where the error sources are most likely to come from and how they will affect your findings. Worry less about random errors (they will attenuate possible associations) than about systematic ones (they will bias the direction of your findings).
9. Try to catch yourself before adjusting the data or correcting for errors in it in ways that make these data fit better the general expectations or specific hypotheses you started with.

#### *Possibly Dangerous Fallacies*

1. ‘Composite-ness’: Many concepts are complex and multi-dimensional in nature and, therefore, can only be measured by similarly complex and multi-dimensional indicators – regardless of variation in their internal structures and, hence, the probability that identical scores will be assigned to quite different clusters of variation.
2. ‘Longevity’: It is always better to use an indicator that has been around for some time, used in a variety of research settings, and can provide the researcher with a longer time perspective – despite the likelihood that during this period the techniques for measurement will have changed and the meanings of items for actors may not be the same.
3. ‘Clarity’: It is always preferable that each variable be given a specific and unambiguous score – even if the nature of its conceptualization and theoretical status is calculatedly ‘fuzzy’ or ‘radial’.

4. 'Reification': What you are measuring is identical to what you have conceptualized which, in turn, is identical to the way in which actors perceive 'it' – regardless of how much is lost in translation as the researcher moves from one realm to another.

### **Test for Association**

By now, the researcher may have momentarily lost almost all strategic control over his or her project and, at best, should consult one among many texts on methodology to discover which among all of the verbal or mathematical, symbolic or numerical, parametric or non-parametric, deterministic or probabilistic devices available for testing for association best fits the data that he or she has gathered.

Variables can be associated with each other in different ways. Typically, the social scientist will be interested in **direction**, or whether the fit is positive, negative or null; **strength**, or how much one variable affects another; and **significance**, or the likelihood that the fit could simply have been due to chance. Since his or her research will almost inevitably be 'historical,' the **time**, **timing** and **sequence** of how they fit to each other should also be important – indeed, these chronological dimensions often provide the basic orientation to how one's findings are presented and defended.

The reason for this is that the most powerful means of testing for the fit among variables and, therefore, for presenting one's findings has long been to tell a believable story in chronological order. Perhaps, within some highly professionalized niches in sociology and political science, storytelling is no longer regarded as acceptable. The occupants of these niches – not infrequently, Americans or those trained in America – have forgotten that their disciplines are profoundly and irrevocably historical. What counts is not just what happens, but when it does and in

relation to what else has already happened or is simultaneously happening. Moreover, the actors themselves are not just passive recipients of scores, but active and reflexive keepers of the score. They remember what they and their ancestors did in the past, and their preferences in the present are conditioned by this knowledge. In my opinion, no means of testing for such associations has yet been invented that can supplant or even surpass the chronological narrative in capturing these subtleties of time and timing, and in bringing simultaneously into focus the multitude of variables involved in the sheer complexity of most social and political phenomena. The narration of your findings can, no doubt, be considerably bolstered in credibility by inserting quantitative tests about specific associations into the basic narrative. Cross-tabulations, rank-orderings, regression equations, factor- or small-space analyses, even mathematical models can often be helpful, but primarily when analyzing topics that are heavily circumscribed in time and space and that can be separated into relative simple and repetitive components.

Even social and political scientists relying exclusively on quantitative data may find it occasionally useful to come up with a plausible story that places the associations they calculate and the inferences they draw in some chronological order. Narration can also serve to fill in the gaps between cause and effect by providing a verbal description of the mechanisms involved – especially when mathematic formulae and formal models typically treat such exchanges as taking place within impenetrable ‘black boxes’. The findings of hardcore quantifiers often circulate only among small groups of *conoscenti* and are incomprehensible to outsiders; but whenever sociologists or political scientists aspire to enlighten and influence wider publics, they will either have to learn how to narrate their findings or hire someone else to translate the esoteric results of their tests into more intelligible stories.

### *Potentially Optimizing Choices*

1. Never forget the 'Inter-Ocular Impact Test' that consists in simply eyeballing the data – scatterplots are especially useful for this – and forming your own visual impression of what is going on among the variables and across the cases.
2. Always try to apply different tests of fit and only try the more demanding ones once you have experimented with simpler ones.
3. If possible (and it will be much more possible with quantitative designs), manipulate the number by eliminating one or two, and/or by dividing the sample into sub-samples – say, by size or location – and do not be discouraged if this shakes up their fit, but try to discover what variables may have intervened to produce such different results.
4. Remember that most tests for association – quantitative for sure, qualitative for some – are exceedingly sensitive to extreme cases, so that you may be well advised to eliminate them in order to find out how persistent or significant is the association among variables when only more 'normal' units are included in the analysis.
5. Remind yourself of the time dimension and test whether successive cross-sections through the data – say, at ten year intervals – produce equally strong associations. If they do not, reflect on what intervening or contextual variables might be responsible for the new findings.
6. Your tests for association will be all the more convincing, the more effort you put into falsifying initial hypotheses, rather than merely seeming to verify them by grasping at all favourable distributions of data.

7. The treatment of ‘deviant’ cases that do not fit the general pattern of association is often taken as an indicator of how seriously the researcher accepts the task of falsification. Ignoring them (or transforming their scores) suggests that you are excessively concerned with verification; embrace them, exploit their contrariness, try to determine the extent to which they call into question the hypothesis and you will gain favour as a ‘falsificationist’.

### *Possibly Dangerous Fallacies*

1. ‘Spuriousness’: You have found a close association between two variables and you report this finding – without considering that if you were to introduce a third variable, it might explain variation in both of the original ones.
2. ‘Contingency’: The associations you find are strong and significant, but only if and when certain, usually unspecified, contextual variables are present.
3. ‘Curve-Fitting’: Since there is always ‘noise’ and ‘error’ in the data, it is permissible to ‘smooth’ distributions by transforming the raw data or eliminating outliers and this will usually result in a more ‘satisfactory’ fit.
4. ‘Anachronism’: Whatever are the associations that satisfy your test criteria and the time period covered by your research, the findings they generate will be valid whenever.
5. ‘Ad-Hocracy’: At some level of abstraction and measurement, each case can be uniquely identified and used to ‘explain away’ any and all observed deviations from the outcome predicted by those variables included in the study.

## Causal Inference

This is by far the most hazardous – and the most rewarding – of the stages in the research cycle. It is the one in which you will have the least disciplinary or academic guidance and, hence, the widest range of discretionary choices to make.

Many social researchers will have exited the process before arriving here. They will have made their accurate observations, published their empirical descriptions and gone home. Others will have stopped even earlier, before having gathered any data, and left satisfied that they have advanced further the plausibility of an argument or helped to specify the universe to which it can be applied. Some will have gone further and proffered tests – numerical and narrative – illustrating how frequently and strongly variables have been associated with each other. But they will have prudently refrained from trying to answer the two further questions: (1) the retrospective one of **why** and **how** these variables combined to produce the outcome that was the topic of the research in the first place; and (2) the prospective one of **what** will be the consequences of this in the future and **when** will these consequences happen?

Consider, as an example, the current controversies over climate research. Do you think that, if climatologists and other scientists had merely filed reports demonstrating that temperatures have been rising across the planet and that various chemical substances have been accumulating in its atmosphere, there would have been much of a reaction? As far as I know, these facts have been accepted by all as uncontroversial. It was only when these researchers correlated these indicators and drew the inference that increases in them masked a causal relation that could not be due to chance or fate that things began to get stirred up. When they attributed primary causation to factors related to human intervention and, even more, when they began to

advance threatening projections about what will happen in the future, then, all Hell broke loose!

Without even hinting that all social scientists have a responsibility for generating such controversy, they should feel a more modest responsibility for exploiting their data to the fullest extent possible; that almost inevitably commits them to drawing retrospective and (sometimes) prospective inferences. Just think back to the number of occasions when you have read a report on extensive and expensive research and still found yourself asking the ‘why’ and ‘how’ question at the end. Granted that this could be regarded as favourable by younger researchers, since it means that there is a very considerable amount of unexploited data out there just waiting for ‘secondary analysis’ at low cost. Nevertheless, it is lamentable when the scholars who initially chose the topic, conceptualized it, selected the cases, and gathered the data do not go as far as they could in drawing ‘grounded’ inferences about the causality it might reveal. Manuals for sociology and political science are full of sage advice concerning the limits of doing this. Not infrequently, teachers of graduate courses and dissertation advisors will revel in providing the student with egregious examples of researchers who exceeded the confines of their data or ignored the contribution of other variables, and made what proved to be erroneous statements about causality or consequence.

The controversy that tends to dog most discussions about inference is **generalizability**. A cautious researcher who draws inferences from his or her findings that are restricted to the cases investigated and the time-period covered is less likely to face much criticism – or to generate much attention. Specialists on the topic will, no doubt, have something to say about the validity of indicators, the accuracy of measurements and the appropriateness of tests for association – but it is not until you

dare to generalize across temporal, spatial or cultural contexts, until you trample on some else's turf, that you will be seriously challenged. No one likes to be told that his or her topic can be differently explained by someone intervening from another theoretical or disciplinary perspective.

And there are good reasons for this. Although they may seem arbitrary or anachronistic (and some no doubt are), the lines of specialization built into different social science disciplines have served to enforce professional standards and preside over the accumulation of knowledge. Generalizations that are based on alternative conceptualizations and/or novel methods should be especially carefully scrutinized. Nevertheless, this is where the real scholarly excitement lies – this is where 'seminal' contributions are to be made – provided the researcher is well prepared to face his or her critics.

The strategy of case selection will play an especially significant role. Single case studies are rarely a convincing basis for generalization – even the so-called 'crucial' ones. Large-N studies should be less objectionable, were it not for the fact that many of their cases are dubious in terms of their (alleged) common capacity to act and the probability that behind any associations found in the whole universe there are bound to be sub-sets of cases where the fit differs considerably – and may even reverse itself. Middle-size samples based on controlling for the 'usual suspects' (geographic location, development, size, religion, cultural area) by their very nature inhibit generalization, unless they are replicated for different samples. Indeed, replication can be a powerful weapon – and not just to the extent that other cases or periods produce the same direction, magnitude and significance of association. If you can show that a reliable pattern holds at different levels of aggregation within the



same sample, you will have added considerable compellingness to the inference that it is more likely to hold elsewhere.

The other critical factor will come from accusations of researcher bias, often alleged to be the product the national or disciplinary context in which the researcher operates. It is only human to prefer to discover what you thought was there in the first place and, then, to extend that finding to other places you know less about. Most often this can be attributed to a natural tendency to ‘over-observe’ what you expected to see and to ‘under-observe’ variation that you were less prepared to encounter. Along with this ‘Type One Confirmation Bias’, one has to mention that Type Two Errors also exist. In this instance, for some perverse reason, the researcher prefers to reject his or her original hypothesis and, thereby, underestimates the degree of association that actually exists. Whether the peculiarities of national cultures or academic disciplines have anything to do with either of these typical errors seems dubious to me, but there is no doubt that both exist.

The most secure way of guaranteeing enduring respect for the inferences you have drawn from your research – and of securing your place in the Pantheon of Notable Social Scientists – is to place them under the protection of a ‘Covering Law’. This law offers an explanation for a much broader range of social or political phenomena, for example the Darwinian ‘Law of the Fittest.’ It should be widely, if not universally, accepted by the Notables who have preceded you and, ideally, it should not be derived from the theory you started with. But do not worry if you do not make it to the Pantheon. Your contribution to knowledge can still be significant and your career as a social scientist still very rewarding.

### *Possibly Optimizing Choices*

1. Add alternative explanatory variables suggested by other cases or experiences (if available without conducting an entirely new piece of research) to discover whether the original fit within your sample holds up.
2. Probe your data by subtracting subsets of cases in the initial sample to see how robust the findings based on it were, especially when you think you are dealing with the entire universe but have reason to suspect ‘regional’ variations.
3. Be careful not to ‘anchor’ your inferences by relying too much on a single prominent association among variables at the expense of lesser (and less expected) ones.
4. When assembling a batch of inferences from a research project, do not privilege or attach greater significance to findings that were easier to document or closer to your own experience.
5. It will be risky, but try on the basis of your inferences from a given sample to predict what analogous behaviours have been in a different sample of persons or places that you know nothing about; and (even more risky) to apply the inferences you have drawn to predicting the future performance of the units you have studied.

### *Potentially Dangerous Fallacies*

1. ‘Triumphalism’: You have discovered a significant finding; therefore, your work is over – even though it could be the result of some variable you forgot to include and that may be very prominent in other cases or samples.
2. ‘Pago-Pago-ism’: Whenever you think you have found something that applies everywhere, there will always be some place that you do not know (or

not even have heard of) where the finding does not fit – and there will always be a scholar who knows the place and will inform you of your error.

3. ‘Exceptionalism’: You chose to study a particular topic only in a particular country because you considered that the context was exceptional and, then, you turn around and claim that the findings you have observed are universal.
4. ‘Cross-level Replicability’: Associations among variables that have been found to be consistent in direction, strong in magnitude and significant at one level of analysis will replicate themselves at other – lower or higher – levels of aggregation within the same sample.
5. ‘Cognitive Dissidence’: If variables that simply ‘should’ not go together still seem to be associated, this must be due either to some unidentified measurement error or conceptual confusion so that you are justified when drawing any inference by excluding the case or withdrawing the variable from your analysis.
6. ‘Temporal Proximity’: You choose to give greater prominence and to infer more importance to associations of variables that have occurred more recently and to presume that earlier associations (or dissociations) should be ‘discounted’.

### **Evaluation of Results**

Once you have arrived at whatever stage in the research cycle you have chosen as your point of exit, your objective should be quite simple: make yourself into the best possible critic of your own work. Anticipate all of the potential objections at each of the previous stages. Where possible, return and enter appropriate corrections. Since this is often impossible, given the numerous and irreversible ‘path dependencies’ built

into the research cycle, signal to your reader-*cum*-critic that you are aware of the defect and have tried your best not to be misled by it or to magnify its impact. Above all, remind yourself right from the start that no research is perfect and all researchers make mistakes. Inscribe above your desk (or on your screen saver) the Latin phrase: *Errare humanum est* – ‘to make mistakes is to be human’ – and recognize that to be a human being studying human behaviour is to be doubly vulnerable to this maxim.

My overarching purpose in writing this chapter has been to help you to become your own best critic.

### **Conclusion**

Social and political research is characterized by the diversity of its concepts, theories, designs – and logics. Only a few will work ‘around the clock’ in Figure 14.1 and conclude with empirically grounded inferences about causal relations among variables. Many will choose a topic for which this would be premature or inappropriate, given the existing state of his or her discipline or his or her purpose for selecting a particular topic. They may exit the cycle relatively early, sometime between 1 and 3 PM – hopefully, with an improved understanding of the generic relations involved and, possibly, with a more elaborate set of hypotheses for future research. Still others will be interested in drawing out the ethical and normative implications of these relationships, perhaps by exploring analogies with previous experiences or prior philosophic assumptions. In Figure 14.3, I have labelled this point of exit as ‘the Logic of Discovery’, the idea being that those who take it will have made their original contribution by discovering empirical or normative relationships previously ignored or distorted by existing wisdom.

[Figure 14.3 about here]

From 3 to 6 PM, fewer social and political researchers will be leaving the cycle.<sup>1</sup> Their distinctive contribution will have been to identify the apposite universe surrounding the topics selected, to select cases that represent specified distributions of key variables and to have invented new ways of defining these variables and embedding them in more comprehensive theories. Most importantly, they will have carried further and in greater detail the existing conceptualization of the relationships surrounding their topic – hence, the notion that they have followed a ‘Logic of Explication’. Donatella della Porta, Peter Mair, Christine Chwaszcza and Fritz Kratochwil have contributed chapters that should be of particular interest to them, but all of them presuppose that explication is not an end in itself, but only a necessary precondition for passing to the next stage which involves the specification of indicators and the gathering of data.

Many more social and political scientists will exit after 6 PM and before 9 PM. They will have produced research that is fundamentally descriptive in nature. Here, the preoccupation is with the validity of their measurements and the accuracy of their observations. They will have gone into the field – even if it is in their own backyard – and generated new data about social and political phenomena. They are also most likely to have contributed to the development of better instruments of observation and more reliable indicators. This may be where the ultimate payoff lies – and certainly where the highest disciplinary status is usually awarded – but only a select few make

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1           Unfortunately, many of them will be so-called ABDs (All But Dissertations) who come up with a design for a previously conceptualized piece of research, could have written a proposal and may even have given some thought about operationalizing its variables, but never managed to actually find the time, resources or energy to gather the relevant data – much less to write them up.

it to this stage in the cycle, and even their conclusions are always contingent upon eventual replication by other scholars.

The reader should not be discouraged by this. To do original research on a topic you care about is an adventure. It can take you in different directions and end in different places. A lot will depend on your point of departure, but you will also be influenced at every turn by your professors and peers – not to mention the fads and fashions of your discipline. The most important thing is to be conscious and confident of the choices you will be making, and then to know when and where to exit from the cycle. Hopefully, this and the other essays in this volume will help you to make the voyage easier and, ultimately, more rewarding.

Figure One: THE CYCLE OF SOCIAL and POLITICAL RESEARCH

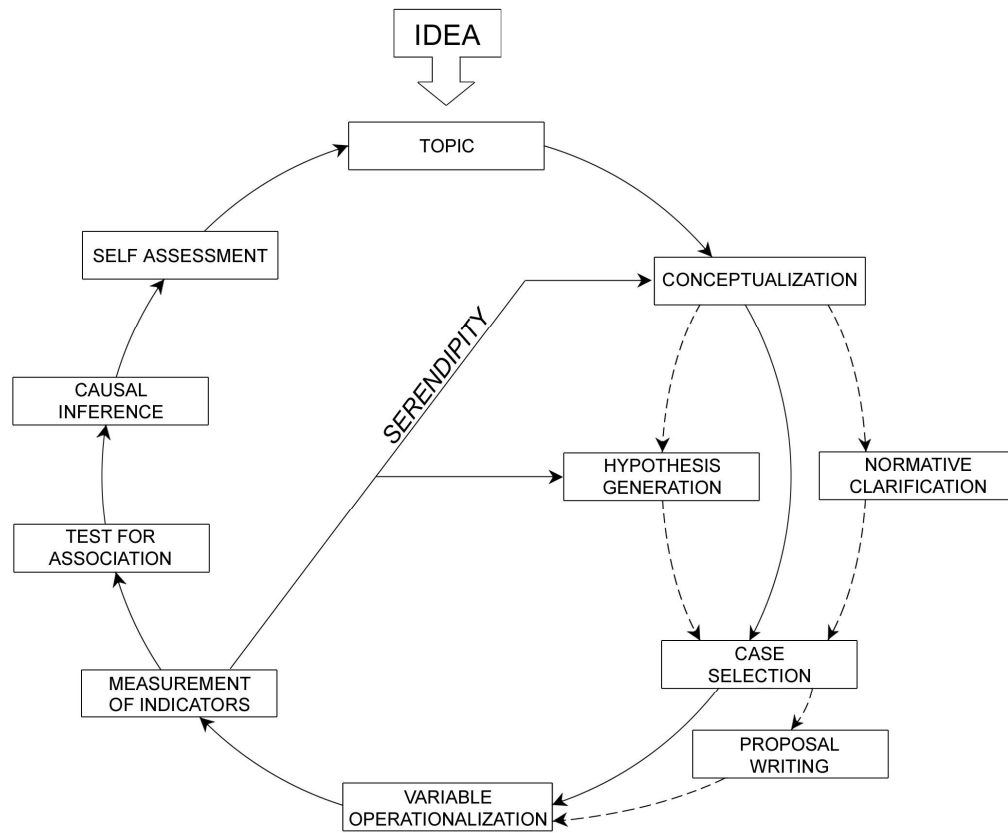


Figure Two: A SCHEMATIC FOR THE SELECTION OF CASES

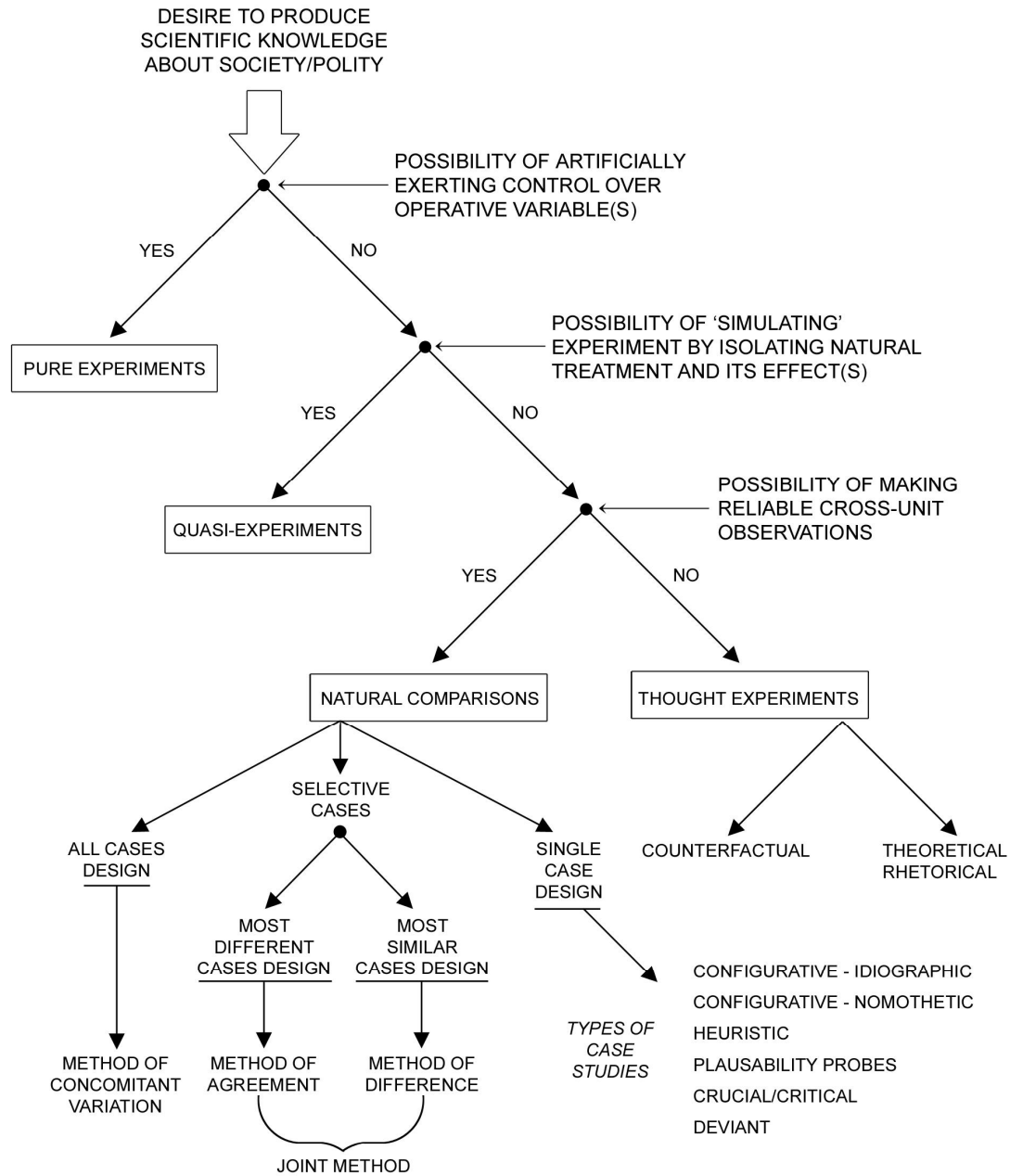
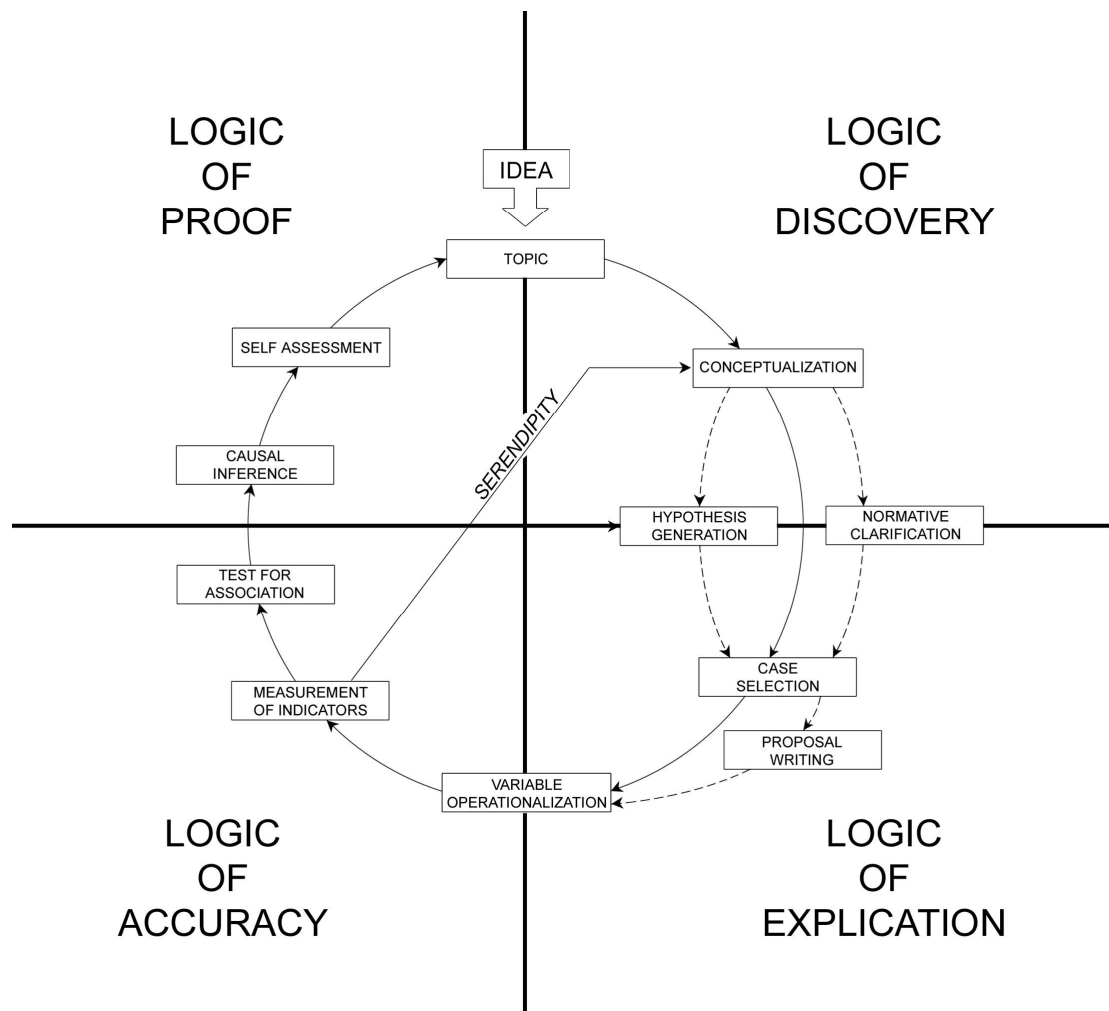




Figure Three: THE CYCLE OF SOCIAL AND POLITICAL RESEARCH AND ITS FOUR LOGICS



## CHAPTER FOUR

### MICRO-FOUNDATIONS FOR THE SCIENCE(S) OF POLITICS\*

Every science – whether physical or human – rests on its micro-foundations. These are the basic assumptions shared by its practitioners that shape the way in which they identify topics and transform them into projects worthy of research. Normally, they are invisible – as befits most foundations – and are usually accepted implicitly and without controversy. However, the visible structures of a science – its concepts, hypotheses, methods, data, associations and inferences – are only as valid as these foundations. And when the above ground products are faced with explaining empirical anomalies or unintended outcomes, then, it may become necessary to dig up its micro-foundations and re-examine them.

Political science (or political sciences, as I shall argue below) is no exception. **Power** – or its many cognates such as influence, pressure, distortion, manipulation, control, coercion, force, conflict, threat, collusion, hegemony, antagonism *e così via* – provides the substance around which its visible structure is built. And power serves to justify its generic claim to possessing distinctive micro-foundations as an academic discipline. Whether it is wielded legitimately or illegitimately, legally or illegally, overtly or covertly, publicly or privately, benignly or malignly, political science is supposed to explain how it is exercised and what are its effects.

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\* This is a revised version of a talk I gave on 25 August 2009 to the Department of Government at Uppsala University on the occasion of my receiving the Johan Skytte Prize. In it, I make a number of assertions about contemporary politics and those who currently practice the discipline of political science that I am convinced are correct, but have not documented with proper references. This will not please hardcore positivists, but either there exist no apposite quantitative sources of proof or the observations seem (to me) to be so qualitatively obvious as not to merit confirmation. Needless to say, I alone am responsible for all errors or omissions and welcome any assistance in discovering them.  
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I begin with the wise and venerable advice of Aristotle, “It is the mark of an educated man to look for precision in each class of things (read: **science** or **academic discipline**) just so far as the nature of the subject admits.”<sup>1</sup> Therefore, the practitioners of any science should be resting their research on a set of assumptions that are as “precise” as their subject matter is distinctive. Modern political science, especially as practiced in the United States, has passed repeatedly through periods in which efforts were made to fill its micro-foundations with assumptions drawn from other human sciences: law, social psychology, functionalist anthropology and, most recently and aggressively, neo-liberal economics. This has threatened to deprive it of the distinctive “precision” of its “class of things,” namely, the exercise of power and the consequences thereof. None of these intrusions has been completely successful, but each has left some subterranean residues.

For several decades now, the practices of politics seem to have been outrunning the theories of politics. Unexpected events have occurred – e.g. the collapse of communism; trends have emerged that are difficult to explain with the usual variables – e.g. the almost universal decline in voter turnout and party membership in ‘real-existing democracies’. Others that were supposed to have happened – remember “The End of History”? – did not; democratization has taken place and survived in settings that supposedly lacked all of the pre-requisites for its occurrence – just try to imagine the persistence of something approximating liberal democracy in Albania and Mongolia with the paradigms that prevailed in the 1960s and 1970s.

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<sup>1</sup> Nicomachaen Ethics, Book 1, Chapter 3, 2-3.,

So, it may be time to excavate and re-examine the micro-foundations of the political sciences.<sup>2</sup> For there are, at least, two quite different “classes of things” that students of politics have historically tried to explain. In this, I am following the advice of Niccolò Machiavelli whose micro-foundations consisted of a mixture of three elements: (1) *necessità* or the imperative of taking costly and consequential decisions under conditions of scarcity of resources, threat of violence and ambition of persons; (2) *virtù* or the capacity of rulers to understand the political context and to exploit it in order to create order and security; and (3) *fortuna* or the ever-present likelihood of unforeseen events and irresistible processes. When the latter becomes the dominant element, the very nature of politics is different. Without prudent “**men**, when times are quiet, (to) provide for them with dikes and dams,” the necessary exercise of power leads to fortuitous results. Since “Fortune is a **woman**,” and since he found himself in “a country without dams and without a dike,” he had to “enter upon a new way, as yet trodden by anyone else,” i.e. to invent a new science of politics.<sup>3</sup> Anticipating (and probably influencing) Hobbes, this misogynist Florentine attempted to pass on suggestions to rulers in situations in which political life was “nasty, brutish and short” and in which their overriding task was to create a “*stato*” – rather than to operate within an existing one. In normal (“male”) times Machiavelli implied, politics takes place within and between institutions (“dikes and dams”) that circumscribe the options of actors and make their behaviours more predictable.

Until recently, this line of demarcation was supposed to run between international relations and domestic politics, and was used to justify their separate

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<sup>2</sup> The French have long used the expression “les sciences politiques,” but as far as I can tell this has nothing to do with the epistemological distinction I am making here.

<sup>3</sup> The Prince ch. 25 & 26 and *The Discourses on Titus Livy*, I, pref. I am indebted to Roger D. Masters, *Machiavelli, Leonardo and the Science of Power* (Notre Dame: University of Notre Dame Press, 1996) for help with this interpretation, although the sexist twist is mine.

status as sub-disciplines within political science. The former was “female” and, hence, potentially anarchic, with no higher authority or predictably binding rules above its (allegedly) unitary and sovereign actors – the national states – that were expected to do whatever was necessary to further their particular interests and to defend themselves from predation by others. The latter was “male” and took place within a political space pre-defined by formal (if not always constitutional) and informal norms, ordered by a supreme (and sometimes legitimate) authority over a specific territory, and even one in a social setting that possessed a distinctive common ‘national’ identity.

This distinction within the discipline is no longer valid. International (or, better, interstate) relations have become clogged with a myriad of conventions, treaties, “regimes,” intergovernmental organizations and even (especially in the case of Europe) regional supra-national polities and courts. Sovereignty has become more and more of a formality; nationality is less and less exclusive. Meanwhile, the number of putatively sovereign and national states has proliferated and many of them have little or none of the “male” qualities described above. The list of outright “failed states” is getting longer and there is a growing waiting list of “defective ones.” Sometime (I suspect in the 1970s or 1980s), the line was crossed and it became statistically more likely that the resident of a given country would be killed in a civil war by one of his or her co-nationals than in an international war by foreigners.

The fact that the empirical loci of “female” and “male” politics has shifted does not invalidate the difference in terms of micro-foundations. Both are still very much present in our world and they definitely still require contrasting, not to say antithetic, sets of basic assumptions. In what follows, I will confine myself to

exploring what Machiavelli called the “male” side of the discipline.<sup>4</sup> While there is bound to be some overlap between the two research agendas, I am going to presume that the visible above-ground topics to be explained or understood are surrounded by pre-existing norms and institutions that are known to the actors and whose effect is more or less predictable. This does not preclude a lot of misunderstanding about what limits these rules impose and of opportunistic behaviour designed to probe their efficacy, and it does not presume that all of these constraints are formal (much less constitutional), but only that they can be taken as prior “givens” both by researchers and practitioners of M-politics.

What are the generic components of a solid and well-balanced micro-foundation for any discipline in the human/social sciences? And what, then, should be distinctive about an M-science of politics? These should be *a priori* assumptions that are more or less isomorphic with the situations involving power that are usually faced by politicians and justifiable according to the norms of the publics involved. Basing one’s science upon conditions that do not exist or values that cannot be satisfied may be useful for constructing formal models or exhorting people to change their behaviour, but both are, at best, of marginal utility as foundations for building the sort of empirical science of M-politics I have in mind.

I will explore five of these components that I am convinced are necessary for supporting a science of M-politics: (1) agents; (2) motives; (3) mechanisms; (4) regimes; and (5) units. In addition, I will discuss briefly one that no longer seems so

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<sup>4</sup> Terry Karl, my wife, has objected to this and suggested that I invert Machiavelli’s nomenclature on the grounds that women who study politics are notoriously reluctant to accept the root assumptions of his “female” science. I concede the point but have resisted out of respect for the original source. However, in order to neuter the discussion somewhat, I have converted the terms to “M-politics” meaning “most political situations” studied by contemporary political scientists and to “F-politics” meaning “few political situations” likely to be of interest to them.

relevant for most practitioners: *telos*. This epistemological exploration is a personal one, not a doctrinal affirmation valid for the discipline as a whole. The choices made by any one political scientist will be a complex function of one's theoretical predisposition and the nature of one's research topic. I am, however, inviting the readers of this essay to engage in a critical examinations of their own micro-foundations – as I have tried to do.

1. **Agents:** This is the most distinctive feature of a human as opposed to a natural or physical science. It begins with the assumption that the objects of research are also its subjects. In the case of politics, this means that agents can make relevant choices that are not completely determined by the conditions in which they find themselves. This inevitably introduces significant sources of innovation and unpredictability into the analysis or, as Machiavelli called them, *virtù e fortuna*. It also implies that the subjects have the capacity for reflexivity. They are historical in the sense that their present actions are influenced by reflections (“memories”) from the past and, hence, by learning they may alter their responses in similar situations. Moreover, the very process of researching the power relations among actors – past or present – can produce changes in the behaviour or expectations of the agents one is studying.

The vast majority of political science researchers presume that these agents are **individual and independent human beings** faced with and capable of making choices between alternative and consequential actions. They may all agree that these actors are uniquely capable of exerting such political agency, but they differ considerably about the properties that humans are capable of bringing to bear on their choices. Most recently, we have been told that

individuals they have pre-established and relatively fixed preferences, are able to rank these preferences consistently and assign to them a specific intensity, possess adequate information about alternative courses of action and theories about their effects, will predictably choose the one that (they think) best realizes those preferences, and still have the same preferences once the consequences of their choice have been experienced. Even with the insertion of caveats referring to such aspects as “bounded rationality,” “limited or asymmetric information,” “intransitive preferences,” “transaction costs,” and “logics of appropriateness or habit,” this generic conception of the role of agents accords not only with currently fashionable theories of rational choice, but reflects the much deeper ideological commitment of modern social and political thought to liberal individualism and rational progress. Shifting to a different micro-foundation would seem to many participants and observers to be equivalent to declaring that M-politics is a ‘passionate’ and not ‘rational’ activity rooted in raw emotion, blind faith, mindless imitation, instinctual tradition, collective stupidity and/or random events - and, hence, incapable of collectively improving the world that we live it.

I have had two reasons in my research for calling this time-worn foundation into question. The first has to do with the sheer complexity and contingency that surrounds the contemporary individual. He or she cannot possibly know what are the ‘real’ (or, even, all of the available) alternatives and, even less, what all of their eventual consequences will be. Moreover, this individual is very likely to discover upon reflection that he or she has many conflicting interests or passions – especially over different time horizons – and, hence, cannot order them consistently according to rank and intensity. And, if



those reasons were not enough, he or she is typically acting within a multi-layered and poly-centric “nested” set of institutions capable of making binding collective decisions – some public and some private. I have been led to conclude that agent preferences are not fixed, but contingent on which policies are proposed and by whom, and probably will change during the course of political exchange between the various layers and centers of power.

The second reason for resetting my micro-foundations is even more subversive of the prevailing orthodoxy. What if most of the significant actors engaged in M-politics were permanent organizations, not individual persons?<sup>5</sup> Granted that these organizations are composed of individuals and some of them may depend very closely on the contributions and allegiance of these persons – but many do not and have developed elaborate rules and sources of support that cannot be reduced to such individual actions. They embody collective choices made long ago and have acquired a reputation and legitimacy of their own. These are not just the arithmetic sum of independent and individual preferences, but political parties, interest associations, social movements, non-governmental organizations, business firms, government agencies and private foundations are often in the business of teaching these persons what their preferences should be and committing them to obeying policies made in their name.

M-politics whether in an autocracy or a democracy is all about representation – about collective bodies acting in lieu of individual persons by

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<sup>5</sup> We owe the initial discovery of this to a Scandinavian, Gunnar Hecksher, when he debated the consequences for the polity of its becoming “genom organiserat,” “Pluralist Democracy, the Swedish Experience,” *Social Research*, 15 (December 1948), 418-461. From another Scandinavian, Johan P. Olsen, came its first incorporation into the title of a full-length book: *Organized Democracy* (Bergen: Universitetsforlaget, 1983). However, I am convinced that this transformation is not limited to this part of the world. – it appears just a little earlier and remains more extreme up there.

intervening between them and their rulers. In the former case, the number of those involved is smaller and the criteria for their selection are more restrictive, but organizations are still likely to be the key actors. In the latter, freedom of association and petition – coupled with the diffusion of organizational skills from the private to the public realm – has made it almost mandatory for individuals to resort to collective bodies if they are to have any impact upon rulers or their policies.

And organizations have transformed the nature of politics. By definition, they have solved the problem of rational collective action by individuals and, in some cases, they may even have addressed some of the issues involved in the inequality of power resources by combining large numbers of individuals to countervail the concentrated influence of privileged groups. Their preferences do not have to be inferred or indirectly revealed; they are articulated publicly through their normal activity. Granted there are bound to be some elements of dissimulation, strategic action and hypocrisy in these activities, but these are minor compared to those of less well-informed and publicly committed individuals. Organizations are also capable (if they choose) of extending the time horizon for political calculations because they usually outlive their members (and sometimes even the social category they claim to represent). They tend to develop standard operating procedures and official ideologies that greatly facilitate the calculation of preferences and intensities and they collectivize these, making it much easier for authorities to consult and negotiate with them.

2. **Motives:** Establishing who the agents are does not tell us what is driving their political actions. Again, M-political science has its orthodox response: **self-interest** (sometimes tempered by the caveat, “rightly understood”). The individual political agent can invariably be relied upon to choose that alternative that best satisfies his or her own and highest ranked preference at the lowest cost. Albert Hirschmann has brilliantly traced the historical evolution of this notion from ancient times, when it was regarded as one of the worst of political vices (i.e. greed), to the present when it has become an omni-present and (allegedly) benevolent virtue.<sup>6</sup> Moreover, the fact that this transformation was closely associated with the emergence and dominance of capitalism has encouraged the wholesale and uncritical importation of the rhetoric and logic of market competition into the analysis of M-politics.

The most obvious objection to this micro-foundation, stressed by Hirschmann, is that human beings also have **passions**, and these have not been completely eliminated by capitalism. Individuals still can care about others, even about the whole society or political unit in which they live. Without belonging to some sort of meaningful collective identity, they would find it impossible to identify or act upon their individual interests since most political actions affect not just a person but a social, economic or cultural group. And, in order to belong to that group, and especially to convince others to act with them, individuals have to appear “reasonable,” i.e. to be capable of justifying their preferences. And they have to behave in an “appropriate” fashion, i.e. to conform to pre-established collective norms. In an M-type polity these

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<sup>6</sup> Albert O. Hirschmann, Passions and Interests (Princeton: Princeton University Press, 1970)

identities and norms are likely to be highly routinized by norms and codified by tradition, but they nonetheless are rooted in the passion for expressing oneself, for belonging and for being admired and these can severely limit the unbridled pursuit of self-interest.

Even if the political analyst insists on retaining the individual as his or her basic foundation, it would seem imperative to recognize explicitly the “mixed motives” of this agent. The “rightly understood” caveat implies internalized recognition of the norms and identities of others, and not just taking into strategic consideration the interests of these others or the likelihood of being punished by the laws of the unit as a whole. And that may even lead the analyst to acknowledge a role for “the public interest” as a factor in resolving the inevitably conflicting motives behind the exercise of power to make binding decisions upon the polity as a whole.

If one abandons the individualistic assumption of agency and switches to organizations, then, the task is greatly facilitated. By their very nature, these more or less permanent organizations are collectivities with internal processes for dealing with the diverse motives of their members and followers – and for coming up with a mediated expression of interests and/or passions that is publicly justifiable and normatively appropriate. Granted, again, that there is plenty of room for dissimulation, strategic action and outright hypocrisy on the part of these organizations, but revealing these will be facilitated by the more abundant and public nature of the information they provide.

3. **Mechanisms:** By and large, the mantra of the discipline is **competition**.

Agents exercise their relative power by competing with each other in order

to satisfy their respective interests (or passions). In the case of M-politics, this presumes the existence of a pre-existing institutional context in which conflicting motives are channelled by mutually respected rules into a process that limits the use of power resources and the range of possible outcomes. Otherwise, the agents would engage in unruly **conflict** not bound by such constraints and would exercise their power by threatening or exercising violence to impose their interests or passions, i.e., they would find themselves in a situation of F-politics.

This seems both a reasonable and realistic assumption and there are certainly many cases of politics in which the use of power has been domesticated in this fashion to the mutual benefit of the agents involved. The major distortion within the discipline comes when political scientists reduce its application to the process of **electoral** competition. The fact that political parties compete with each other for the representation of territorial constituencies and the right to form governments – even when these elections are freely and fairly conducted, and their outcomes uncertain – does not exhaust the mechanisms whereby political agents compete with each other over “the authoritative allocation of values.” Not surprisingly, these other mechanisms are populated less with individuals than with organizations: competition between interest associations to influence public policy; prosecution of politicians for violating legal norms by law firms or public interest groups; demonstrations by social movements to set the public agenda or to block the implementation of policies; revelations by rival media firms to discredit or support the reputation of rulers. All of these are important (and often highly institutionalized) features of M-politics that deserve at least as

much attention as the more sporadic and routinized conduct of electoral competition.<sup>7</sup>

Another mechanism also deserves a more prominent place in the micro-foundations of M-politics, namely: **cooperation**. If competition is not to degenerate into conflict, political agents have first to cooperate by agreeing upon the rules – formal or informal – that limit and channel their use of power. Many of these are habits or strictures inherited from previous generations (*vide* “path dependence”), but they are continuously subject to challenges as power relations and the identity of agents change and therefore require re-affirmation by contemporary agents. Moreover, these agents also cooperate in order to ally with each other, both to modify the pre-existing rules of engagement and to affect present policy outcomes. While it is understandable that political science should privilege competition – if only because its presence is much more visible and consequential – cooperation deserves more status and attention than it usually receives.

And, so does its perverse form, **collusion**, i.e., when agents act in agreement to prevent outsiders from competing through whichever of the usual mechanisms. Taken to the extreme, the resort to this kind of collective behaviour by insiders would involve shifting from M- to F-politics in the treatment of outsiders.

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<sup>7</sup> It has been argued by Adam Przeworski in his minimalist and dichotomous definition of democracy that these other mechanisms can be ignored because they are parasitic upon the electoral one. If elections are conducted properly, their outcome will condition and dominate the other forms of political competition between associations, movements, local and regional governments, etc. Democracy and the Market (Cambridge: Cambridge University Press, 1991), p. 10. I submit that the most elementary empirical examination of liberal democratic M-polities would reveal a great deal of divergence in the performance of these other mechanisms of competition. It is one thing to state that holding ‘proper’ elections is a necessary condition for sustaining a particular type of regime, i.e., liberal democracy, quite another to claim that this is a sufficient condition for all of its competitive mechanisms to function well.

4. **Regimes:** Students of M-politics typically assume that the polity they are analyzing has a relatively stable configuration of institutions that are complementary with each other, presumably as the result of a historical experience of searching among alternatives and eliminating incompatible ones through competition. The actions produced by its agents, motives and mechanisms are somehow – functionally, ideationally, intentionally or constitutionally – related to each other at a higher foundational level, such that their nature or importance cannot just be assessed alone and uniformly. They are embedded in an institutionalized whole that conditions what role can be played by individuals or organizations, self- or other-regarding interests, competitive or cooperative mechanisms.

Normally, these regimes are given a label and it is presumed that those in the same generic category will share many foundational elements. At one time, there were three such labels: **democratic**, **totalitarian** and **authoritarian** or **autocratic**. More recently, the middle one has dropped out and been replaced with “**hybrid**” or some other diminutive version of democracy or augmentative version of autocracy. Needless to say, each of these can be broken down further by the analyst into sub-types when he or she explores the performance of more specific agents, motives or mechanisms.

The implications of this intrusion of “regimes” into the micro-foundations of the discipline are considerable – if still debatable. For one thing, the recognition of such categoric diversity means giving up the quest for universalistic “covering laws” that can be applied to any agent, motive or mechanism. Individuals or organizations do not behave the same way in democracies and autocracies; the “reasonableness” and “appropriateness” of interests or passions depends on the institutions to which they

are addressed; mechanisms such as competitive elections or cooperative multi-party alliances can take on different meanings depending on their complimentary relationship with other mechanisms of competition/conflict or cooperation/collusion. This also may be reflected in the quite noticeable decline in references to “national” or “regional” peculiarities in explaining political behaviour. Adjectives such as “Asian,” “Latin American,” “African,” “Bolivian” or “Albanian” placed in front of substantives such as democracy or political culture tend now to have a descriptive and not an analytic importance. What counts are generic institutional configurations wherever they are located, rather than geo-cultural specificities.

**5. Units:** Ever since Aristotle collected the constitutions of 158 Greek city polities, the privileged unit in political science for both observation and analysis was supposed to have a relatively autonomous economy, a self-governing polity and a distinctive collective identity – all coinciding with each other in a given territory. Eventually, thanks to European domestic politics and overseas empires, this unit became the **sovereign national state**. It is usually presumed that only within this type of unit are agents capable of making choices and implementing them effectively: most individuals or organizations calculate their interests and passions; most political mechanisms really operate; most regimes work out their stable institutional complementarities.

Nothing is more firmly rooted in the micro-foundations of M-political science than this assumption.<sup>8</sup> Virtually every existing proposition about politics should be

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<sup>8</sup> It was precisely the absence of such a unit that distinguished the “female times” of Machiavelli and formerly characterized the anarchic universe of international relations. Even in the latter case, however, most analysts presumed the existence of a number of such self-contained and self-referential unitary polities capable of acting in a sovereign manner to defend their national interests.



prefaced with the following phrase: “Take an existing national state and, only then, will ...(X be related to Y in the following manner).”<sup>9</sup> But what if this unit of analysis can no longer be taken for granted? What if that presumed coincidence between autonomy, capacity and identity has been disrupted beyond repair? What units, then, can provide the necessary micro-foundations for observing and comparing M-political behaviour?

In the contemporary world, no political unit can realistically connect cause and effect and produce intended results without regard for the actions of others. Virtually all polities have persons and organizations within their borders that have identities, loyalties and interests that overlap with persons and organizations in other polities. Nor can one be assured that polities with the same formal political status or level of aggregation will have the same capacity for agency. Depending on their insertion into multi-layered systems of production, distribution and governance, their capacity to act or react independently to any specific opportunity or challenge can vary enormously. This is most obviously the case for those national states that have entered into supra-national arrangements, such as the European Union, or signed binding international treaties such as those of the IMF or the WTO. Not only do they occasionally find themselves publicly shamed or found guilty by such organizations, but they regularly anticipate such constraints and alter their behavior accordingly. Moreover, many contemporary national polities have granted or been forced to concede extensive powers to their sub-national units and, in some cases, these *provinces, cantons, regioni, estados autónomicos* or *départments* have entered into cooperative

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<sup>9</sup> Only rarely do M-political scientists bother to ask **why** a given sovereign state exists. This is simply taken as a given upon which to build a structure of explanation. And most comparative analyses presume that all of the units included are of an identical or similar nature. Explaining how these units got that way is presumably a job left to national historians who, of course, give detailed and idiosyncratic descriptions of the process of state formation.

arrangements with equivalent units in other national states. From these observations, I conclude that M-political scientists need to dedicate much more thought to the units they choose and the properties these units supposedly share with regard to the specific agent, motive, mechanism or regime that is being examined. There still remains a great deal of variation that can only be explained by conditions prevailing at the internal (national) level, but exorcising or ignoring the increasingly complex external context in which these units are embedded introduces a serious analytical distortion.

The practice of political science does follow, and should incorporate, changes in “real-existing politics” – but it always does so with a considerable delay. The most important set of generic changes that have occurred in recent decades involves the spread of “complex interdependence” and many of the anomalies and unexpected political outcomes can be traced to them. There is absolutely nothing new about the fact that formally independent polities have extensive relations with each other. That used to be justified in the micro-foundations of F-politics. What is novel is not only the sheer magnitude and diversity of these exchanges, but also the extent to which they penetrate into virtually all social, economic and cultural groups and into almost all geographic areas within these polities. Previously, they were mainly concentrated among restricted elites living in a few favored cities or regions. Now, it takes an extraordinary political effort to prevent the population anywhere within national borders from becoming “contaminated” by the flow of foreign ideas and enticements. “Globalization” has become the catch-all term for these developments, even if it tends to exaggerate the evenness of their spread and scope across the planet.

Its impact upon specific agents, motives, mechanisms and regimes is highly contentious, but two (admittedly hypothetical) trends would seem to have special relevance for the conduct of M-political science:

(1) Globalization narrows the potential range of policy responses, undermines the capacity of (no-longer) sovereign national states to respond autonomously to the demands of their citizenry and, thereby, weakens the legitimacy of traditional political intermediaries and state authorities;

2) Globalization widens the resources available to non-state actors acting across national borders and shifts policy responsibility upward to trans-national quasi-state actors – both of which undermine formal institutions and informal arrangements at the national level, and promote the diffusion of new trans-national norms of political behavior.

If either of these is true, and especially if both are, then a major shift in micro-foundations should occur – and M-political scientists will have to give serious thought to whether the sovereign national state should remain their exclusive or even privileged unit of observation and analysis.

The most difficult challenge will come from abandoning the presumption of “stateness.” Sovereignty has long been an abstract concept that “everyone knew” was only a convenient fiction, just as they also “knew” that almost all states had social groups within them that did not share the same common political identity. One could pretend that the units were independent of each other in choosing their organizations, regimes and policies and one could get away with assuming that something called “the national interest” existed and, when invoked, did have an impact upon such collective choices. But the notion of stateness impregnates the furthest corners of the vocabulary used to discuss politics – especially stable, iterative M-politics. It seems self-evident to most analysts that this particular form of organizing political life will continue to dominate all others, spend most publicly generated funds, authoritatively allocate most resources, enjoy a unique source of legitimacy and furnish most people with a distinctive identity. However we may recognize that the sovereign national state is under assault from a variety of directions – beneath and beyond its borders –

its “considerable resilience” has been repeatedly asserted.<sup>10</sup> To expunge it (or even to qualify it significantly) would mean, literally, starting all over and creating a whole new language for talking about and studying politics. The assiduous reader will have noted that I have already tried to do this by frequently referring to “polity” in this talk when the normal phrase would have been “state” or “nation”.

**6. Telos:** Here I shall be brief since very few practicing M-political scientists would now include this among their micro-foundations. It used to be assumed that politics was heading in a predictable direction – that the entire sub-structure of power and authority was moving somewhere over time, however erratically and unevenly, across different units. The Will of God, the power of human rationality, the natural selection of historical evolution, or the greater normative appeal of liberal democracy have been at various times candidates for explaining why better values and institutions would eventually win out. Francis Fukuyama’s re-furbishing of Hegel’s dialectic of rationality briefly revived such thinking, but the “End of History” proved ephemeral. Not even the most enthusiastic students of democratization (and I include myself among them) would argue that these recent regime changes are definitive or deny that well-entrenched “real-existing” democracies might not be in deep trouble.

### **Conclusion**

Aristotle famously argued that political science was the “master science” since all of the other human sciences depended upon the results produced by politics. Ironically, this assertion of its superiority is also a source of weakness when it comes to assessing its micro-foundations. Political science is bound to be an “open science.” It reaches into and affects crucial aspects of other realms of human behaviour and is,

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<sup>10</sup> As Stanley Hoffmann argued so eloquently in his “Obstinate or Obsolete? The Fate of the Nation-State and the Case of Western Europe,” *Daedalus*, Vol. 95, No. 3 (Summer 1966), pp. 862-915.

therefore, bound to be penetrated by assumptions and concepts coming from them. Law, sociology, psychology and, especially, economics have all claimed to be closed and, therefore, self-referential sciences and each of them has attempted to penetrate the deepest micro-foundations of political science. At times, this has threatened to deprive the discipline of its distinctive focus on the use of power and its conversion (sometimes) into legitimate authority to resolve conflicts and achieve collective purposes.

In recent decades, attempts have been made to reduce politics to the voluntary exchange of information, the joint product of individual attitudes, the rational search for optimal institutions, or the deliberative discourse between *herrschaftsfrei* persons or organizations. This may have served to illuminate some of its peripheral aspects or expand its scope of inquiry, but they have all floundered when trying to explain situations in which “the preferences, desires or intentions of one or more actors bring about conforming actions, or predispositions to act, of one or more other actors.”<sup>11</sup> When some person (and, even more, when some established public or private organization) can alter the distribution of information, manipulate the attitudes of individuals, restrict the range of ‘acceptable’ institutions or distort the course of deliberations, the outcome becomes different – often radically different – than the one envisaged by any of these models. If political scientists were to narrow their research agenda to situations where these conditions were not present, not only would they be depriving their discipline of the cornerstone of its foundation, but they would also become incapable of providing useful knowledge to the politicians, representatives,

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<sup>11</sup> This is the classical definition of control (power) provided in Robert Dahl, *Dilemmas of Pluralist Democracy* (New Haven: Yale University Press, 1982), p. 16.

citizens and subjects who have to cope with the very real existence of power and its consequences.<sup>12</sup>

I am convinced that the micro-foundations of M-political science should be dug up and critically re-examined. The above-ground edifices that they are supposed to be supporting have changed significantly and I suspect that one of the reasons political scientists are less capable of explaining these behaviours and outcomes is because they often start with the wrong basic assumptions about agents, motives, mechanisms, regimes and units. Worse, they have periodically allowed themselves to be undermined by the practitioners of other social sciences that may have greater professional prestige,<sup>13</sup> but which have no comprehension of the distinctive “precision” of the “class of things” that is politics. However, if the past still has any validity, each time it has been invaded, the discipline has eventually recuperated its political precision (or, better, its understanding of political imprecision) and even extracted some things of value from those who invaded it.

The sciences of politics – both of them – have to adapt to the increased complexity and contingency of their subject matter. Power and its cognates are being wielded in new settings, under different constraints and by a greater variety of actors. My hunch is that awareness of this will lead to a loosening and diversifying of each of these five micro-foundational elements – and to greater sensitivity to their interactions.

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<sup>12</sup> Which is not to claim that all mistaken predictions and retrodictions must be due to distortions that can be attributed to power or its cognates. Some of them may be due to erroneous or unrealistic assumptions embedded within these apolitical conceptions of how decisions are made or conflicts resolved. And, of course, there is always *fortuna*, i.e. conditions or events that could not be foreseen. Machiavelli’s estimate was that practitioners of the new science of politics should be content if they were right 50% of the time – even after taking *necessità* and *virtù* into account.

<sup>13</sup> One shall see if the recent precipitous decline in the prestige of neo-liberal economics will suffice to break the hold of its acolytes, especially on American political science – or whether its penetration of micro-foundations has been so profound that the discipline will continue to support research and publication based on its assumptions. The fact that the careers of so many professional political scientists in the United States (and a few in Europe) have become dependent on such a survival suggests that the latter outcome will prevail.

Agents can be individuals or organizations ... depending. Motives can be based on narrow, immediate self-regarding interests or broad, future and collective-regarding passions ... depending. Mechanisms of competition and cooperation can involve elected representatives or self-selected ones ... depending. Regimes can be classified as democratic or autocratic ... and they can change, even permute into each other. Finally, units can be simultaneously relevant at different levels of identity and aggregation ... and so can the agents, motives, mechanisms and regimes within them.

What is needed for advancing such a radically pluralistic M-science of politics is not the testing of single hypotheses or models from a single perspective, but the generation of hypotheses and models rooted in different micro-foundational assumptions and the systematic comparison of the different results they predict (or, more often, retrodict). Most probably, this can only be accomplished by using different types of data and tests for association. If this sounds messy and inconclusive, so is “the class of things” that political scientists are condemned to deal with.

**SEVEN (DISPUTABLE) THESES CONCERNING THE FUTURE OF  
'TRANSATLANTICIZED' OR 'GLOBALIZED' POLITICAL SCIENCE\***

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[revised version]

\* An earlier version of this essay was written for a conference organized by Luigi Graziano on the occasion of the 50<sup>th</sup> Anniversary of the International Political Science Association. The panel at which it was presented was asked to address the issue: "Political Science: A American Science?" Although I was unable to attend the conference, I gather that it was not well received by the assembled cast of international luminaries. Not only did I imply that IPSA might have been complicit in the effort to americanize the discipline, but I also expressed the opinion that political science is not and cannot ever be "American" and that, if current trends persist unchanged on the other side of the Atlantic, it will become less so in the future. I thank the editor of this review, James Newell, for having rescued this piece from obscurity and. Therewith, absolve him of any responsibility for its content.

In this slightly revised version, I have not changed two things: (1) I have abjured recourse to extensive footnotes, hence, almost all of my factual claims remain undocumented; and (2) I have not toned down its manifestly polemic and personal tone, despite repeated suggestions that I do so.



The one thing no one questions is that the disciplined study of politics is in flux. Political scientists, *politologues*, *Politischewissenschaftler*, *polítologas* and *politologi* seem, even more than their brethren in the other social sciences, to have to deal with a major crisis in their discipline and, hence, to be “condemned to live in interesting times” – as the Chinese proverb puts it. I can sympathize with those who find their pet paradigms in ruins or their area of specialization virtually eliminated in a burst of “creative destruction,” but I am confident that in the long run this turbulence will improve the discipline. Of course, those of us now in the midst of this crisis may not be around to benefit personally or professionally from these improvements when they are finally realized.

It is tempting to view this turbulence from the perspective of “globalization,” i.e. as the product of a rather vast (if mysterious) array of changes in scale that tend to cluster together, reinforce each other and seem to be accelerating in their cumulative impact. In our field, they all have something to do with encouraging exchanges between individual scholars and academic institutions – compressing their exchanges in time and space, lowering their transaction costs and overcoming previous restrictions posed by national political or cultural barriers. Political science has always been, in principle, a cosmopolitan enterprise. Its practice, however, has been strongly conditioned by the very parochial concerns of the national compartments in which it has heretofore been confined. Since its “invention” in ancient Greece, the center of innovation in political thinking has shifted several times, but its concepts, assumptions and methods have eventually diffused from one place to another.

Its cumulative development as a distinctive scholarly discipline has been relatively recent and closely associated with the emergence of stable republican/democratic governments, hence, the longer any given polity has had rulers who have been systematically accountable to their citizenry in some form or another, the more likely it is that political science will have prospered within it. In short, there is reason to believe that the evolution of political science is isomorphic with the evolution of its subject matter. As goes the practice of politics, so will (eventually, if belatedly) go the science of politics.

None of the individual changes presently affecting the discipline is novel. What is unprecedented is their volume, variety and cumulative impact. Moreover, despite the label, “global,” the distribution of this temporal and spatial compression is neither universal nor even. It is very much concentrated on the scholarly exchanges between America and Europe. The former is seen by many observers (and, especially by its fans) as playing the compound role of coach, goal-keeper, striker and referee, with the latter at best occupying the mid-field and the rest of the world setting on the bench waiting to be called into the game.

From this “transatlanticized” perspective, the future of political science is clear – and it is already on display on the western side of the Atlantic. It is merely a matter of time before national and regional resistances are overcome and the entire discipline will converge upon an identical set of concepts, assumptions and methods. In the first part of this essay, I have attempted to formulate and formalize this perspective in a set of seven “theses.” They are stated without nuance or respect for national/regional sensitivities – hence, I do

not expect that most non-American political scientists would overtly subscribe to them or that most American political scientists would admit openly to such an “imperial” attitude. What I do expect is that very many in both groups would, at least covertly, agree with them – although I have no independent proof of this.

Until I came across the introductory chapters to A New Handbook of Political Science! Especially in the essay by Robert E. Goodin and Hans-Dieter Klingemann on “Political Science: The Discipline,” I found a massive confirmation of my hunch. While none of my seven theses is explicitly stated there, they can all be inferred from its text. Moreover, in their celebratory style, Goodin and Klingemann seem to welcome unconditionally this skewed transatlantic convergence toward American concepts, assumptions and methods and to regret whatever resistance to it might still be residing in national or regional practices within Europe. In the entire chapter, there is not a single hesitation or critical remark about how Americans do their political science. “Rapprochement” with the victors on the other side of the Atlantic is described as taking place “gladly” and not “grudgingly” according to them.<sup>i</sup>

Since the Goodin & Klingemann volume bears very prominently the *imprimatur* of the International Political Science Association, anyone reading it is likely to assume that it is the policy of IPSA not only to promote more universalistic standards of training and accomplishment in the discipline, but also to serve as the agent of its Americanization-*cum*-Transatlanticization. Indeed, considering the composition of topics and invitees to the 50<sup>th</sup> Anniversary Conference at which this essay was initially presented, it does not seem far-

fetched to describe that meeting as part of such an “imperial” campaign. Scholars from the United States and “foreigners” educated or teaching there comprised the vast bulk of its participants.

## SEVEN THESES

Let us then convert this implicit notion of American superiority into explicit theses that could eventually be tested:

- I. **The Convergence Thesis:** Scholars specializing in the study of politics will increasingly converge in their use of concepts, assumptions and methods. Previous national and regional differences will diminish and eventually disappear. Political scientists, wherever they find themselves, will eventually perform virtually the same operations on the same variables for the same purposes and arrive at shared conclusions about causality based on the same criteria of inference. The increasing dominance of English within the discipline, no doubt, promotes this outcome, but even those writing in other languages will be compelled into “convergent translation” or risk being relegated to obscurity.
- II. **The Asymmetry Thesis:** This convergence in concepts, assumptions and methods will not involve “splitting the difference” or “regressing to the mean” as it is presently distributed across nations and regions, but movement toward the standards set previously in the discipline by its most hegemonic player, i.e. the United States of America. Most innovations will come from mainstream “leading” American political scientists, and their diffusion to practitioners in lesser nations and regions is only a matter of time. “Dissident” American political scientists will first be marginalized at home and, then, have little or no impact outside their country.
- III. **The Sequential Thesis:** The initial core area for this process of diffusion consists of the North Atlantic. All things being equal, American concepts, assumptions and methods will first have an impact upon Northern (and later Southern) Europe – and only then will they “travel” to more peripheral settings. The fact that political science has long been practiced in relatively self-contained national compartments within Europe is an impediment to the smooth functioning of “transatlanticized” political science; therefore, strong pressures will be exerted to promote increased convergence among Europe’s national disciplines, but only as a prelude to the eventual convergence of all of them with American political science. Analogous processes of sequential convergence at the regional level

seem also to be developing among Latin American and some African practitioners. In Asia, regional aggregation seems almost non-existent and more direct forms of dependence upon the United States seem to be the rule.

- IV. **The Professionalization Thesis:** The primary mechanism behind this process of staggered but irrevocable convergence will be that of professional standardization. Norms with regard to how political science should be practiced will be set, monitored and policed initially by American professional associations and institutions of higher learning and, subsequently, these will be adopted by national and regional organizations in more peripheral locations. Political scientists who refuse to conform to these norms will be discriminated against in hiring, promotion, access to journals, invitations to scholarly congresses and leadership positions in national and international associations – with IPSA, incidentally, likely to play a key role in this process. The obvious analogy is with the discipline of economics which succeeded within a few decades in driving out of all of its major university departments almost all “dissident” practitioners – first in the United States and, more recently, elsewhere.
  
- V. **The Efficiency Thesis:** A secondary but very important mechanism in this process consists of the insertion of principles of market competition into this increasingly transatlanticized and eventually globalized profession. Because the practice of political science in the United States is already more sensitive to market pressures and, hence, more capable of adapting its incentive structure to changes in supply and demand, it will be more successful in rewarding those who conform to its norms than will competing national or regional producers. As one result, there will be a permanent “brain drain” from Europe and the periphery to the United States. Even the most dynamic and critical practitioners of “non-American” political science will find it hard to resist these rewards – especially when they have to cope with more formalized, hierarchically structured and publicly regulated work environments in which salaries and status bear little relation to the teaching or research performance of individual scholars. Needless to say, this draining-off of talent will only further enhance the disciplinary hegemony of American political science – at least until saturation and crowding-out effects set in.
  
- VI. **The Universality Thesis:** In addition to all of the above, the superiority of American political science is further ensured by the fact that the *moeurs* of American culture are becoming increasingly universal. This permits its practitioners to base their generic assumptions about political behavior upon observations (and presumptions) that are specific to the immediate

setting with which they are familiar: individualism (social and political, as well as methodological), spontaneously “opportunistic” and “self-regarding” behavior, “non-Tuism,” maximization at the margin, basic material satisfaction, weak ideological motivation, low degrees of interest in politics, lots of “slack” in citizen participation, respect for the law and existing practices, “normal,” i.e. uni-modal and non-skewed, distributions of preference on most issues, relatively legitimate institutions, established national identity, high tolerance for social and economic inequalities, absence of over-riding or over-whelming cleavages, *e cosí via*. In short, it can be presumed that what seems rational in American political behavior must also be rational elsewhere – hence, an enormous saving on time and effort in that it allows American political scientists to forego the high cost of learning foreign languages and conducting field research. The data, so to speak, come to them without effort on their part (and, increasingly, it even comes in English!). When they don’t, positions can always be “simulated” or preferences can be safely “inferred” on the basis of American precedents.

- VII. **The Ahistoricity Thesis:** Given the presumption of universality built into American political science, its practitioners can also afford to be indifferent to specificities of time and space. Whatever happened long ago or came before can be safely ignored, either because current outcomes can be explained by relatively short-term calculations of utility maximization that are not sensitive to previous choices, or because whatever the preferences may have been in the past they are being increasingly overridden by the global diffusion of common norms and expectations in the present. To the extent that time is a relevant factor in this new “trans-historical” science, it can be reduced to iterative interaction among like-minded actors and discounted across some reliable time period. Another useful, effort-saving device is systematic (if naïve) “presentism.” One can either simply ignore all previous instances of the behavior one is presently studying on the grounds that “preferences” were then different; or one can reconstruct these instances via “stylized facts” in such a way that contemporary motives and calculations will provide a plausible *ex post* explanation for whatever outcome occurred in the past. Needless to say, this is much easier to get away with in the context of the United States -- where formal political institutions have been unusually constant over time and where successive generations of newcomers have been assimilated into a continuously “re-invented mythical national community” -- than it is in all but a very few European countries and none of the ex-colonial ones.

Now, let us take a second and more critical look at these seven theses:

## **I. Convergence**

The evidence on the independent variable in this thesis – globalization -- is quite convincing. Any systematic compilation of data would no doubt show that the probability of any two political scientists, randomly selected from different national contexts, meeting each other sometime in their careers has greatly increased over the past two decades. The volume of and attendance at international congresses, the composition of boards of international professional associations, the number of papers jointly authored by persons of different nationality, the extent of cross-citation across political borders on almost any substantive issue, the likelihood of younger graduate students obtaining at least part of their training in another country or being employed at some point in their career in more than one country – all these unobtrusive indicators seem to be pointing in the same, i.e. upward, direction. Moreover, my hunch is that most of them show a cumulative, exponential tendency – especially when plotted over successive generations of political scientists. The more recent one's training, the more likely one is to have been exposed to concepts, assumptions and methods from a different national tradition.

The problem lies in inferring the consequences of this burgeoning “transatlanticization” or “globalization” of academic production and exchange – i.e. in assuming that it must lead to convergence. It is certainly logically possible that, in a fashion analogous to trade and investment in material goods, the actors involved learn to exploit their different mixes of resources. Instead of mimicking their competitors, they specialize even more in what they do best (and, in the

process, become more aware of the limitations built into the products of “first-movers”). Especially when the political science community is relatively small and, hence, its “niche” products do not threaten the status or the market share of the hegemonic producer, this would seem to be an intelligent strategy – as is abundantly illustrated by the relative success in “diversified quality production” of small and medium size firms in small and medium size European countries.<sup>ii</sup> If nothing else, the historical experience of Europe’s small democracies shows a marked tendency for diversity, innovation and experimentation. I see no reason why this should not be the case for their respective *politologues* and these qualitative differences may even increase in the future in response to certain characteristics of mass-produced, first-moving American political science.

## **II. Asymmetry**

Here again, no one can contest certain facts. Of the world’s total number of employed political scientists, as much as 80% are said to work in the United States.<sup>iii</sup> Measured in terms of words, pages or number of published articles and books, their production is much greater than all others combined – although it is debatable whether this volume of output is proportional to the absolute number of those engaged (and even more debatable whether the quality of output is similarly proportional).<sup>iv</sup> For example, at the present moment, there are ca. 7,500 regular members of the American Political Science Association (APSA) and somewhere between 3,500 and 5,000 scholars are affiliated with the +200 institutions that are members of its European-level equivalent, the European Consortium for Political Research (ECPR). Considering that the two “regions”



are of approximately equal total population, that translates into a relative density some 1,5 to 2 times greater in the case of the United States (most Canadian political scientists are likely to be members of the APSA, but their total number -- ca. 500 -- does not change the calculation very much).

Nor is this so surprising. The lengthy and continuous history of liberal democracy, the proliferation of state universities due to federalism, the parallel existence of a very numerous set of (usually) smaller private universities, the larger proportion of youth who enter post-secondary education -- all these factors translate into a greater demand for instruction in the social sciences relative to Europe. When combined with the greater ease with which political science managed to separate itself institutionally from the other academic specializations that often supplied its original practitioners: law, sociology, philosophy and history, the creation of the world's first private foundations devoted to funding social and political research, and the enormous boost it received when German, Austrian and Italian refugees entered its ranks, the head-start of American political science seems, in retrospect, to have been an over-determined outcome. Seen, however, from the immediate perspective, the rest of the world (and Europe, in particular) is rapidly closing the numerical gap.<sup>v</sup>

Which still leaves the issue of asymmetry in terms of the content and methods of the discipline. More is not necessarily better. Goodin & Klingemann take it for granted not only that American political science is presently hegemonic in virtually every sense of the term, but that it will remain so and that this is a very desirable thing. While noting the resurgence of interest and production in

Europe, they do not seem to envisage the possibility that Europeans might surpass their “masters” – not necessarily in quantity, but in quality. For example, I suspect that more political science graduate students from European countries now study in other European universities than go to the United States. Some of this is a simple matter of relative costs, but much of the shift can be attributed to deliberate programs of national governments and the European Union, as well as a growing sense that American political science may have little to contribute to understanding their countries or Europe as a whole. One of the biggest contemporary producers of doctorates in political science is the European University Institute in Florence which recruits from all EU member countries (and then some) and whose graduates increasingly find academic job opportunities outside their country of origin.

I also suspect that if it were possible to measure the “consumption” of political science scholarship, rather than its mere production, Europeans would not come out so far behind.<sup>vi</sup> A very substantial (and growing) proportion of American output seems to be oriented exclusively to those within the discipline (and, increasingly, to a small “club” of like-minded practitioners within it). Much of what European *Politischewissenschaftler* publish goes into less specialized journals aimed at a broader audience (and, therefore, is often not even counted as “professional output”). It enters into a broad public intellectual domain – much like the New York Review of Books or op-ed articles in major newspapers. Very few American political scientists dare to make such an effort, presumably on the grounds that it would be a waste of their professional skills.<sup>vii</sup> I do not think it

exaggerated to claim that, while American political scientists see their task as exclusively “professional,” their European (and Latin American and African) counterparts see it as equally “intellectual.”

Which may be one of many reasons why these political scientists outside of America are much more likely to be engaged in the political struggles of their respective countries and to make a more significant contribution to setting the policy agenda.<sup>viii</sup> Except for a recent brief flurry around the issue of impeachment, our American colleagues have habitually chosen to stay *au dessus de la mêlée*. And those who do get involved, such as the occasional TV pundit, are often dismissed for having “acted unprofessionally.”<sup>ix</sup>

### **III. Sequentiality**

This thesis is crucial for evaluating the ‘global’ future of the discipline since, given the disparities in absolute numbers and points of departure, it will only be by creating regionally-based “communities” of political scientists that those training and/or working outside the United States will be able to break its hegemonic grip. Only such a strategy can bring together the economies of scale and scope that are needed to develop alternative approaches and to offer attractive career opportunities to those who choose them.

And there is considerable evidence that the effort is being made. In Europe, this is manifested by European Consortium for Political Research (ECPR) which has grown steadily in the number of its member institutions (significantly, its membership is not of individuals but organizations) and in attendance at its annual meetings.<sup>x</sup> In Latin America, something analogous is

happening *via* the Consejo Latino-americano de Ciencias Sociales (CLACSO) and in the various branches of the Facultad Latin-Americana de Ciencias Sociales (FLACSO). Ambitious graduate students in political science in both parts of the world are increasingly aware of the need for “cross-national” training and career experience, but a steadily growing proportion of them has chosen to do this within the two regions -- rather than make the previously obligatory pilgrimage to the Meccas of U.S. scholarship: Harvard, Yale, Chicago, Berkeley, Michigan, Stanford, MIT, Princeton, Columbia , Minnesota, North Carolina *et ainsi de suite*.

All this effort would be irrelevant if, ultimately, these points of trans-national aggregation proved to be but “way-stations” on the route to a more thorough-going Americanization of concepts, assumptions and methods. If institutions such as the ECPR and its Essex Summer School (and I would include Department of Political and Social Sciences of the European University Institute where I am presently employed) merely serve as “academic franchises” peddling the same merchandise as their American counterparts, they will not only have failed in their ostensible purpose, but they will also have added greater legitimacy to the whole process of “transatlanticization-*cum*-americanization.” If and when their products become indistinguishable from what is done on the other side of the Atlantic, then, it will be impossible to deny that a globalization of the discipline has indeed occurred and on terms set by the American political science community.

It is obviously too early to judge whether “regionalization” in either Europe or Latin America is going to be able to challenge “transatlanticization-*cum*-americanization.” In a fascinating parallel with the more comprehensive process of European economic and political integration, we still do not know whether such policies are going to result in an acceleration of trends that are already under way in other advanced industrial societies, an inversion of them, or simply no net effect at all.<sup>xi</sup> Seen from the *optique* of the EUI, I can see both signs of resistance to the hegemony of American concepts, assumptions and methods, and some evidence of the firming-up of an alternative “historical-sociological-institutional” paradigm, but I cannot ignore the contrary evidence – namely, that when our *ricercatori* are offered the opportunity to spend a year at one of several of America’s major research universities, they jump at it! There is a strong demand for attending the APSA annual convention (although many who do so come back reporting how boring it was) and presenting a paper there is still a major status asset. Especially ironic is the fact that in two fields where one might expect European-ness to assert itself academically – namely, the comparative study of Europe’s “domestic” politics and the analysis of the politics of European integration – very substantial numbers of political scientists cross the Atlantic to attend the bi-annual meetings of the Council on European Studies (CES) and the European Community Studies Association (ECSA).

Regardless of these mixed signals, one thing I can affirm with confidence: perhaps the most indispensable element for the eventual success of regional resistance is coming into place, i.e. the creation of a genuinely cross-national

market within Europe for political science talent. The United States of America has long benefited from its flexibility in being able to hire “the best” without regard to nationality (not to mention its status as a *refuge* for distinguished exile scholars). Thanks in part to the directives of the European Union, discrimination in hiring among its members is no longer possible and one can even occasionally observe competition among them to attract the best *extra-comunitari* talent. It is no longer “axiomatic” that Europeans with doctoral degrees in political science will work in their own country – or, if they do not, that they will emigrate to the States, Canada or Australia.

#### **IV. Professionalization**

Which brings us to the crucial role played by professionalization. Again, no one can deny the trend: persons who engage in the study of politics are more likely today than yesterday to do so full-time, to have undergone a specialized type of certification, to have their work evaluated by pre-established standards, to be a member of a specific sub-set of associations, and to be advanced in their careers according to the meritocratic criteria embedded in the previously mentioned process of professionalization. This is just as true in Europe as in the United States in terms of the direction of change, even if specific components may still differ. For example, part-time and irregular forms of employment for young political scientists seem to be increasing everywhere – hopefully, only temporarily -- and, I suspect, the fetish attached to “peer-reviewed articles in major journals” may still be stronger in the United States.<sup>xii</sup>

What is questionable is the inference that the training and standards involved in this process are being set exclusively by the Americans, that this will continue to be true into the indefinite future, – and that this is “gladly and not grudgingly” accepted by the others. One must first begin with the recognition that training and standards are by no means homogenous in the United States. Despite the vested interests of two successive “clans” -- i.e. the behaviorists and the rational choicers -- to ensure that all newcomers acquire their assumptions and techniques, there is still resistance in all but a few faculties to such a monolithic set of theoretical assumptions, measurement techniques and evaluative norms. Ironically, the social science disciplines from which they have so uncritically drawn these assumptions, techniques and norms – first, social psychology and, more recently, liberal economics -- seem to have become aware of their limitations and have expanded their training programs and research paradigms accordingly. Emphasizing internal logical coherence at the expense of empirical reality and substantive relevance has costs, and not just benefits. Like all “late converts,” their acolytes in political science seem not yet to have learned the lesson of “requisite diversity” in concepts and methods.

Which has not, however, been lost on Europeans and others. They are not only capable of resisting the latest American fads out of a respect for traditional canons of scholarship, but they also are not subjected to the same competitive pressures to conform.

## **V. Efficiency**

For if there is one irresistible force driving the process of professionalization in American political science, it is relentless competition between individual scholars and academic institutions. European capitalists at the turn of the century frequently commented on the “extreme” nature of competition between their Americans counterparts. Firms not only tried by all means available to gain market share, but their objective was to drive other firms out of existence or to subordinate them *via* holdings. Presumably, the conditions that promoted such ruthlessness then are still present in American society – and apply to American political science: a large number of potential suppliers and consumers, the diverse cultural and national backgrounds of an immigrant society, the absence of a national aristocracy or rigid oligarchy, a legal framework regulating “conspiracies against trade,” relatively easy entry into the market/profession, absence of state-imposed bureaucratic coordination, plurality of sources of investment/funding, etc. On virtually all of these scores, European political science communities (especially those in small countries) score low and, hence, whatever competition occurs is strongly limited by social norms and “personal connections.” Some of them even have elaborate formal rules precluding the “raiding” of one faculty by another. The rise in globalization noted in Thesis #1 no doubt has introduced elements of cross-national and even cross-regional competition in hiring, research funding, slots on congress panels, participation in collaborative ventures, etc., but my hunch is that even these are quietly “managed” in a more gentile and cartel-like fashion in Europe.



The most visible consequence of these deeply-rooted differences in competition is, in my opinion, the propensity for fads to swept through the American profession and for their proponents to use whatever momentary advantage they have gained to seek to drive competitors out of their institution or the profession as a whole. Whether the fad is based on concepts, assumptions or methods (and especially when it contains all three as was the case with behaviorism and is now the case with rational choice), any practitioner who does not succumb to their appeal risks being labeled as “unprofessional” or, worse, “unscientific.” The historical result has often been departments that are “layered” into cliques, each having enjoyed momentary prominence, that have very little to say to each other (and many past resentments that they continue to harbor). European “faculties” of political science have had and continue to have many of the same cleavages, but the less intense competitiveness within them seems to leave less persistent scars.<sup>xiii</sup>

## **VI. Universality**

Political science in the United States of America has always labored under a profoundly ambiguous, not to say, schizophrenic pair of assumptions. On the one hand is the notion that this country has been blessed politically by its “exceptionalism.” Thanks to the absence of hostile neighbors and the presence of an open frontier, to the absence of any fierce struggle over feudal privileges and the presence of multiple and overlapping cleavages in an immigrant society, to the absence of profound class conflicts and the presence of a continuous increase in collective wealth, it has avoided many of the “pathologies” of

European politics – not to even mention, the *bizzarries* of belated national liberation from imperial domination. Very few Americans – including virtually all of its political scientists – question the notion that their country has superior political institutions because of its exceptional location and good historical fortune.

On the other hand, these same observers are thoroughly convinced that the United States has the most universal political culture and appeal. They take it for granted that foreigners would naturally prefer, not just an American style of life, but an American standard of politics -- if only they could have it. When undergraduates are introduced academically to the study of politics, the first course they invariably take is based not on a comparison with other countries but a unique exposition of American institutions and behavior. The message is clear and seems to stick: American politics are “normal;” those practiced by others are “abnormal” and definitely “inferior.”<sup>xiv</sup>

# THE INSTITUTIONAL LOCATION OF FIRST AUTHOR (%) OF ARTICLES<sup>(\*)</sup> in YEARS 1996 to 1999 in

*American Political Science Review, Political Studies, Politische Vierteljahresschrift, Revue Française de Science Politique, Rivista Italiana di Scienza Politica, Scandinavian Political Studies*

Institutional Location of First Author (%) & (Number of articles)		American Political Science Review				Political Studies				Politische Vierteljahresschrift				Revue Française de Sciences Politiques				Rivista Italiana Scienza Politica				Scandinavian Political Studies			
Continent	Years Region	96	97	98	99 3 iss.	96	97	98	99 3 iss.	96	97	98	99 2 iss.	96	97	98	99 2 iss.	96	97	98	99 1 iss.	96	97	98	99 3 iss.
North America	Canada		3 (2)				4 (2)	4 (2)	7 (2)					2 (1)	2 (1)							5 (1)			
	USA	93 (42)	92 (50)	97 (48)	91 (33)	6 (3)	18 (9)	25 (12)	7 (2)			4 (2)			2 (1)	3 (1)	15 (2)	16 (3)	14 (2)			15 (3)	11 (2)	15 (3)	13 (2)
Western Europe	Western Europe	2 (1)	3 (2)		5 (2)	10 (5)	12 (6)	4 (2)	14 (4)			13 (6)	13 (2)	25 (10)	2 (1)	6 (2)	15 (2)	27 (5)	7 (1)	26 (4)	16 (1)		5 (1)		6 (1)
	National					76 (38)	57 (28)	51 (24)	64 (18)			81 (35)	86 (13)	71 (28)	86 (33)	90 (28)	69 (9)	55 (10)	78 (11)	73 (11)	83 (5)	80 (16)	83 (15)	84 (16)	80 (12)
Others	Eastern Europe							8 (4)	3 (1)																
	Others	4 (2)		2 (1)	2 (1)	8 (4)	8 (4)	6 (3)	3 (1)						5 (2)										
TOTAL	% Articles	99 (45)	98 (54)	99 (49)	98 (36)	100 (50)	99 (49)	98 (47)	98 (28)			98 (43)	99 (15)	98 (39)	97 (38)	99 (31)	99 (13)	98 (18)	99 (14)	99 (15)	99 (6)	100 (20)	99 (18)	99 (19)	99 (15)

- Articles, Research Notes, Forum, Book Review Essays, Debates, Review Articles, and Focus.

This virtually instinctual parochialism built-into the discipline is well illustrated in Table One, where I have taken the issues from the last four years of the national political science journals of several major producing countries and coded them according to the nationality of the first author of each article, research commentary or bibliographic essay. The American Political Science Review has consistently been the one that has the fewest contributions by scholars in foreign universities or research institutions – 91 to 97% (and that counts Canadians as foreigners!). The closest rival is the Revue Française de Sciences Politiques where French nationals produced between 69% and 90% of its articles. It should be noted that in the cases of Political Studies and Scandinavian Political Studies a substantial proportion of the foreign authors were Americans – as much as 25% in the 1998 volume of Political Studies. Elsewhere, as further confirmation of what we discussed above under the *rubrique* of sequentiality, the growing cosmopolitanism of such journals as Politische Vierteljahresschrift and Rivista Italiana di Scienza Politica is the result of their opening their pages to other Europeans.

Defenders of American hegemony will, of course, reply that this constitutes proof of the superiority of its standards and production. Not only does it dominate its “home market,” but also expanding its share in some of the European markets. Obviously, a test of this proposition would hinge upon a much more detailed examination of the pattern of article submissions and peer reviewing, but I would be willing to advance the hypothesis that very few non-

Americans even bother to submit their work to the APSR because they know that its editorial board is controlled by a particular clique within the American profession that has no interest in their work (unless, of course, it successfully mimics what is fashionable in the States). Fortunately, for these Europeans, there are more specialized journals such as Comparative Political Studies, Comparative Politics and World Politics that are likely to be more receptive – although, even there, the preference for national contributors still seems to be a factor.

My conclusion is that American political science has no especially valid claim to universality; Indeed, its root assumptions and concepts are often thoroughly parochial. Much of the post-war evolution of the sub-field of comparative politics has consisted of country and regional specialists trying to purge the discipline of these distorted elements. The failure of America's most concerted effort at asserting hegemony over the study of "other people's politics," i.e. the structural-functional approach pioneered by Gabriel Almond and the SSRC's Committee on Comparative Politics, can best be interpreted in this light. As its conservative notions about the interdependence of "functions" and its static assumptions about equilibrium became increasingly apparent, it sought refuge in a more European-style of historical institutional analysis<sup>xv</sup> – but it was too late and that fad has been completely exorcised from the profession.

## **VII. Ahistoricity**

It is certainly inaccurate to characterize the United States of America any longer as a "new nation" and, hence, not responsible for the astonishing

ahistoricity of the two most recent fads in its political science. Both behaviorism and rational choice have been (and still are) flagrantly unconcerned with the role played by “memories” of previous conflicts, by unusual sequences of events, by the intervention of particular forces or persons, by complex patterns of interaction under conditions of high uncertainty, by twists and turns of ideological fortune, by diffusion from one case to another, by acquired habits and instincts that are not subject to rapid attitudinal change or momentary calculation, by the arcane processes whereby preferences have been formed and transformed, *e così via*. The best that they have had to say about political change has been limited to notions of “realigning elections,” “iterative games,” “path dependencies” and “punctuated equilibria” – and even these are often invoked only to “explain away” outcomes that would otherwise be inexplicable or irrational. My hunch is that it is precisely the protracted stability, the sheer “taken-for-grantedness” of American political institutions when compared to virtually any other polity in the world, that allows its students of politics to exclude so programatically the unavoidably complex patterns embedded in any historically specified notion of causality. It is not because the United States has no history, but because it has had too long and too continuous a history that students of its politics can be so ahistorical.

Elsewhere, political scientists cannot afford such a luxury. Change in political status, regime, values, rules and behaviors are much more omnipresent features of their respective environments and they have to be “explained,” not “explained away.” What happened long ago or just came before cannot be safely ignored – and this is not just the case within a given polity but across a

sub-set of polities within an interdependent “region.”<sup>xvi</sup> However, once one has made the formation of preferences endogenous to one’s paradigm or introduced the possibility that similarly situated actors might have very different propensities for risk-taking or alliance-formation due to their past experiences, the potential for a parsimonious and self-contained explanation diminishes considerably – as does the intellectual distance from one’s “less scientific” colleagues in history and the other social sciences. To Europeans and their Third World brethren this may not seem so threatening (if only because so many of their political scientists came from these disciplines), but to those Americans avidly bent on establishing their distinctive professional credentials (and imitating their economist colleagues), the prospect is not likely to be welcomed.

### **AN ACADEMIC CONCLUSION**

Political science cannot be “an American science.” No country, no matter how many professionals it has employed or how much of a head-start it has gained, can expect to be the hegemonic producer of the concepts, assumptions and methods that will guide this increasingly globalized discipline in the future. Moreover, the United States of America is singularly (one might say, exceptionally) ill-equipped to play this role since the basic parameters that have conditioned and continue to condition its political life are so different from those that operate elsewhere. Its state-, nation- and regime-building experiences have few parallels in Europe or the Third World. What most of its political scientists take for granted is quite often what is regarded as most problematic by scholars working on the politics of other countries. And the contemporary gap between

what is driving American politics and what is driving “other peoples’ politics” is growing wider, not narrower. If, “as goes the practice of politics, so will (eventually) go the science of politics,” then there is every reason to expect a decline in U.S. hegemony in the future – no matter how hard its political scientists try to prevent it.

Precisely because they are so numerous and self-contained, American political scientists have an unfortunate tendency to ignore what is happening academically and intellectually elsewhere. Precisely because they sincerely believe that the norms and behaviors they study are universal and timeless, they find it difficult to incorporate spatial and historical factors in their work. But there are two “saving graces” of major significance at work within the American profession of political science: (1) its diversity in recruitment; and (2) its insatiable competitiveness. Together, they will ensure that, whatever zealousness and parochialism may characterize it at a given moment, their joint impact will be short-lived. Hopefully, this time the demise of these excesses will be hastened by insights and criticisms coming not just from inside but also from outside the American profession. Only once this has happened and the “American” science of politics has been put in its proper (comparative) place will one be able to speak of an authentically globalized science of politics.

### **A PERSONAL CONCLUSION**

In the United States, ambitious or frustrated persons have been traditionally advised to “Go West, Young Man” where they could expect to find greater freedom to act, receptivity for innovation and tolerance of diversity.



Admittedly no longer a young political scientist, I went West (or, better, returned West) and I there found conformity to power, rampant careerism, hostility to alternative paradigms and a scholasticism indifferent to the concerns of the real world. I am convinced that the maxim should, at least for the moment, be inverted. For those who want to practice a political science that is critical of established power, sensitive to the distinctive nature of its subject matter and capable of explaining the complexities of political life to real people, they would be better advised to “Go East ... and, if possible, now and then, South.” That is where you will be free to question prevailing assumptions, to develop innovative concepts and methods, to address issues of significance and, maybe, even to influence the course of political events. You will also be more likely to make a significant contribution to a globalized science of politics.

#### **\* ENDNOTES \***

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<sup>i</sup> It should be observed that this Handbook is an enormous improvement upon its (English language) predecessors that merely presumed that only U.S. political science was worth considering when assessing “the state of the art.” Goodin and Klingemann are manifestly proud that “just under half of our 42 contributors (have) non-American affiliations” (p. xiii). I cannot resist pointing out, however, that almost all of them are Northern Europeans (German, Scandinavian or British) and the two that are not (Mattei Doggan and Giandomenico Majone) both were either holding or had shortly before held appointments in the United States. As for political science and political scientists not on the transatlantic circuit, one could read the entire essay and not even know that they existed!

<sup>ii</sup> On this issue of convergence, political scientists could profit from reading an important analysis written collaboratively by an (Austrian) economist and a (Dutch) sociologist: Brigitta Unger & Frans van Waarden (eds.), Convergency or Diversity? Internationalization and Economic Policy Response (Aldershot: Avebury, 1995).

<sup>iii</sup> This figure is cited by Dirk Berg-Schlosser who refers to David Easton et al., The Development of Political Science (London: Routledge, 1991) as his source: “Vergleichende europäische Politikwissenschaft – Ansätze einer Bestandaufnahme,” Politische Vierteljahresschrift, 38, 4 (1998), p. 829. My hunch is that this is an exaggeration and, moreover, I am convinced that the relative numerical superiority of the United States is declining.

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iv One must not confuse the exponential increase in the use of English with the spread of American (or, for that matter, British) versions of political science. Needless to say, Americans and Brits do have an initial linguistic advantage and most conceptual innovations will appear first in English simply because of its use as a *lingua franca*, but this is insufficient to ensure that the content and the assumptions behind it will conform to American (or British) usage. More and more national journals within Europe are routinely publishing articles in English as well as their native language – except, of course, for the *Revue Française de Sciences Politiques* – but I would not interpret this as an indicator of growing American hegemony in the profession.

v A similar observation can be inferred from the review article by Kenneth Newton and Josep M. Valles, “Introduction: political science in Western Europe, 1960-1990,” European Journal of Political Science, Vol. 20, Nos. 3-4 (December 1991), pp. 227-238. They make the intriguing suggestion that “once over the threshold requirement of open and democratic government and a relatively high standard of living, political science is not particularly dependent upon a special configuration of social and political circumstances” (p. 229). Europe took longer to get to that threshold – and Newton & Valles specifically note the factor of its “rigid and centralized university systems” in which other, better established, disciplines could better resist the assertion of a distinctive role for political science, in addition to the delay in political freedom and economic affluence compared to the United States.

vi One can forget about using the Social Science Citation Index as a possible test for the relative attractions of U.S. and European political science. The list of journals included is skewed. With few exceptions, anyone having the misfortune of writing or being translated into “non-English” is simply not considered to have made a contribution to knowledge. Only 9% -- 10 out of the 111 journals in political science and international relations monitored by it -- are not published in English. As someone who makes a regular practice of publishing in non-US journals (admittedly, sometimes in English), I can only testify that these are the pieces that often seem to attract the most attention – perhaps, precisely because it so unusual for a non-national to do so.

vii On the inconsequentiality of recent American political science for American politics, see Charles Lindblom, Inquiry and Change: The Troubled Attempt to Understand and Shape Society (New Haven: Yale University Press, 1990).

viii I leave out of this generalization the rather special case of Harvard’s Government Department, several members of which have used their connections with American diplomatic, security and intelligence services to both facilitate their academic advancement and obtain important positions in the policy-making apparatus of the country. Yale’s (former?) relation with the American “intelligence community” was another (less public) exception.

ix For a similar conclusion, see David McKay, “Why is There a European Political Science?” PS: Political Science and Politics, (Fall 1988), p. 1053. McKay does, however, note that Great Britain is an exception in which “the national and local political world is more closed to academics than is the American.” In this and many other regards, British political science is much closer to American practices than to those of the continent.

x The number of affiliated institutions has climbed steadily from 157 in 1992 to 235 in 1998, although one should note that among its “associate members” there has been a growing number of U.S. universities. Recently, participation in its annual “Joint Sessions” has fluctuated from a high of 570 at Madrid in 1994 to a low of 325 at Bochum in 1990, with some 400 attending the last one at Warwick in 1998. It is significant that when it was created in 1970, the ECPR founders chose not to imitate the “three-ring-circus” format of the APSA, but required all participants to choose from a dozen or so options a single, five day workshop on a specific topic. The manifest intent was to assist in the creation of Euro-centric networks and to bring together a critical mass that was often not possible within any one country. Given the subsequent proliferation of sites at which European *politologues* meet routinely, as well as the increase absolute numbers wishing to participate, one can question whether this format has not reached the limit of its utility.

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<sup>xi</sup> See Richard Breen and Daniel Verdier, “Globalization and Europeanization—Part I,” unpublished paper presented to the Workshop on Europeanization, Department of Political and Social Sciences, European University Institute, 31 March 1999 for this conceptualization of the possible outcomes of the integration process and some evidence that what they call “globalization minus” has occurred in some key political arenas.

<sup>xii</sup> Especially when “major” is so frequently defined as “the one in which my clique publishes.” Needless to say, American political scientists rarely consider non-American journals as “major” – and those not published in English are not even taken into consideration.

<sup>xiii</sup> The legacy seems to vary with the type of fad. Behaviorists succeeded in producing a lot of very useful data about the real world so that their decline in academic prominence was mitigated by their finding important (and well-remunerated) employment as pollsters, consultants, TV pundits around election time, etc. “Area Specialists” were never so threatening since their competitive advantage was primarily conceptual and topical – not methodological or epistemological – and they, too, have settled into relatively comfortable enclaves in the profession where they produce important information and analysis about “exotic” countries. One of my worries about the aftermath of the inevitable bursting of the rational choice bubble is that it is going to be much more difficult to find a satisfactory niche for its enthusiasts since they have produced virtually no substantively useful information or findings. One might hope that they would be taken in as “academic refugees” by Departments of Economics, although I doubt this will happen since the assumptions and methods they are applying in political science are already out of date in this discipline. On the issue of vacuousness, see Donald P. Green and Ian Shapiro, Pathologies of Rational Choice: A Critique of Applications in Political Science (New Haven: Yale University Press, 1994).

<sup>xiv</sup> More than in Europe or elsewhere, the American profession of political science tends to be dominated by “Americanists,” i.e. by those who specialize in “their own” politics and policies – whether domestic or foreign. Except for a brief period in the 1960s and 1970s when comparativists seemed to be in the ascendance, most departments and virtually all the journals and associations (except for the “area studies” or “comparative” ones) have been dominated by persons who have had little or no professional experience outside of the United States and who virtually never read, cite or contribute to “foreign” political science. As Gianfranco Pasquino has noted, European political scientists have long been obliged “to go comparative” and would not think of trying to understand their own politics without reference to any other country. “Comparative Politics in Comparative Perspective,” APSA-CP Newsletter, Vol. 9, No. 2 (Summer 1998), p. 8.

<sup>xv</sup> Its last volume edited by Charles Tilly, The Formation of National States in Western Europe (Princeton: Princeton University Press, 1975) is not only European in subject matter, but in conceptual orientation – and almost diametrically opposed to the Committee’s previous work.

<sup>xvi</sup> This may help to explain another particularity of American political science, namely, its rigid distinction between “American – not to mention, comparative -- politics” and “international relations.” Coming from a polity that has always considered that it had nothing to fear or to learn from its neighbors, it must have seemed especially plausible to separate the two so radically and, hence, to presume that they were governed by completely different principles and relatively immune from each other’s influence. In the rest of the world, the persistent effects of imperialism, foreign intervention, economic interdependence and policy diffusion made this assumption manifestly implausible.

## CHAPTER 6A

### WHAT FUTURE FOR COMPARATIVE POLITICS?

Philippe C. Schmitter

Not that long ago, there was a simple and compelling answer to this question: one could just look at what have been the dominant “fads and fashions” in recent American political science, trace their respective trajectories and intercepts, and predict where comparative politics would be going for the next decade or more. Who could doubt that this sub-discipline of political science as practiced in the United States of America showed the rest of the world “the face of its future”? <sup>1</sup> After all, by far the largest

number of professionals applying this method to describing and analyzing the widest variety of polities has always been employed in this country – not to mention the fact that a substantial proportion of those applying it elsewhere have been trained in the USA.

The central assumption of this essay is that the future of comparative politics should (and, hopefully, will) diverge to some degree from the “fads and fashions” followed in recent years by many (if certainly not all) political scientists in the United States. As I have expressed it elsewhere, the sub-discipline is presently “at the crossroads” and the direction that its ontological and epistemological choices take in the near future will determine whether it will continue to be a major source of

critical innovation for the discipline as a whole, or dissolve itself into the bland and conformist “Americo-centric” mainstream of that discipline.<sup>2</sup>

In other words, this essay will not be an effort that even pretends to survey objectively and comprehensively what has been produced by comparativists – American or otherwise – in the recent past. It will be what the French call a *plaidoyer*, a biased plea from a particular (and personal) advocate on behalf of a client who faces a critical “mid-career” choice that will determine his or her status in the future.

First, Some Congratulations are in Order

Let us begin, however, with some self-congratulation. Thanks to the assiduous efforts of many methodologically minded colleagues (mostly Americans, it is true), much fewer students applying the comparative method neglect to include in their dissertations an explicit defense of the cases selected – their number and analogous characteristics, a conscious effort to ensure sufficient degrees of freedom between independent and dependent variables, an awareness of the potential pitfalls involved in selecting the cases based on the latter, a sensitivity of the universe of relevant units and the limits to the external validity of findings ...<sup>3</sup>

These important gains in methodological self-consciousness have produced (or been produced by) some

diminution in the “class warfare” between quantitative and qualitative political scientists. There is still some sniping going on and some of the former persist in asserting their intrinsic “scientific” superiority over the latter, but there is more and more agreement that many of the problems of design and inference are common to both and that the choice between the two should depend more on what it is the one wishes to explain or interpret than on the intrinsic superiority of one over the other – or, worse, how one happens to have been trained as a graduate student. Indeed, from my recent experience in two highly cosmopolitan institutions, the European University Institute and the Central European University, I have encountered an increasing number of dissertations in comparative politics that make calculated and intelligent



use of both methods – frequently with an initial large N comparison wielding relatively simple quantitative indicators to establish the broad parameters of association, followed by a small N analysis of carefully selected cases with sets of qualitative variables to search for specific sequences and complex interactions to demonstrate causality (as well as the impact of neglected and ‘accidental’ factors). To use the imaginative vocabulary of Charles Tilly, such research combines the advantages of “lumping” and “splitting.”<sup>4</sup> Hopefully, this is a trend that will continue into the future.

The real challenge currently facing comparative politics, however, comes from a third alternative, namely, “formal modeling” based on rational choice assumptions. Much of

this stems from a strong desire on the part of American political scientists to imitate what they consider to be the “success” of the economics profession in acquiring greater status within academe by driving out of its ranks a wide range of dissident approaches and establishing a foundation of disciplinary orthodoxy (neo-liberalism) upon which all their research is based. This path toward the future would diverge both methodologically and substantively from the previously competing quantitative and qualitative ones. It would involve the acceptance of a strong set of limiting initial assumptions, exclusive reliance on the rational calculations of individual actors to provide “micro-foundations,” deductive presumptions about the nature of their interactions and reliance on either ‘stylized facts’ or ‘mathematical illustrations’ to

prove the correctness of hypotheses. The comparative dimension enters into these equations to demonstrate that individual behavior is invariant across units or, where it is not, that institutions (previously chosen rationally) do make a difference.

## The 'Genealogical Tree' of Comparative Politics

Place Figure One Here

Figure One is a spatially schematized and temporally compressed representation of the genealogical roots, trunks and branches that have evolved into contemporary comparative politics.

Its deepest root lies in something I have called “sociological constitutionalism” as invented by Aristotle and subsequently nourished by such a diverse group of “Dead White European Males” as Polybius, Machiavelli, Montesquieu, Constant, de Tocqueville, von Stein, Marx, Ostrogorski, Weber, Durkheim, Michels, Mosca and Pareto. Through various extensions and permutations this has become the branch subsequently labeled as “historical political sociology” with such luminaries as Stein Rokkan, T. H. Marshall, Reinhard Bendix, Otto Kirchheimer, Seymour Martin Lipset, Juan Linz, Hans Daalder, S. N. Eisenstadt, Harry Eckstein, Dankwart Rustow, Gerhard Lehmbruch, Erik Allardt, Charles Tilly, Guy Hermet, Arendt Lijphart and Peter Katzenstein located somewhere along it. Karl Deutsch probably should be placed here at

an odd angle, since he was so single-handedly responsible for inserting a cybernetic graft into it. On the outer reaches of this cluster is where I can be most safely located.

The other deep root lies in “legal constitutionalism” fertilized initially by distinguished Anglo-French jurists such as Leon Duguit, Georges Burdeau, James Bryce, A. Lawrence Lowell and Woodrow Wilson, and developed during the subsequent century by scholars such as Maurice Duverger, Herman Finer, Samuel Finer, Giovanni Sartori, Carl J. Friedrich, Samuel Beer, Jean Blondel and Klaus von Beyme. Someone like Robert Dahl can probably be best located hanging comfortably between both the sociological and legal branches – which, in any case, have

been converging for some time. Samuel Huntington is another distinguished comparativist whose roost in the tree is difficult to place, although it is easier to imagine him hanging on this branch rather than on the neighboring one.

From these two tap roots have been added a number of exogenous grafts during the 20th century. Political science became a voracious consumer of conceptual and methodological innovations from other, increasingly professionalized, social science disciplines – first, from social psychology with the so-called “behaviorist movement” and later (and somewhat more surreptitiously) from anthropology with the “structural-functionalist approach.” The most distinctive product of

the former was the rapid rise of comparative survey research, symbolized by the publication of the highly successful (and criticized) work, The Civic Culture, in 1963. Today, this branch of comparative politics is routinely conducted within and often across virtually all of the world's polities by scholars such as Sidney Verba, Rudolf Wildenmann, Richard Rose, Alex Inkeles, Ronald Inglehart, Max Kase, Giacomo Sani, Richard Gunther, José Ramon Montero, and Hans-Dieter Klingemann. Certainly, it is the most distinctive (and successful) contribution of American political science to the sub-discipline.

The anthropological graft has contributed much less in volume and attractiveness to the evolution of comparative politics. Its most important contribution was undoubtedly

to preside over a vast extension in the range of countries brought under comparative scrutiny. When embracing “Non-Western” politics and faced with the need to explain “elections in Albania,” “budgeting in Zaire,” “civil-military relations in Indonesia,” and “federalism in Argentina,” scholars such as David Apter, Leonard Binder, Lucian Pye, James Coleman and Myron Weiner found it difficult to apply the usual legal or sociological categories and took refuge behind a variety of “functions” that presumably had to be performed by analogous “structures” in all political systems if they were to remain stable. After a major flurry of activity in mid-1950s to the early 1970s under the prestigious auspices of the SSRC Committee on Comparative Politics, scholars began to realize that the stipulated functions were excessively abstract and that the



structures they were trying to explain often could not be assigned to a single one of them. Moreover, the entire notion of “systemic equilibrium” as the central metaphor for guiding comparisons among Non-Western polities came into question when the stability of their institutions was revealed to be highly precarious. Once the key question was seen to be change, especially change in regime from democracy to autocracy or, more recently, the inverse, the approach became much less relevant – as exemplified by one of its most prestigious proponent’s shift to an approach that emphasized “crisis, choice and change.”<sup>5</sup>

Finally, comparativists have always borrowed ideas and concepts from economics, especially from such early

political economists as Adam Smith, Marx, J. S. Mill, Ricardo and Bentham. Albert Hirschman, although a heterodoxical figure among the ranks of contemporary neo-liberal economists, has made several seminal contributions. But the real novelty of the past few decades has been the transfer of root assumptions, deductive thinking and mathematical modeling techniques into the study of politics – first, in research on American politics and, increasingly, into research on “other people’s politics.” The leading figures have been Anthony Downs, Thomas Schelling, Howard Raiffa, Kenneth Arrow, Douglas North, Mancur Olson, Gary Becker, George Stigler, and, most centrally, James Buchanan and William Riker. As we shall see shortly, this graft from economics has opened up a radically new path to the future for comparativists.

Presently, the evolutionary tree of comparative politics resembles more a tropical Banyan than a Florentine cypress. It has a wide canopy of branches, certainly not a single tapered and elegant peak. Its most curious aspect, however, is the number of practitioners who are roosting up there, and who seem content with sharing the same generic label: institutionalists. Closer inspection of the foliage reveals that it contains an extraordinary variety of flora and fauna. About all these comparativists seem to agree upon is that "institutions matter." They differ widely on what institutions are, why is it that they matter, and which ones matter more than others. Moreover, some of those perched up there (such as myself) will even admit that other things also matter: collective identities, citizen

attitudes, cultural values, popular memories, external pressures, economic dependencies, even instinctive habits and informal practices – not mention the old favorites of Machiavelli, *fortuna* and *virtù* – when it comes to explaining and, especially, to understanding political outcomes. This urge to find shelter under the capacious tent of “institutionalism” (sometimes prefaced with a “neo-”) can either be interpreted as a bizarre effort to return to their legalistic origins (precisely in a world context in which such formalized constraints are manifestly inadequate for solving problems and resolving conflicts) or as a desperate attempt to make common cause with the greatest possible number of disciplinary brethren (precisely when these so many of them are

heading in a direction that would radically challenge their basic assumptions and methods) .

### At the Crossroads of Three Paths

At the top of Figure One, there is a large question mark – a decision point that could determine the future configuration and even the very viability of the whole tree. The safest thing one can say today about the future of comparative politics is that it will not be the same as in the past.

Of course, not everything is going to have to change. Comparative politics will continue for the foreseeable future to bear major responsibility for the description of

processes and events in “other people’s countries” and, hence, to providing systematic and reliable information to those politicians – in and out of power – and to those administrators – at the top and bottom – charged with making and implementing national policies concerning these countries. The end of the Cold War and collapse of the Soviet Empire has, if nothing else, led to an impressive increase in the sheer number of polities whose (allegedly autonomous) behavior has to be described. The globalization of capitalism has produced increasingly indirect and articulated systems of production, transport and distribution that are much more sensitive to disturbances in the behavior of their most remote and marginal components. The ubiquitous penetration of the mass media has meant the happenings anywhere in the

world are being immediately transmitted everywhere and comparativist pundits will be expected "to place them in context" for public consumption.

Comparison between "real-existing polities" will also remain the best available method for analyzing similarities and differences in behavior and for inferring the existence of patterns of regularity with regard to the causes and consequences of politics. It may be the second best instrument for this purpose, but as long as it remains impossible for students of politics to experiment with their subjects and subject matter, political scientists will have to settle for analyzing as systematically as possible variations they do not control.

Figure One suggests that the sub-discipline has to choose between three distinctive paths. It can continue along the mainstream and very broad “institutionalist” trajectory it has been on for the last decades, presumably adding more “neo-neo-neo-” prefixes to itself. Otherwise, it can take a sharp turn to either the left or the right. Whatever the choice, it is most unlikely that comparative politics will taper toward a single peak – however much some practitioners would like it to.<sup>6</sup> The most “clear and present” danger, as I see it, is that the sub-discipline’s evolution will lead to an irreversible split in the canopy.

No doubt, many of its practitioners will continue with “business as usual” by comparing the political behavior in



and of national states under the very capacious (but very patch-worked) label of institutionalism.

Others will take a sharp turn to the right towards economics by opting for 'simplification.' They will be led by those American colleagues who have accepted the limited initial assumptions, exclusive reliance on individualistic "micro-foundations," deductive presumptions about how these actors behave with regard to each other, and proof by 'stylized facts' or 'mathematical illustrations' that characterize the path known as rational choice.<sup>7</sup>

Still others will choose the leftward path by opting for what I call "complexification." They will follow the lead

of a much more cosmopolitan and trans-disciplinary group of scholars who:

(1) accept far fewer and less restrictive initial assumptions – indeed, who rely upon a calculated proliferation of assumptions about the identity and motives of actors;

(2) are convinced that adequate micro-foundations in the present world context cannot be only individualistic – indeed, they have to take more-and-more into consideration the composition effects generated by multiple levels of political power and authority;

(3) rely upon 'reasonableness' rather than rationality, i.e. on 'improvising' and 'avoiding the worst" in complex situations where maximization of marginal returns is virtually impossible given the number of actors, the plurality of sources of information and the unintended consequences generated by interdependent layers of political aggregation;

(4) consider that the usual fallacies of composition can be converted into novel "laws of composition" to explain outcomes in situations where multiple layers of different types of actors from a plurality of centers of power and authority bargain and deliberate with each other;

(5) have a healthy respect for 'real' data – whether generated by the normal operations of the polity or invented by themselves, coupled with an abiding suspicion of simple aggregative indicators for complex phenomena or of so-called stylized facts; and, finally,

(6) insist upon endogenizing as many potentially causal variables as possible, even those notoriously difficult to measure such as "preferences," rather than shoving them into the background, assuming them out of existence or presuming what values they take in a given situation.

The impending competition between the three alternative paths depicted at the top of Figure One is hardly going to be equal. The middle one, toward “neo-neo-neo-institutionalism,” should clearly be the most favored choice, if only due to sheer inertia and the fixed intellectual assets of most practicing comparativists. Given the profusion of qualifiers that usually precede it – historical, sociological, legal and rational, just to name the most common ones – the approach is sufficiently ambiguous to be appealing to a large number of them for some time to come, even if it is subject to diminishing marginal returns, fuzzier and fuzzier specifications, and less and less capacity to deal with anomalies.

The sharp right turn toward the simplicity of formal modeling should be (and already has been) very tempting, especially in the United States, for reasons I have discussed elsewhere.<sup>8</sup> Most saliently, those who take this path consider themselves likely to reap the same rewards from higher 'scientific' status as have the neo-liberal, mathematized economists from whom they have lifted their intellectual baggage – "lock, stock and barrel." They can also be assured that their work will only be understood by a small group of *conoscenti*.<sup>9</sup> Even if "economic science" has been notoriously unsuccessful in predicting the rate/direction/location of change in the macro-economy and even if it has suffered recently some notorious defections from within its ranks, it still remains very prestigious in the eyes of other social scientists.

Public or rational choice is by now firmly entrenched in a dominant strategic position within several leading departments and journals in the United States – although it has recently provoked a strong reaction from non-believers within the profession gathered under the Perestroika banner (a substantial proportion of whom seem to have been comparativists).<sup>10</sup>

The leftward turn toward complexity does not have any such disciplinary prototype to follow. At best, it can only pick up assumptions, concepts and ideas from a scattered diversity of sources, some from abstruse disciplines in the physical and mathematical sciences that deal with cybernetics, advanced circuits, complex systems, chaotic behaviour, and so forth. Recent developments in

international relations theory, e.g. Nye and Keohane's exploration of "complex interdependence,"<sup>11</sup> Hayward Alker's ,<sup>12</sup> and the work by Gary Marks and Lisbet Hooghe on the EU as a "multi-layered system of governance,"<sup>13</sup> all point in the right direction, but they do not by any means add up to a comprehensive, coherent and consistent theory.<sup>14</sup> Indeed, virtually none of these sources refer to each other and even less do they offer a cumulative perspective that can subsume previous knowledge, explain new puzzles, provide counter-intuitive answers, and stand up to repeated attempts at falsification by both its practitioners and its competitors. Sending present and future students of "other people's politics" down such an un-explored (and unfashionable) path may well seem like the height of folly. Quite understandably,



beginning political scientists need to identify with a research program that is already flourishing and promising – and, not coincidentally, likely to provide them eventually with better career opportunities. Comparativists may be even more susceptible to such a bandwagon effect since they are unusually dependent upon theory to identify the basic analogies between cases and to sustain the external validity of their findings. While I would argue that embracing complexity would be more “progressive” in the Laktosian sense than the other two (both of which I regard as already “degenerative”), I would be the first to admit how difficult it is going to be to convince vulnerable younger scholars that this is the case.

By now, it will come as no surprise to the reader that I am strongly in favor of “tilting” the future evolution of comparative politics toward embracing rather than rejecting complexity. Perhaps, it is psychologically understandable that just when the surrounding political world has rapidly become more interdependent across units and more assertive within them – i.e. when national states are becoming less sovereign externally and internally – that students would seek refuge in simplicity, parsimony and consistency. To many of those contemplating the sheer “messiness” and “noisiness” of contemporary politics at the national, sub-national and supra-national levels, it must be profoundly comforting to imagine that, under all of this, there lies some easily specified and widely comprehensible ‘model’ for

explaining what is going on. The fact that this model may not be very good at predicting future outcomes becomes less important than its utility in retrodicting past ones (and then only provided that one has eliminated much of the allegedly random “mess and noise”). If, however, it is still true that “a theory can be judged by the range and apparent verisimilitude of the predictions it makes about the world” – and not just by its formal elegance or logical consistency – then, I can see no viable alternative for comparativists than to confront the messy and noisy world we live and design our theories accordingly.<sup>15</sup>

## Coping with a Messy and Noisy World

The core of my argument is that comparative political analysis, if it is to remain significant, productive and innovative in the future, has to reflect the “real-existing” environment from which it should draw its observations and to which it should refer its findings. If it fails to monitor what is going on around it or reports only trivial or irrelevant results, it will become a residual component in the political science curriculum – a sort of “exotic extra” for the delectation of undergraduates.

One thing that differentiates comparativists from their colleagues who only study one polity or one international system is supposed to be greater sensitivity to contextual factors that are so deeply embedded that they are taken for granted or treated as “exceptional” by Americanists or

International Relations specialists.. Inversely, they should be uniquely equipped to identify and incorporate the trends that affect – admittedly, to differing degrees – virtually all of the world's polities.

Two of these trends, in my opinion, are sufficiently pervasive as to affect the basic design of comparative research. They are: (1) increased complexity; and (2) increased interdependence. However independent their sources may be – for example, logically speaking, a polity may become more complex without increasing its interdependence upon other polities and a polity may enter into increasingly interdependent relations with others while reducing its internal complexity through specialization – these two trends tend to be related and,

together, they produce something that Joseph Nye and Robert Keohane have called "complex interdependence."<sup>16</sup>

The implication that I draw from this is that complex interdependence is having an increasing influence not just on the substance of politics, but also upon its form. It is changing, in other words, the units that we should be using for specifying our theories and collecting our data and the levels at which we should be analyzing them.

Complexity: This undermines one of the key assumptions of most of traditional comparative political research, namely, that the variable selected and observed with

equivalent measures will tend to produce the same or similar effect(s) across the units being compared.

Interdependence: This undermines the most important epistemological assumption in virtually all comparative research, namely, that the units selected for comparison are sufficiently independent of each other with regard to the cause-effect relationship being examined.

Complex Interdependence: The 'compound' condition makes it difficult, if not impossible, to determine what constitutes an independent cause (and, hence, an independent effect) and whether the units involved have an independent political capacity to choose and

implement (and, therefore, to act as agents connecting cause and effect).

When Aristoteles gathered data on the 'social constitutions' of 158 Greek city-states, he set an important and enduring precedent. The apposite units for comparison should be of the same generic type of polity and at the same level of aggregation. And they should be more-or-less self-sufficient and possess a distinctive identity. Since then, almost all theorizing and empirical analysis has followed this model. One could compare "empires" or "alliances of states" or "colonies of states," but not across these categories. Most of all, the vast proportion of effort has gone into studying supposedly 'sovereign' states whose populations shared a supposedly



unique 'nationality.' It was taken for granted that only these 'sovereign-national' polities possessed the requisite capacity for "agency" and, therefore, could be treated as equivalent for comparative purposes. Needless to say, large N comparisons of all United Nations member-states rested on this fiction – and have rarely produced convincing findings. Even area specialists working with geographically or culturally denominated subsets of countries in Latin America, Sub-Saharan Africa, the Middle East and North Africa, and South, South-East or North-East Asia occasionally have had to face this issue of inference and external validity. Was Honduras in the 1950s when a substantial portion of its territory was owned by foreign banana producers "really" comparable with the much larger Brazil whose then major export resource, coffee,

was in native hands? What is the utility of comparing the fiscal systems of Kuwait that rests virtually exclusively on petroleum derived revenues with that of Jordan that depends largely on foreign aid and its own citizens? Comparativists who pioneered in the application of their method to sub-units of the same federal or decentralized polity had to limit themselves to circumscribed issues for which these units possessed some equivalent autonomy of action.

In the contemporary setting, due to differing forms of complexity and degrees of interdependence, as well as the compound product of these two, it has become less and less possible to rely on the properties of sovereignty and nationality to identify equivalent units. No polity can

realistically connect cause and effect and produced intended results without regard for the actions of others. Virtually all polities have persons and organizations within their borders that have identities, loyalties and interests that overlap with persons and organizations in other polities.

Nor can one be assured that polities at the same formal level of political status or aggregation will have the same capacity to agency. Depending on their insertion into multi-layered systems of production, distribution and governance, their capacity to act or react independently to any specific opportunity or challenge can vary enormously. This is most obviously the case for those national states that have entered into supra-national

arrangements such as the European Union or signed binding international treaties such as those of the IMF or the WTO. Not only do they occasionally find themselves publicly shamed or found guilty by such organizations, but they regularly anticipate such constraints and alter their behavior accordingly. Moreover, many contemporary national polities have granted or been forced to concede extensive powers to their sub-national units and, in some cases, these *estados autonómicos*, *provinces*, *regioni*, *länder*, *municipios* and *communes* have entered into cooperative arrangements with equivalent units in other national states.

From these observations, I conclude not only that it is literally absurd to compare only at the level of individuals,

but also that comparativists need to dedicate much more thought to the collectivities they do choose and the properties these units of analysis supposedly share with regard to the specific institution, policy or norm that is being examined. Try to imagine someone studying the extent of commitment to environmental policies across European polities without reference to the European Union. Or another scholar comparing the human rights record of African states without taking into consideration the conditionalities posed by bi-lateral and multi-lateral foreign aid programs. I would admit that, in neither of these examples, should one presume that all variation in behavior or outcome can be explained by supra-national linkages. There still remains a great deal of difference that can only be explained by conditions within national

polities, but exorcising or ignoring the complex external context in which these units are embedded would be equally foolish.

## Polities not (just) States

The practice of comparative political research does follow and imitate changes in “real-existing politics” – but always with a considerable delay. As I mentioned above, the most important set of general changes that have occurred in recent decades involves the spread of “complex interdependence.” There is absolutely nothing new about the fact that formally independent polities have extensive relations with each other. What is novel is not only the sheer magnitude and diversity of these exchanges, but also the extent to which they penetrate into virtually all social, economic and cultural groups and

into almost all geographic areas within these polities. Previously they were mainly concentrated among restricted elites living in a few favored cities or regions. Now, it takes an extraordinary political effort to prevent the population anywhere within national borders from becoming “contaminated” by the flow of foreign ideas and enticements. “Globalization” has become the catch-all term for these developments, even if it tends to exaggerate the evenness of their spread and scope across the planet.

Globalization has certainly become the independent variable – the ‘first mover’ – of contemporary political science. It can be defined as an array of transformations at the macro-level that tend to cluster together, reinforce each other and produce an ever accelerating cumulative impact. All of these changes have something to do with

encouraging the number and variety of exchanges between individuals and social groups across national borders by compressing their interactions in time and space, lowering their costs and overcoming previous barriers – some technical, some geographical, but mostly political. By all accounts, the driving forces behind it have been economic. However, behind the formidable power of increased market competition and technological innovation in goods and services, lies a myriad of decisions by national political authorities to tolerate, encourage and, sometimes, subsidize these exchanges, often by removing policy-related obstacles that existed previously – hence, the close association of the concept of globalization with that of liberalization. The day-to-day manifestations of globalization appear so natural and



inevitable that we often forget they are the product of deliberate decisions by governments that presumably understood the consequences of what they have decided to *laisser passer* and *laisser faire*.

Its impact upon specific national institutions and practices is highly contentious, but two (admittedly hypothetical) trends would seem to have special relevance for the conduct of comparative political inquiry:

(1) Globalization narrows the potential range of policy responses, undermines the capacity of (no-longer) sovereign national states to respond autonomously to the demands of their citizenry and,

thereby, weakens the legitimacy of traditional political intermediaries and state authorities;

(2) Globalization widens the resources available to non-state actors acting across national borders and shifts policy responsibility upward to trans-national quasi-state actors – both of which undermine formal institutions and informal arrangements at the national level, and promote the diffusion of new trans-national norms of political behavior.'

If either of these is true and especially if both are, then, a major "paradigm shift" is likely to occur – whichever of the paths in Figure One is selected.

Comparativists have occasionally given some thought to the implications of these developments for their units of observation and analysis, but have usually rejected the need to change their most deeply entrenched strategy, namely, to rely almost exclusively upon the so-called “sovereign national state” as the basis for inferring similarities and differences in response to the impact of national variation in independent conditions. They (correctly) observe that most individuals still identify primarily (and some exclusively) with this unit and that national variables when entered into regressions or cross-tabulations continue to predict a significant amount of variation in attitudes and behavior. Hence, if one is researching, say, the relation between gender and voting preferences, most of the subjects surveyed will differ from

national state to national state – and this will usually be greater than the variation between sub-units within respective national states.

While I would concede this assumption for comparative analyses when based exclusively on behaviour and attitudes at the individual level, I am convinced that the same does not hold for the behaviour of meso- and, especially, macro-units of binding collective choice. Due to differing forms of complexity, differing degrees of interdependence and differing compounds of both of these conditions, no polity can realistically connect cause and effect through its own institutions and policies without regard for the actions of others. Virtually all of them have persons and organizations within their borders that have

identities and loyalties that overlap with those of other polities; virtually all decisional units within national states are affected by “extra-national” events over which they have quite limited control. The days when such exchanges only passed through Foreign Offices and were governed by international treaties or formal bilateral arrangements are over. Regions, provinces, even municipalities engage in external relations with each other; trade unions and professional associations become part of overarching regional and international peak organizations; economic sectors and industries include firms from many different countries; social movements regularly exchange their programs and adopt each others’ tactics. Admittedly, political parties remain among the most national in their organization and ideology (and, not infrequently, are

legally prohibited from receiving foreign funds), but they do meet with each other often, join “internationals,” coordinate their appeals and sometimes even support each others’ candidates.

My conclusion is that it has become less and less appropriate to rely on the properties of sovereignty, nationality and stateness for identifying the relevant units for theory and inference. No doubt, comparative politics at the descriptive level will continue to dedicate most of its effort to sovereign national states. That is the level at which such information is normally consumed by policy-makers, the media and the public at large. But at the analytical level, it will have to break through that boundary and recognize that units with the same formal

status, e.g. all members of the United Nations or of some regional organization, may have radically different capabilities for taking and implementing collective decisions – and that virtually no national state can afford to presume that it is politically sovereign, economically self-sufficient and culturally distinct.

A first step in research design would be to compare units at different levels of spatial or legal aggregation, provided they had similar properties and capacities with regard to the problem being studied. Another would be to ignore the boundaries imposed by area studies and try to identify units of analysis that share similar patterns of complex interdependence, regardless of their cultural or geographic propinquities. Yet another would be to shift to

functional criteria and compare economic sectors with similarly layered production and marketing arrangements. The most audacious would be to search for similarly configured, territorial or functional, patterns of power and authority and the effects they produce – wherever they were located spatially, culturally or even temporally.<sup>17</sup>

The most difficult challenge, however, will come from abandoning the presumption of “stateness.” Sovereignty has long been an abstract concept that “everyone knew” was only a convenient fiction, just as they also “knew” that almost all states had social groups within them that did not share the same common political identity. One could pretend that the units were independent of each other in



choosing their organizations and policies and one could get away with assuming that something called "the national interest" existed and, when invoked, did have an impact upon such collective choices. But the notion of stateness impregnates the furthest corners of the vocabulary we use to discuss politics – especially stable, iterative, "normal" politics. Whenever we refer to the number, location, authority, status, membership, capacity, identity, type or significance of political units, we employ concepts that implicitly or explicitly refer to a universe composed of states and "their" surrounding national societies. It seems self-evident to us that this particular form of organizing political life will continue to dominate all others, spend most publicly generated funds, authoritatively allocate most resources, enjoy a unique

source of legitimacy and furnish most people with a distinctive identity. However we may recognize that the sovereign national state is under assault from a variety of directions -- beneath and beyond its borders -- its "considerable resilience" has been repeatedly asserted.<sup>18</sup> To expunge it (or even to qualify it significantly) would mean, literally, starting all over and creating a whole new language for talking about and analyzing politics.

The assiduous reader will have noted that I have already tried to do this by frequently referring to "polity" when the normal phrase should have been "state." I confess that I first became aware of this lexical problem when working on what is, admittedly, an extreme case, namely, the European Union. I then asked my reader to try to imagine

a polity that did not have the following: (1) a single locus of clearly defined supreme authority; (2) an established, and relatively centralized hierarchy of public offices; (3) a pre-defined and distinctive "public" sphere of *compétences* within which it can make decisions binding on all; (4) a fixed and (more-or-less) contiguous territory over which it exercises authority; (5) an unique recognition by other polities and exclusive capacity to conclude international treaties; (6) an overarching identity and symbolic presence for its subjects/citizens; (7) an established and effective monopoly over the legitimate means of coercion; (8) a unique capacity for the direct implementation of its decisions upon intended individuals and groups; and (9) a uncontested potential ability to control the movement of all goods, services, capital and persons within its borders.

Now, the European Union has yet to acquire these properties – which is quite different from those well established national states that have been losing some of them. I will also admit that there is a lot of variation in stateness among such units and that the actual and aspiring members of the EU have moved much further in this direction than others. Europe is unique as a region in which supra-national policy cooperation has been generating norms – some 80,000 pages of them it is alleged – that condition the choices and organizations of its member polities, literally on a day to day basis. Nothing remotely like this has come from NAFTA, MERCOSUR, CACOM or ASEAN. Nevertheless, the polities of

North America, South America, Central America and South-East Asia are all ensnared in a growing network of supra-national norms and sometimes adjudication mechanisms that call into question many of the nine "imaginary" dimensions set out above. "Conditionality" may be a vague term and its efficacy is often doubtful, but no one can question that organizations such as the United Nations, the World Bank, the International Monetary Fund, the World Trade Organization and a myriad of other global and regional "inter-governmental organizations" are having some effect "beyond the nation state." And this is not to mention the much more numerous "non-governmental organizations" with direct links to political groups "beneath the nation state."

Before comparative politics can embrace complexity, it will have to admit to a much wider variety of types of decision-making units and question whether those with the same formal status are necessarily equivalent and, hence, capable of behaving in a similar fashion. As we shall now see, this is very likely to involve paying much more attention to “patterns” than to “variables.”

## Patterns not (just) Variables

Contemporary comparative politics has tended to focus on variables. The antiquated version tried to explain the behaviour of whole cases – often one of them at a time.

The usual approach has been to choose a problem, select some variable(s) from an apposite theory to explain it, decide upon a universe of relevant cases, fasten upon some subset of them to control for other potentially relevant variables, and go a searching for “significant” associations. Not only were the units chosen presumed to reproduce the underlying causal relations independently of each other, but each variable was supposed to make an independent contribution to explaining the outcome. We have already called into question the first assumption, now let us do the same with the second.

Complexity requires that one attempt to understand the effect(s) of a set of variables (a “context” if you will) rather than those of a single variable. And, normally, the

problem or puzzle one is working on has a multi-dimensional configuration as well. In neither case is it sufficient simply to standard score and add up several variables (as one does, for example, with such variables as economic or human development, working class militancy, ethnic hostility, quality of democracy, rule of law, etc.). The idea is to capture the prior interactions and dependencies that form such a context and produce such an outcome. In other words, the strength of any one independent variable depends on its relation with others, just as the importance of any chosen dependent variable depends on how and where it fits within the system as a whole.



In political terms, this can be (partially) captured by invoking two terms that have been used to describe the European Union – my assumption being again that this regional polity represents an extreme case, even a *reductio ad absurdum*, of the direction in which all polities are heading (admittedly at very different paces):

Multi-Level Governance: an arrangement for making binding decisions that engages a multiplicity of politically independent but otherwise interdependent actors – private and public – at different levels of territorial aggregation in more-or-less continuous negotiation, and that does not assign exclusive policy *compétence* or assert a stable hierarchy of political authority to any of these levels.

Poly-centric Governance: an arrangement for making binding decisions over a multiplicity of actors that delegates authority over functional tasks to a set of dispersed and relatively autonomous agencies that are not controlled – *de jure* or *de facto* – by a single collective institution.

To the extent that the polities one is comparing have something like these characteristics, they are both spatially and functionally complex. NB this is not just a “supra-national” property, even if they are most prominently on display in the European Union. A polity may be relatively free of constraints imposed by global or regional organizations (or by hegemonic neighbors) and

still find itself in a situation of multi-level and poly-centric governance due to the autonomous behavior of sub-national units or functionally specific agencies. Consider, for example, the United States which under its present government is most emphatic about its lack of accountability to international law and organizational constraints (that it does not control) on the issue of stem-cell research where it has been unable to exert sovereignty over sub-units and agencies that oppose the policy of the central state. Infra-national units can contribute just as much to the complexity of politics as supra-national ones.

In addition to major implications for the operationalization of indicators – i.e. simple aggregate

measures of individual or collective behavior will not do – complex interdependence within and between polities raises the prospect of major fallacies of inference between different levels of analysis. Properties reliably observed at the micro-, meso- or macro-levels may be poor predictors of what happens at other levels, lower and higher. Just as one cannot simply add up a large numbers of “civically cultured” and “democratically minded” citizens in order to produce a democracy, one cannot guarantee that conditionalities imposed by regional organizations or hegemonic democratic neighbors will suffice to ensure a successful transition from autocracy to democracy – or even to have an important impact. The correct inference may depend on context, i.e. upon the multiple layers

involved and the possible existence of a plurality of competing centers of authority.

It is no coincidence that both of these descriptive qualifiers are attached to the same substantive concept: governance. No student of comparative politics over the past two and a half decades can have ignored the amazingly rapid and widespread diffusion of this concept. Whatever its opportunistic origins or the ambiguous meanings attached to it, I am convinced that behind the notion of governance lies an important message about changes in the practice of "authoritatively allocating values" (as Easton so eloquently put it). Government, doing so through a hierarchically disposed and legitimately recognized set of public institutions, is less

and less capable of doing this – especially when confined to a single level of spatial aggregation. What it takes is something much more complex which I have elsewhere described as:

a method/mechanism for dealing with a broad range of problems/conflicts in which actors, private as well as public, sub-national and supra-national as well as national, regularly arrive at mutually satisfactory and binding decisions by negotiating and deliberating with each other and cooperating in the implementation of these decisions.<sup>19</sup>

The core of such a complex arrangement rests on horizontal forms of interaction between actors who have

conflicting objectives, but who are sufficiently independent of each other so that neither can impose a solution on the other and yet sufficiently interdependent upon each other so that both would lose if no solution were found.<sup>20</sup> In both modern and modernizing societies, some of the actors involved in governance are non-profit, semi-public and, at least, semi-voluntary organizations with leaders and members; and it is the embeddedness of these organizations into something approximating a civil society that is crucial for its success. These organizations do not have to be equal in their size, wealth or capability, but they have to be able to hurt or help each other mutually.

Also essential is the notion of regularity. The participating organizations interact not just once to solve a single common problem, but repeatedly and predictably over a period of time so that they learn more about each other's preferences, exchange favors, experience successive compromises, widen the range of their mutual concerns and develop a commitment to the process of governance itself. Here, the codewords here tend to be trust and mutual accommodation – specifically, trust and mutual accommodation between organizations that effectively represent more-or-less permanent social, cultural, economic or ideological divisions within a society or across several of them.



Note also that governance is not just about making decisions via deliberation and negotiation, but also about implementing policies. Indeed, the longer and more extensively it is practiced, the more the participating organizations develop an on-going interest in this implementation process since they come to derive a good deal of their legitimacy (and material rewards) from the administration of mutually rewarding programs.

Governance is not a goal in itself, but a means for achieving a variety of goals that are chosen independently by the multiple layers of actors involved and affected. *Pace* the frequent expression, "good governance," resort to it is no guarantee that these goals will be successfully achieved. It can produce "bads" as well as "goods."

Nevertheless, it may be a more appropriate method than the more traditional ones of resorting to public coercion or relying upon private competition.

Moreover, it is never applied alone, but always in conjunction with state and market mechanisms. For “governance” is not the same thing as “government,” i.e. the utilization of public authority by some subset of elected or (self-)appointed actors, backed by the coercive power of the state and (sometimes) the legitimate support of the citizenry to accomplish collective goals. Nor is it just another euphemism for the “market,” i.e. for turning over the distribution of scarce public goods to competition between independent capitalist producers or suppliers. It goes without saying that, if this is the case, the legitimacy

of applying governance to resolving conflicts and solving problems will depend upon different principles and operative norms than are used to justify the actions of either governments or markets.

This concept of “governance” will not suffice to bear all of the weight imposed upon future comparativists who turn toward complexity. But it is definitely a start point for those setting off in that direction.

## Concluding Thoughts

“Complexity” is still only a specter haunting the future of comparative politics. Unless it can attract successful grafts from disciplines in the physical sciences that deal

with analogous situations, it may well fade away. Just picking up a few scattered concepts from within political science, such as multi-layeredness, polycentricity and governance, will not carry comparativists very far. And there is always the possibility that simplifiers will find their way to more relevant topics, manage to decompose complex processes in parts that can be modeled, and make more serious efforts to falsify their assumptions and hypotheses.

Nevertheless, I cannot escape the conviction that this is the most promising path forward. The emergence of a new instrument, Qualitative Comparative Analysis (QCA), has given it some initial momentum. Its originator, Charles Ragin, has been unusually articulate (for a methodologist)

about the implications it has for theory, in general, and comparison, in particular.<sup>21</sup> Especially in its “fuzzy set” and “two stage” versions, QCA offers a far more appropriate estimation technique for complex causality than the usual ones from social statistics.<sup>22</sup>

And it seems uniquely capable of explaining something that I think will become more and more important in the future, namely, equifinality. Since its Aristotelian origins, the comparative method has been applied mainly to explaining differences. Why is it that polities sharing some characteristics, nevertheless, behave so differently? This has allowed the sub-discipline to largely ignore what John Stuart Mill long ago identified as one of the major barriers to developing cumulative social science: the

simple fact that in the “real-existing” world of politics identical or similar outcomes can have different causes. Perhaps, it is because my recent research has focused on two areas where this phenomenon has been markedly present: European integration and democratization that I am so sensitive to this ontological problem. In both of these sub-fields, the units involved had quite different points of departure, followed different transition paths, relied upon different institutional mixes and public opinions and, yet, ended up in roughly the same place. Granted there remain significant quantitative and qualitative divergences to be explained – presumably, by relying on the usual national suspects – but the major message they suggest is that of equifinality.

Of course, not all of the world's polities are converging toward each other in either institutions, policies or behaviors. Neo-neo-neo-institutionalists will have plenty of differences to explain into the distant future. Simplifiers will no doubt come up (*ex post*) with plenty of arguments why actors have rationally chosen different rules and policies. In other words, there will still be lots of room in the broad canopy of comparative politics. All I have been trying to do in this essay is to ensure that it will have a secure place and adequate rewards for those who choose to embrace complexity.

## ENDNOTES

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<sup>1</sup> If anyone doubts the wisdom of this assumption, he or she should consult the recent A New Handbook of Political Science, edited by Robert Goodin and Hans-Dieter Klingemann (Oxford: Oxford University Press, 1996). In their introduction, the editors explicitly (and uncritically) assume that the best that one can expect for the future is to imitate contemporary trends in American political science.

<sup>2</sup> Reference to JM paper.

<sup>3</sup> Here considerable credit has to be given to KKV and the critical literature that it has inspired..

<sup>4</sup> Reference to Tilly

<sup>5</sup> G. Almond et al., Crisis, Choice and Change: Historical Stories of Political Development (Boston: Little, Brown (1973).

<sup>6</sup> For example, by encouraging everyone to adopt a similar syllabus for introductory courses that is strongly skewed to promoting the new graft from economics. David Laitin ...

<sup>7</sup> They prefer to think of themselves as “positive political theorists,” although it is a mystery to me what is so positive about their approach and (presumably) negative about all of the others. And they are definitely not “positivists” given their reliance on stylized facts or mathematical proofs.

<sup>8</sup> “Seven Erroneous Theses ...

<sup>9</sup> Moreover, this approach has acquired some powerful allies from the political right in the United States who have correctly understood its fundamental hostility to politics in general and state solutions to policy conflicts in particular. Public/rational choice analysis provides respectable academic support for market-based ideological preferences and this goes a long way to explaining why neo-liberal think-tanks and foundations are so involved in financing work from this perspective. With a very few exceptions (the Santa Fe Institute is one of them), there are no equivalent sources of support for those who embrace complexity.

<sup>10</sup> The Perestroika Movement has had some success in countering the seemingly unstoppable advance of rational/public choice in American political science, especially within its professional association and journals. However, my (sporadic) reading of its extensive e-mail correspondence suggests that it remains too fixed upon the divide between “quantitativists” and “qualitativists” which I regard as less and less relevant in the future. It also has occasionally become a vehicle for the defense *à outrance* of “area studies” which at least in its “culturalist” version can be just as simplifying and simplistic as rational/public choice.

<sup>11</sup> Reference

<sup>12</sup> Alker reference and concept

<sup>13</sup> Rfere



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<sup>14</sup> As I write these words, I am reminded of Karl Deutsch's forgotten masterwork, The Nerves of Government ( ). It has been along time since I read it (and when I did I could not make productive use of it), but I wonder if its "cybernetic" approach to politics might be just the sort of founding statement that "complexifiers" need.

<sup>15</sup> The quotation is from Peter A. Hall, "Aligning Ontology and Methodology in Comparative Research," paper presented at a Workshop on Comparative Historical Analysis, Harvard University, November 10-11 2000, p. 23.

<sup>16</sup> Reference to Nye and Keohane

<sup>17</sup> It is intriguing to note, for example, the frequency with which the metaphor of "neo-medieval" has emerged to describe crudely recent developments in the relations between previously sovereign polities.

<sup>18</sup> Reference to Stanley Hoffmann

<sup>19</sup> Reference to HIS paper.

<sup>20</sup> One frequently encounters in the literature that focuses on national or sub-national "governance" the concept of **network** being used to refer to these stable patterns of horizontal interaction between mutually respecting actors. As long as one keeps in mind that with modern means of communication the participants in a network may not even know each other – and certainly never have met face-to-face – then it seems appropriate to extend it to cover transnational and even global arrangements.

<sup>21</sup> References to Ragin

<sup>22</sup> Reference to the dissertations of Carsten Schnieder and Claudius Wagemann.

## CHAPTER 6B

### THE NATURE AND FUTURE OF COMPARATIVE POLITICS

If you are reading this, you must be interested in something called “comparative politics.” Perhaps, it is only a passing or a required interest; but it could become a more enduring and personal one. This chapter is intended to familiarize you – the reader – with what is an intrinsically ambiguous and contentious sub-discipline of political science.

On the one hand, comparison is an **analytical method** – perhaps, the best available one – for advancing valid and cumulative knowledge about politics. At least since Aristotle it has been argued that only by identifying and labeling the generic relations of power and then examining how they produce variable or invariable effects in otherwise different societies, can scholars claim that their discipline is scientific. Admittedly, controlled experimentation would be even better, but – unlike the physical sciences – students of politics are not normally given the opportunity of introducing a treatment and holding everything else constant. We have to take the world as it is, observe its myriad similarities and differences and, then, infer patterns of causality from simultaneous or time-lagged occurrences.<sup>1</sup>

The core of the method is really quite simple and it helps to explain why comparativists tend to be addicted to two things: (1) classification systems; and (2)

the Latin expression, *ceteris paribus*. First, it is necessary to identify what units have in common by placing them in some generic category – say, democratic as opposed to autocratic regimes. Then, the category may be extended further into subtypes *per genus et differentiam* – say, democracies with single dominant party systems, with alternating two party systems, with alternating multiple party coalitional systems, and with hegemonic (non-alternating) multiparty systems. Once these factors have been controlled for, the Latin kicks in, namely, the assumption that units in the same category share the same characteristics and, therefore, that “all things being equal” it must be something that they do not share – say, level of trade union organization, that is responsible for producing the differences in outcome that the analyst is interested in – say, the level of public spending. Of course, waving that magic Latin wand does not really control for all of the potential things that might be causing variation in public spending, but it does help to eliminate some of them.<sup>2</sup>

On the other hand, comparison has always had a **practical objective**, namely, to produce useful descriptive information about how politics is conducted in countries other than one’s own. Makers of public policy and investors of private funds, for example, need specialized bits of information in order to make reasonable choices when dealing with ‘exotic’ actors and organizations. They could not care less about the ‘scientific basis’ of the information, provided it is accurate and reliable. Predicting behavior and, thereby, lowering the risk involved in transactions with foreigners is what they are interested in, and fancy theories may be no better at this than simple projections from past experience or calculation of statistical probabilities.

**Why** a given country decides to expropriate foreign property may be much less significant for these consumers than **whether** polities with certain kinds of leaders, party systems, ethnic or class conflicts, etc. are more likely to do so and, therefore, can be predicted to engage in this kind of behavior..

While there is no intrinsic reason why these two sides of the sub-discipline should contradict each other in principal, they often do in practice. Accurate and reliable information for description usually comes in the form of expressions and perceptions generated by the actors themselves; cumulative and valid data for analysis depends on analogies and concepts rooted in generic categories, themselves embedded in specific theories. The closer they are to each other, the narrower will be the potential for comparison in time and space – until comparative politics becomes nothing more than a description of “other people’s politics,” and every case has its unique explanation.

As hinted above by invoking Aristotle, comparative politics is as old as the empirical study of politics itself. Today, even those scholars who only conduct research on a single polity find themselves ineluctably drawn into the sub-discipline. As soon as they move beyond pure description in native terms and start using a vocabulary based on generic analogies or more comprehensive systems of classification, they risk exposing themselves to comment and criticism from aggressive comparativists. For example, a student of American politics who concludes that a two-party system has been an indispensable element for this regime’s democratic stability may find

him or herself challenged by those who have studied such exotic polities as Uruguay or Colombia where analogous institutions have sometimes failed to produce the same result. Indeed, in the latter case, it could even be argued that one of the most destabilizing features has been its oligarchic and sclerotic two-party system. Meanwhile, perhaps unbeknownst to the naïve Americanist, there are many multi-party systems in Western Europe that have been models of political stability and policy innovation.

So, even casual students of political science may not be able to escape the tentacles of comparison, no matter how hard they try. Knowing everything there is to know about some period or aspect of one's own country's politics could be misleading without some effort at placing it "in comparative perspective." Even seeking refuge in international relations will no longer suffice. There may be only one world system to be observed (although there are several of them to be compared over time), within that single case there have emerged ambiguous 'trans-national' polities, such as the European Union, other regional and functional "regimes" and a myriad of non-governmental organizations whose internal rules and relations of power more closely resemble those within national states than the allegedly endemic 'anarchy' and 'insecurity' that are supposed to characterize the relations between sovereign national states.<sup>3</sup>

The student in search of a field of specialization should be aware that the threshold for entry into comparative politics is high. You will normally be expected to learn at

least one foreign language – the more the better and the more exotic the better! You will also have to spend long hours familiarizing yourself with someone else's history and culture – and be willing to spend considerable time living away from home, often in rather uncomfortable places. Despite the amazing amount and variety of data made available by the Internet, field research abroad is still a *rite de passage* for most comparativists.

### **A Promising but Controversial Future**

If you do accept the challenge, be prepared to cope with controversy. There have been periods of relative tranquility when the sub-discipline was dominated by a single paradigm. For example, until the 1950s, scholarship consisted mostly of comparing constitutions and other formal institutions of Europe and North America, interspersed with wise comments about more informal aspects of national character and culture. 'Behaviorialism' became all the rage for a shorter while, during which time mass sample surveys were applied across several polities in efforts to discover the common social bases of electoral results, to distinguish between "bourgeois/materialist" and "post-bourgeois/post-materialist" value sets, and to search for the 'civic culture' that was thought to be a pre-requisite for stable democracy. 'Aggregate data analysis' of quantitative indicators of economic development, social structure, regime type and public policy at the national and sub-national levels emerged at roughly the same time. 'Structural-functionalism' responded to the challenge of bringing non-European and American polities into the purview of comparativists, by seeking to identify universal tasks that all political

systems had to fulfill, regardless of differences in formal institutions or informal behaviors.

None of these approaches has completely disappeared and all academic departments of political science are likely to have some mixture of them. But none is “hegemonic” at the present moment. As one of its most distinguished practitioners described them, present day comparativists are sitting at different tables, eating from different menus and not speaking to each other – not even to acknowledge their common inheritance from the same distinguished ancestors.<sup>4</sup>

There is, however, one characteristic that they all shared. Every one of these recent approaches originated in the United States of America, usually having been borrowed from some adjacent academic discipline.<sup>5</sup> The prospective student interested in comparative politics had only to look at the dominant “fads and fashions” in American political science, trace their respective trajectories and intercepts, and he or she could predict where comparative politics would be going for the next decade or more. Who could doubt that this sub-discipline of political science as practiced in the United States of America showed the rest of the world “the face of its future”?<sup>6</sup> After all, by far the largest number of professionals applying this method to describing and analyzing the widest variety of polities has always been employed in this country – not to mention the fact that a substantial proportion of those applying it elsewhere have been trained in the USA.

The central assumption of this essay is that the future of comparative politics should (and, hopefully, will) diverge to some degree from the trends and trajectories followed in recent years by many (if certainly not all) political scientists in the United States. As I have expressed it elsewhere, the sub-discipline is presently “at the crossroads” and the direction that its ontological and epistemological choices take in the near future will determine whether it will continue to be a major source of critical innovation for the discipline as a whole, or dissolve itself into the bland and conformist “Americo-centric” mainstream of that discipline.<sup>7</sup>

In other words, this essay will **not** be an effort that even pretends to survey objectively and comprehensively what has been produced by comparativists – American or otherwise – in the recent past. It will be what the French call a *plaidoyer*, a biased plea from a particular advocate on behalf of a client who faces a critical “mid-career” choice that will determine his or her status long into the future.

### **First, Some Congratulations are in Order**

Let me begin, however, with some self-congratulation. Thanks to the assiduous efforts of many methodologically minded colleagues (mostly Americans, it is true), much fewer students applying the comparative method neglect to include in their dissertations an explicit defense of the cases selected – their number and analogous characteristics, a conscious effort to ensure sufficient degrees of freedom between



independent and dependent variables, an awareness of the potential pitfalls involved in selecting the cases based on the latter, a greater sensitivity of the universe of relevant units and to the limits to generalizing about the external validity of findings.<sup>8</sup> Despite many criticisms about the “non-cumulative” nature of the knowledge generated by comparative politics, there have emerged some continuous lines of research in which successive generations have built (critically) upon each other’s work. At the present moment, I would cite the burgeoning field of democratization as one where this has occurred. Even in my other current specialty, regional integration, something like a ‘common tradition’ has developed – despite quite fundamental theory-based differences at the point of departure.

These important gains in methodological self-consciousness have produced (or been produced by) some diminution in the “class warfare” between quantitative and qualitative political scientists. There is still some sniping going on and some of the former persist in asserting their intrinsic “scientific” superiority over the latter, but there is more and more agreement that many of the problems of design and inference are common to both and that the choice between the two should depend more on what it is the one wishes to explain or interpret than on the intrinsic superiority of one over the other – or, worse, how one happens to have been trained as a graduate student. Indeed, from my recent experience in two highly cosmopolitan institutions, the European University Institute in Florence and the Central European University in Budapest, I have encountered an increasing number of dissertations in comparative politics that make calculated and intelligent use of

both methods – frequently with an initial large N comparison wielding relatively simple quantitative indicators to establish the broad parameters of association, followed by a small N analysis of carefully selected cases with sets of qualitative variables to search for specific sequences and complex interactions to demonstrate causality (as well as the impact of neglected or ‘accidental’ factors). To use the imaginative vocabulary of Charles Tilly, such research combines the advantages of “lumping” and “splitting.”<sup>9</sup> Hopefully, this is a trend that will continue into the future.

The real challenge currently facing comparative politics, however, comes from a third alternative, namely, “formal modeling” based on rational choice assumptions. Much of this stems from a strong desire on the part of American political scientists to imitate what they consider to be the “success” of the economics profession in acquiring greater status within academe by driving out of its ranks a wide range of dissident approaches and establishing a foundation of theoretical (neo-liberalism) and methodological (mathematical modeling) orthodoxy upon which their research is based. This path toward the future would diverge both methodologically and substantively from the previously competing quantitative and qualitative ones. It would involve the acceptance of a much stronger set of limiting initial assumptions, exclusive reliance on the rational calculations of individual actors to provide “micro-foundations,” deductive presumptions about the nature of their interactions and reliance on either “stylized facts” or “mathematical proofs” to demonstrate the correctness of initial assumptions and hypotheses derived from them. The comparative dimension enters into these equations to prove that individual behavior

is invariant across units or, where it is not, that institutions (previously chosen rationally) can make a difference.

### **The ‘Genealogical Tree’ of Comparative Politics**

[Place Figure One Here]

As a prospective or practicing comparativist, the reader will find him or herself hanging or, better, sitting somewhere in the tree depicted in Figure One. It is a spatially schematized and temporally compressed representation of the genealogical roots, trunks and branches that have evolved into contemporary comparative politics. Some intrepid young scholars may be agile enough to scramble horizontally from one branch to another in the canopy; most, however, will have arrived and will remain on their roost by climbing vertically up one or another of the multiple trunks rooted in past traditions of political thought.

Its deepest root lies in something I have called “**sociological constitutionalism**” as invented by Aristotle and subsequently nourished by such a diverse group of “Dead White European Males” as Polybius, Machiavelli, Montesquieu, Benjamin Constant, Alexis de Tocqueville, Lorenz von Stein, Karl Marx, Moisei Ostrogorski, Max Weber, Emile Durkheim, Roberto Michels, Gaetano Mosca, Vilfredo Pareto, and Herbert Tingsten. Through various extensions and permutations this has become the branch subsequently labeled as “historical political sociology” with such luminaries as Stein Rokkan, T. H. Marshall, Reinhard Bendix, Otto Kirchheimer, Seymour Martin Lipset, Juan Linz, Hans Daalder, Mattei Dogan, S. N. Eisenstadt, Harry

Eckstein and Dankwart Rustow located somewhere along it during the decades immediately following the Second World War. Karl Deutsch probably should be placed here at an odd angle, since he was so single-handedly responsible for inserting a cybernetic graft into it. On the outer reaches of this cluster, one generation later, is where I can be most safely located.

The other deep root lies in “**legal constitutionalism**” fertilized initially by distinguished Anglo-French jurists such as Léon Duguit, Georges Burdeau, James Bryce, A. Lawrence Lowell and Woodrow Wilson, and developed during the subsequent century by scholars such as Maurice Duverger, Herman Finer, Samuel Finer, Giovanni Sartori, Carl J. Friedrich, Samuel Beer, Jean Blondel F.A. Hermens and Klaus von Beyme. Someone like Robert Dahl can probably be best located hanging comfortably in a hammock strung between the sociological and legal branches – which, in any case, have been converging for some time. Samuel Huntington is another distinguished comparativist whose roost in the tree is difficult to place, although it is easier to imagine him clinging closer to this branch than to the neighboring one.

From these two tap roots have been added a number of exogenous grafts during the 20th century. Political science became a voracious consumer of conceptual and methodological innovations from other, increasingly professionalized, social science disciplines – first, from **social psychology** with the so-called “behaviorist movement” and later (and somewhat more surreptitiously) from **anthropology** with

the “structural-functionalist approach.” The most distinctive product of the former was the rapid rise of comparative survey research, symbolized by the publication of the highly successful (and criticized) work, Gabriel Almond’s and Sidney Verba’s, The Civic Culture, in 1963. Today, this branch of comparative politics is routinely conducted within and often across virtually all of the world’s polities by scholars. Certainly, it is the most distinctive (and successful) contribution of American political science to the sub-discipline.

The anthropological graft has contributed much less in volume and attractiveness to the evolution of comparative politics. Its most important contribution was undoubtedly to preside over a vast extension in the range of countries brought under comparative scrutiny. When embracing “Non-Western” politics and faced with the need to explain “elections in Albania,” “budgeting in Zaire,” “civil-military relations in Indonesia,” and “federalism in Argentina,” scholars such as David Apter, Leonard Binder, Lucian Pye, James Coleman and Myron Weiner found it difficult to apply the usual legal or sociological categories and took refuge behind a variety of “functions” that presumably had to be performed by analogous “structures” in all political systems if they were to remain stable. After a major flurry of activity in mid-1950s to the early 1970s under the prestigious auspices of the SSRC Committee on Comparative Politics, scholars began to realize that the stipulated functions were excessively abstract and that the structures they were trying to explain often could not be assigned to a single one of them. Moreover, the entire notion of “systemic equilibrium” as the central metaphor for guiding comparisons among Non-Western

polities came into question when the stability of their institutions was revealed to be highly precarious. Once the key question was seen to be change, especially change in regime from democracy to autocracy or, more recently, the inverse, the approach became much less relevant – as exemplified by one of its most prestigious proponent’s shift to a more historical and agent-centered approach that emphasized “crisis, choice and change.”<sup>10</sup>

Finally, comparativists have always borrowed ideas and concepts from **economics**, especially from such early political economists as Adam Smith, Karl Marx, John. Stuart Mill, David Ricardo, Jeremy Bentham, Friedrich List, and Adolf Wagner. Albert Hirschman, although a heterodoxical figure outside the ranks of contemporary neo-liberal economists, has made several seminal contributions. But the real novelty of the past few decades has been the transfer of root assumptions, deductive thinking and mathematical modeling techniques into the study of politics – first, in research on American politics and, increasingly, in research on “other people’s politics.” The leading figures have been Anthony Downs, Thomas Schelling, Howard Raifa, Kenneth Arrow, Douglas North, Mancur Olson, Gary Becker, George Stigler, and, most centrally, James Buchanan and William Riker. As we shall see shortly, this graft from economics has opened up a radically new path to the future for comparativists.

Presently, the evolutionary tree of comparative politics resembles more a Tropical Banyan than a Florentine Cypress. It has a wide canopy of branches, certainly not a

single tapered and elegant peak. Its most curious aspect, however, is the number of practitioners who are roosting up there in the canopy, and who seem content with sharing the same generic label: **institutionalists**. Closer inspection of the foliage reveals that it contains an extraordinary variety of flora and fauna. About all they seem to agree upon is that “institutions matter.” They differ widely on what institutions are, how they come about, why is it that they matter, and which ones matter more than others. Moreover, some of those perched up there will even admit that other things also matter: collective identities, citizen attitudes, cultural values, popular memories, external pressures, economic dependencies, even instinctive habits and informal practices – not mention the old favorites of Machiavelli, *fortuna* and *virtù* – when it comes to explaining and, especially, to understanding political outcomes. This urge to find shelter under the capacious tent of “institutionalism” can either be interpreted as a bizarre effort to return to their legalistic origins (precisely in a world context in which such formalized constraints are manifestly inadequate for solving problems and resolving conflicts) or as a desperate attempt to make common cause with the greatest possible number of disciplinary brethren (precisely when so many of them are heading in a direction that would radically challenge their basic assumptions and methods) .

### **At the Crossroads of Three Paths**

At the top of Figure One, I have placed a large question mark – a decision point that will determine the future configuration and even the very viability of the whole tree.

The safest thing one can say today about the future of comparative politics is that it will not be the same as in the past.

Of course, not everything is going to have to change. Comparative politics will continue for the foreseeable future to bear major responsibility for the **objective description** of processes and events in “other people’s countries” and, hence, for providing systematic and reliable information to those politicians (in and out of power) and to those administrators (at the top and bottom) charged with making and implementing national policies concerning these countries. The end of the Cold War and collapse of the Soviet Empire has, if nothing else, led to an impressive increase in the sheer number of polities whose (allegedly autonomous) behavior has to be described. The globalization of capitalism has produced increasingly indirect and articulated systems of production, transport and distribution that are much more sensitive to disturbances in the behavior of their most remote and marginal components. The ubiquitous penetration of the mass media has meant that the happenings anywhere in the world are being immediately transmitted everywhere and comparativist pundits will be expected “to place them in context” for public consumption.

Comparison between “real-existing polities” will also remain the best available **research method** for analyzing similarities and differences in behavior and for inferring the existence of patterns of regularity with regard to the causes and consequences of politics. It will always be the second best instrument for this



purpose, but as long as it remains impossible for students of politics to experiment with most of their subjects and subject matter, political scientists will have to settle for analyzing as systematically as possible variations they cannot control directly.

Figure One suggests that comparative politics will have to choose between three distinctive paths. It can continue along the mainstream and very broad “institutionalist” trajectory it has been on for the last decades, presumably adding more “neo-neo-neo-“ prefixes as it permutes into more specialized approaches. Otherwise, it can take a turn to either the left or the right. Whatever the choice, it is most unlikely that comparative politics will taper toward a single peak – however much some practitioners would like it to.<sup>11</sup> The most “clear and present danger”, as I see it, is that the sub-discipline’s evolution will lead to an irreversible split in the canopy with less-and-less communication or cross-fertilization between scholars perched on its different branches and more-and-more efforts to exclude dissidents from claiming the professional right to called themselves “scientific” students of politics.

Those who take a sharp turn to the right towards economics will be opting for **‘simplification.’** They will be led by those American colleagues who have already accepted the limited initial assumptions, exclusive reliance on individualistic “micro-foundations,” deductive presumptions about how these actors behave with regard to each other, and proof by ‘stylized facts’ or ‘mathematical formulae’ that characterize the path know as rational or public choice.<sup>12</sup>

Those who choose the leftward path will opt for what I call, for lack of a better term: **“complexification.”** They will follow the lead of a less well-defined and less self-confident group of scholars who:

(1) Accept far fewer and less restrictive initial assumptions – indeed, who rely upon a calculated proliferation of assumptions about the identity and motives of actors and about the role of entrenched institutions and historical memories in determining seemingly ‘irrational’ behaviors;

(2) Are convinced that adequate micro-foundations in the present world context cannot only be based on individual persons – indeed, they have to include collectivities that cannot be simply decomposed into the preferences or actions of individuals and to take more-and-more into consideration the composition effects generated by multiple levels of political power and authority;

(3) Choose to rely upon ‘reasonableness’ rather than rationality, i.e. on ‘improvising’ and ‘avoiding the worst’ in complex situations where optimal pursuit of marginal returns is virtually impossible given the number of actors involved, the plurality of sources of information and the unintended consequences generated by interdependent layers of political aggregation;

(4) Consider that the usual fallacies of composition can be converted into novel “laws of composition” to explain outcomes in situations where multiple layers of different types of actors from a plurality of centers of power and authority bargain and deliberate with each other;

(5) Have a healthy respect for ‘real’ data – whether generated by the normal operations of the polity or invented and gathered by themselves, coupled with an abiding suspicion of simple aggregative indicators for complex phenomena, so-called “stylized” facts that suppress confounding observations or simulations produced by impressive mathematical equations;

(6) Insist upon endogenizing as many potentially causal variables as possible, even those notoriously difficult to measure such as “preferences” – rather than shoving them into the background, assuming them out of existence, presuming what values they take in a given situation or inserting new ones *ex post* in order to ‘prove’ the alleged rationality of observed outcomes.

The competition between the three alternative paths depicted at the top of Figure One is hardly going to be equal. The middle one, toward various permutations of “the new institutionalism,” should be the most favored choice, if only due to sheer inertia rooted in the fixed intellectual assets of most practicing comparativists. Given the profusion of qualifiers that usually precede it – historical, sociological, legal and rational, just to name the most common ones – this approach is sufficiently

ambiguous to be appealing to a large number of them for some time to come, even if in my opinion it is already subject to diminishing marginal returns, divisive specifications, and less and less capacity to deal with anomalies.

The sharp right turn toward the simplicity of formal modeling should be (and already has been) very tempting, especially in the United States, for reasons I have discussed elsewhere.<sup>13 14</sup> Comparativists may be especially seduced by its appeal since it provides a convenient justification for eliminating what has, heretofore, been some of the most demanding requirements of the sub-discipline, namely, the need to learn a 'foreign' language, culture and history and to do protracted field research in a 'foreign' setting. Dedicated rational choicers already know what the dominant preferences are supposed to be,<sup>15</sup> and have no need to observe directly or interview 'exotic' respondents.<sup>16</sup> Information requirements have been radically simplified and, if they are not available in one or another of a multitude of on-line data banks, they can always be smoothed out by asserting "stylized facts" or just by simulating their probable distribution or trajectory.

Most saliently, those comparativists who take this path may be convinced that they are likely to reap the same rewards from higher 'scientific' status as have the neo-liberal, mathematized economists from whom they have lifted their intellectual baggage – "lock, stock and barrel."<sup>17</sup> They can also be assured that their work will only be understood by a small group of *conoscenti*.<sup>18</sup> Even if "economic science" has been notoriously unsuccessful in predicting the rate, direction or locus of

change in the macro-economy and even if it has suffered recently some notorious defections from within its ranks, it still remains very prestigious in the eyes of other social scientists. Public or rational choice is by now firmly entrenched in a dominant strategic position within several leading departments and journals in the United States – although it has recently provoked a strong reaction from non-believers within the profession gathered under the banner of “Perestroika” (a substantial proportion of whom seem to have been comparativists).<sup>19 20</sup>

The leftward turn toward complexity does not have any such disciplinary prototype to follow. At best, it can only pick up assumptions, concepts and ideas from a scattered diversity of sources within political science. Eventually, it may receive grafts from abstruse disciplines in the physical and mathematical sciences that deal with cybernetics, advanced circuits, complex systems, chaos theory, and so forth, but that remains to be experienced. By far the most important political scientist who has attempted to address these issues head on is Robert Jervis. His System Effects. Complexity in Political and Social Life is a foundational statement that deserves to be more widely read and absorbed.<sup>21</sup> It is not coincidental that Jervis is a specialist in international relations and draws most of his illustrations from that literature. This is certainly the field of political science that has been most manifestly challenged by increases in complexity, as measured by the volume and variety of transactions across national borders and of inter-governmental and non-governmental organizations that have emerged to deal with them. Moreover, the approach that had dominated this field for so long – “realism” – was avowedly

reductionist in its assumptions, and strongly resistant to according any significance to sub-national or trans-national political or legal authority. Joseph Nye and Robert Keohane,<sup>22</sup> Hayward Alker,<sup>23</sup> Liesbet Hooghe and Gary Marks,<sup>24</sup> Margaret Keck and Kathryn Sikkink<sup>25</sup> are other IR scholars who have made important and innovative efforts at conceptualization. Unfortunately, the compartmentalization of knowledge in political science departments has limited their contribution to comparative analysis. Such notions as “chains of consequence,” “emergent properties,” “indirect and delayed effects,” quasi-homeostasis,” “domino dynamics,” “spiral model,” “trans-national advocacy networks,” “cheap talk,” “embedded liberalism,” “complex interdependence,” “multi-layered governance,” and so forth, point the “complexifier” in the right direction and are applicable to all levels of political aggregation – and to their intercepts. *Notabene* that these concepts differ in to a significant extent from those commonly used by institutionalists, even from those historical institutionalists whose approach comes closest. Capturing the nature of complexity requires not only a more dynamic conceptualization of political relations, but also one that captures emergent, informal and even ephemeral ones. They do, however, not by any means add up to a comprehensive, coherent or consistent theory.<sup>26</sup> Indeed, virtually none of these sources refers to the other and even less do they offer a cumulative perspective that can subsume previous knowledge, explain new puzzles, provide counter-intuitive answers, and stand up to repeated attempts at falsification by both its practitioners and its competitors.

Sending present and future students of “other people’s politics” down such an unexplored (and un-fashionable) path may well seem like the height of folly. Quite understandably, apprentice political scientists need to identify with a research program that is already flourishing and promising – and, not coincidentally, likely to provide them eventually with better career opportunities. Comparativists may be even more susceptible to such a bandwagon effect since they are unusually dependent upon theory to identify the basic analogies between cases and to sustain the external validity of their findings. While I would argue that embracing complexity would be more “progressive” in the Lakatosian sense than the other two (both of which I regard as already “degenerative”), I would be the first to admit how difficult it is going to be to convince younger scholars at an especially vulnerable moment in their career that this is the case.

By now, it will come as no surprise to the reader that I am strongly in favor of “tilting” the future evolution of comparative politics toward embracing rather than rejecting complexity – even if I am manifestly incompetent to lead the way myself.<sup>27</sup> Perhaps, it is psychologically understandable that just when the surrounding political world has rapidly become more interdependent across units and more assertive within them – i.e. when national states are becoming less sovereign externally and internally – that scholars of politics would seek refuge in simplicity, parsimony and consistency. To many of those contemplating the sheer “messiness” and “noisiness” of contemporary politics at the national, sub-national and supra-national levels, it must be profoundly comforting to imagine that, under all of this, there lies some

easily specified and widely applicable ‘model’ for explaining what is going on. How reassuring it must seem to assume that “clear models about actors and preferences, strategical interaction (i.e. ‘game theory PCS), endogenization of variables one-at-a-time”<sup>28</sup> constitute an adequate response. The fact that this model may not be very good at **pre**-dicting future outcomes becomes less important than its utility in **retro**-dicting past ones (and then only provided that one has eliminated much of the allegedly random “mess and noise” and subsequently inserted the ‘proper’ preferences). If, however, it is still true that “a theory can be judged by the range and apparent verisimilitude of the predictions it makes about the world” – and not just by its formal elegance or logical consistency – then, I can see no viable alternative for us comparativists than to confront the messy and noisy world in which we live and design our theories accordingly.<sup>29</sup> And the place to start is by changing the basic concepts and classification systems that one needs to control for similarities and to identify differences. And the best tool for producing these building blocks is the “ideal type.” It combines a multitude of discrete variables into a recurrent ‘qualitative’ pattern of interrelations that invites attention to differences in type, not differences in magnitude.<sup>30</sup>

### **Coping with a Messy and Noisy World**

The core of my argument has been that comparative political analysis, if it is to remain significant, productive and innovative in the future, has to reflect the “real-existing” environment from which it should draw its observations and to which it



should refer its findings. Take, for example, the admonition made above by a comparativist advocate of rational choice, Carles Boix. His assumption, I repeat, is that “clear models about actors and preferences, strategic interaction (i.e. ‘game theory PCS), endogenization of variables one-at-a-time” constitute a threesome that is capable of generating non-trivial findings about politics in the contemporary environment. But what if what is needed are “fuzzy and under-specified models about a plurality of types of actors with preferences that are contingent upon differences in political setting,” “strategic interaction between a large number of players at different levels of aggregation with inconsistent payoffs, constant communication and multiple interdependencies” and “endogenization not of single discrete variables, but of patterns of multiple variables within the same time frame”? Would not such a transposition from the simplified world of conceptual clarity, stylized two-person games and ‘stepwise’ causality risk producing findings that bare no relation to the complexity of the “real-existing” world of politics? My contention is that if their concepts, assumptions and hypotheses fail to capture – not all, that would be impossible – but at least some of core characteristics of their subject matter, comparativists will at best report only trivial or irrelevant findings. They will address problems and provide answers to issues that are primarily internal to their own scholastic paradigm. These are **not** likely to be the problems that citizens and rulers have to cope with or the answers they expect comparative political research to provide.

One thing that differentiates comparativists from their colleagues who only study one polity or one international system is supposed to be greater sensitivity to contextual factors that are so deeply embedded that they are often taken for granted or treated as “exceptional” by Americanists or “unique” by International Relations specialists.. Inversely, they should be especially well equipped to identify and incorporate the trends that affect – admittedly, to differing degrees – virtually all of the world’s polities.

Two of these trends, in my opinion, are sufficiently pervasive as to affect the basic design and conduct of comparative research. They are: (1) **increased complexity**; and (2) **increased interdependence**. However independent their sources may be – for example, logically speaking, a polity may become more complex without increasing its interdependence upon other polities and a polity may enter into increasingly interdependent relations with others while reducing its internal complexity through specialization – these two trends tend to be related and, together, they produce something that Joseph Nye and Robert Keohane have called “**complex interdependence**.”<sup>31</sup>

One major implication that I draw from this is that complex interdependence is having an increasing influence not just on the substance of politics, but also upon its form. It is changing, in other words, the **units** that we should be using for specifying our theories and collecting our data and the **levels** at which we should be analyzing these data.

**Complexity:** This undermines one of the key assumptions of most of traditional comparative political research, namely, that the variable selected and observed with equivalent measures will tend to produce the same or similar effect(s) across the units being compared.

**Interdependence:** This undermines the most important epistemological assumption in virtually all comparative research, namely, that the units selected for comparison are sufficiently independent of each other with regard to the cause-effect relationship being examined.<sup>32</sup>

**Complex Interdependence:** The 'compound' condition makes it difficult, if not impossible, to determine what constitutes an independent cause (and, hence, an independent effect) and whether the units involved have an independent political capacity to choose and implement (and, therefore, to act as agents connecting cause and effect).

When Aristoteles gathered data on the 'social constitutions' of 158 Greek city-states, he set an important and enduring precedent. The apposite units for comparison should be of the same generic type of polity and at the same level of aggregation. And they should be more-or-less self-sufficient and possess a distinctive identity. Since then, almost all theorizing and empirical analysis has followed this model. One could compare "empires" or "alliances of states" or "colonies of states," but not

across these categories. Most of all, the vast proportion of effort has gone into studying supposedly 'sovereign' states whose populations shared a supposedly unique 'nationality.' It was taken for granted that only these 'sovereign-national' polities possessed the requisite capacity for "agency" and, therefore, could be treated as equivalent for comparative purposes. Needless to say, large N comparisons of all United Nations member-states rested on this fiction – and have rarely produced convincing findings. Even area specialists working with geographically or culturally denominated subsets of countries in Latin America, Sub-Saharan Africa, the Middle East and North Africa, and South, South-East or North-East Asia occasionally have had to face this issue of inference and external validity. Was Honduras in the 1950s when a substantial portion of its territory was owned by foreign banana producers "really" comparable with the much larger Brazil whose (then) major export resource, coffee, was in native hands? What is the utility of comparing the fiscal system of Kuwait that rests virtually exclusively on petroleum derived revenues with that of Jordan that depends largely on foreign aid and its own citizens? Comparativists who pioneered in the application of their method to sub-units of the same federal or decentralized polity had to limit themselves to circumscribed issues for which these sub-units possessed some equivalent autonomy of action. For example, it hardly made much sense to study differences in the application of military conscription across US States – with the possible exception of the "Free Republic of Berkeley" during the 1960s and 1970s.

In the contemporary setting, due to differing forms of complexity and degrees of interdependence, as well as the compound product of these two, it has become less and less possible to rely on the properties of sovereignty and nationality to identify equivalent **units**. No polity can realistically connect cause and effect and produced intended results without regard for the actions of others. Virtually all polities have persons and organizations within their borders that have identities, loyalties and interests that overlap with persons and organizations in other polities.

Nor can one be assured that polities at the same formal **level** of political status or aggregation will have the same capacity for agency. Depending on their insertion into multi-layered systems of production, distribution and governance, their capacity to act or react independently to any specific opportunity or challenge can vary enormously. This is most obviously the case for those national states that have entered into supra-national arrangements such as the European Union or signed binding international treaties such as those of the IMF or the WTO. Not only do they occasionally find themselves publicly shamed or found guilty by such organizations, but they may regularly anticipate such constraints and alter their behavior accordingly. Moreover, many contemporary national polities have granted or been forced to concede extensive powers to their sub-national units and, in some cases, these *estados autonómicos, provinces, regioni, Länder, cantons, municípios* and *communes* have entered into cooperative arrangements with equivalent units in adjacent national states.

From these observations, I conclude not only that it is literally absurd to compare only at the level of individuals, but also that comparativists need to dedicate much more thought to the collectivities they do choose and the properties these units of analysis supposedly share with regard to the specific institution, policy or norm that is being examined. Try to imagine someone studying the extent of commitment to environmental policies across European polities without reference to the European Union. Or another scholar comparing the human rights record of African states without taking into consideration the conditionalities posed by bi-lateral and multi-lateral foreign aid programs. I would admit that, in neither of these examples, should one presume that all variation in behavior or outcome can be explained by supra-national linkages. There still remains a great deal of difference that can only be explained by conditions within national polities, but exorcising or ignoring the complex external context in which these units are embedded would be equally foolish.

But what is the method one should apply when comparing units in such complex settings? The traditional answer is “to tell a story.” After all, what does a political historian –comparative or not – do but construct a narrative that attempts to pull together all of the factors within a specified time period that contributed to producing a specific outcome. Unfortunately, such narratives – however insightful – are usually written in “ideographic” terms, i.e. those used by the actors or the authors themselves. Systematic and cumulative comparison across units (or even within the same unit over time) requires a “nomothetic” language, i.e. one that is based on

terms that are specific to a particular approach or theory, not to a unique case. A first step for prospective “complexifiers” would be to invent or re-invent concepts so that they were more capable of grasping “fuzzy,” “contaminated,” and “layered” interrelationships among individuals and, especially, organizations (since the latter are much more salient components of contemporary political life).

For example, I experienced such a need when I began to think more seriously about the range of likely outcomes that the European Union might be heading for.<sup>33</sup> To fill this prospective space, I had to resort to pseudo-Latin and to define four “ideal type” configurations that I called: *federatio*, *confederatio*, *consortio* and *condomino*. I also delved into the language that European officials and politicians were inventing in order to make sense of what they were doing and discovered such odd things as: subsidiarity, co-decision, proportionality, additivity, complementarity, transparency, *géométrie variable*, co-responsibility, *juste retour*, transposition, mutual recognition, pooled sovereignty, home country control, economic and social cohesion, sustainable convergence, euro-compatibility, opting-in and opting-out, *comitologie*, concentric circles, *le spill-over*, *l’engrenage*, *la méthode communautaire*, *la supranationalité*, and, of course, *l’acquis communautaire*. And, believe it or not, this is just the tip of the Euro-speak iceberg!

### **Polities not (just) States**

The practice of comparative political research does follow and should imitate changes in “real-existing politics” – but always with a considerable delay.<sup>34</sup> As I

mentioned above, the most important set of generic changes that have occurred in recent decades involves the spread of “complex interdependence.” There is absolutely nothing new about the fact that formally independent polities have extensive relations with each other. What is novel is not only the sheer magnitude and diversity of these exchanges, but also the extent to which they penetrate into virtually all social, economic and cultural groups and into almost all geographic areas within these polities. Previously, they were mainly concentrated among restricted elites living in a few favored cities or regions. Now, it takes an extraordinary political effort to prevent the population anywhere within national borders from becoming “contaminated” by the flow of foreign ideas and enticements. **“Globalization”** has become the catch-all term for these developments, even if it tends to exaggerate the evenness of their spread and scope across the planet.

Globalization has certainly become **the** independent variable – the ‘first mover’ – of contemporary political science. It can be defined as an array of transformations at the macro-level that tend to cluster together, reinforce each other and produce an ever accelerating cumulative impact. All of these changes have something to do with encouraging the number and variety of exchanges between individuals and social groups across national borders by compressing their interactions in time and space, lowering their costs and more easily overcoming previous barriers – some technical, some geographical, but mostly political. By most accounts, the driving forces behind globalization have been economic. However, behind the formidable power of increased market competition and technological innovation in goods and



services, lies a myriad of decisions by national political authorities to tolerate, encourage and, sometimes, subsidize these exchanges, often by removing policy-related obstacles that existed previously – hence, the close association of the concept of globalization with that of liberalization. The day-to-day manifestations of globalization appear so natural and inevitable that we often forget they are the product of deliberate decisions by governments that presumably understood the consequences of what they have decided to *laisser passer* and *laisser faire*.

Its impact upon specific national institutions and practices is highly contentious, but two (admittedly hypothetical) trends would seem to have special relevance for the conduct of comparative political inquiry:

(1) Globalization narrows the potential range of policy responses, undermines the capacity of (no-longer) sovereign national states to respond autonomously to the demands of their citizenry and, thereby, weakens the legitimacy of traditional political intermediaries and state authorities;

(2) Globalization widens the resources available to non-state actors acting across national borders and shifts policy responsibility upward to trans-national quasi-state actors – both of which undermine formal institutions and informal arrangements at the national level, and promote the development of trans-national interests and the diffusion of trans-national norms

If either of these is true (and especially if both are), then, a major “paradigm shift” is going to have to occur – whichever of the paths from the canopy in Figure One you choose.

Comparativists have occasionally given some thought to the implications of these developments for their units of observation and analysis, but have usually rejected the need to change their most deeply entrenched strategy, namely, to rely almost exclusively upon the so-called “sovereign national state” as the basis for controlling variation and inferring similarities and differences in response to the impact of remaining variation in (allegedly) independent conditions. They (correctly) observe that most individuals still identify primarily (and some exclusively) with this unit and that national variables when entered into statistical regressions or cross-tabulations continue to predict a significant amount of variation in attitudes and behavior. Hence, if one is researching, say, the relation between gender and voting preferences, most of the subjects surveyed will differ from national state to national state – and this will usually be greater than the variation between sub-units within respective national states.

While I would concede this assumption for comparative analyses when based exclusively on behavior and attitudes at the individual level, I am convinced that the same does not hold for the behavior of meso- and, especially, macro-units of binding collective choice. Due to differing forms of complexity, differing degrees of interdependence and differing compounds of both of these conditions, no polity can

realistically connect cause and effect through its own institutions and policies without regard for the actions of others. Virtually all of them have persons and organizations within their borders that have identities and loyalties that overlap with those of other polities; virtually all decisional units within national states are affected by “extra-national” events over which they have quite limited control. The days when such exchanges only passed through Foreign Offices and were governed by international treaties or formal bilateral arrangements are over. Regions, provinces, and even municipalities engage in external relations with each other; trade unions and professional associations become part of overarching regional and international peak organizations; economic sectors and industries include firms from many different countries; social movements regularly exchange their programs and adopt each others’ tactics. Admittedly, political parties remain among the most national in their organization and ideology (and, not infrequently, are legally prohibited from receiving foreign funds), but they do meet with each other often, join “internationals,” coordinate their appeals and sometimes even support each others’ candidates.

My conclusion is that it has become less and less appropriate to rely on the properties of sovereignty, nationality and stateness for identifying the relevant units for theory, observation and inference. No doubt, comparative politics at the descriptive level will continue to dedicate most of its effort to formally sovereign national states. That is the level at which such information is normally consumed by policy-makers, the media and the public at large. But at the analytical level, it will have to break through that boundary and recognize that units with the same formal

status, e.g. all members of the United Nations or of some regional organization, may have radically different capabilities for taking and implementing collective decisions – and that virtually no national state can afford to presume that it is politically sovereign, economically self-sufficient and culturally distinct. In other words, comparativists have to give more thought to what constitutes a relevant and equivalent case once they have chosen a problem or puzzle to analyze and to do so **before** they select the number and identity of the units they will compare.

One innovation in research design would be to compare units at different levels of spatial or legal aggregation, provided they had similar properties and capacities with regard to the problem being studied. Another would be to ignore the boundaries imposed by area studies and try to identify units of analysis that share similar patterns of complex interdependence, regardless of their cultural or geographic propinquities. Yet another would be to shift to functional criteria and compare economic sectors with similarly layered production and marketing arrangements or ethnic groups with similarly proportioned and conflictual relations with titular majorities. The most audacious would be to search for similarly configured, territorial or functional, patterns of power and authority and the effects they produce – wherever they were located spatially, culturally or even temporally.<sup>35 36</sup>

The most difficult challenge, however, will come from abandoning the presumption of “stateness.” Sovereignty has long been an abstract concept that “everyone knew” was only a convenient fiction, just as they also “knew” that almost all states had

social groups within them that did not share the same common political identity. One could pretend that the units were independent of each other in choosing their organizations and policies and one could get away with assuming that something called "the national interest" existed and, when invoked, did have an impact upon such collective choices. But the notion of stateness impregnates the furthest corners of the vocabulary we use to discuss politics – especially stable, iterative, "normal" politics. Whenever we refer to the number, location, authority, status, membership, capacity, identity, type or significance of political units, we employ concepts that implicitly or explicitly refer to a universe composed of states and "their" surrounding national societies. It seems self-evident to us that this particular form of organizing political life will continue to dominate all others, spend most publicly generated funds, authoritatively allocate most resources, enjoy a unique source of legitimacy and furnish most people with a distinctive identity. However we may recognize that the sovereign national state is under assault from a variety of directions -- beneath and beyond its borders -- its "considerable resilience" has been repeatedly asserted.<sup>37</sup> To expunge it (or even to qualify it significantly) would mean, literally, starting all over and creating a whole new language for talking about and analyzing politics.

The assiduous reader will have noted that I have already tried to do this by frequently referring to "polity" when the normal term should have been "state." I confess that I first became aware of this lexical problem when working on what is, admittedly, an extreme case, namely, the European Union. I then asked my reader

to try to imagine a polity that did **not** have the following: (1) a single locus of clearly defined supreme authority; (2) an established, and relatively centralized hierarchy of public offices; (3) a pre-defined and distinctive "public" sphere of *compétences* within which it can make decisions binding on all; (4) a fixed and (more-or-less) contiguous territory over which it exercises authority; (5) an unique recognition by other polities and exclusive capacity to conclude international treaties; (6) an overarching identity and symbolic presence for its subjects/citizens; (7) an established and effective monopoly over the legitimate means of coercion; (8) a unique capacity for the direct implementation of its decisions upon intended individuals and groups; and (9) a uncontested potential ability to control the movement of all goods, services, capital and persons within its borders.<sup>38</sup>

Now, the European Union has yet to acquire these properties. which makes the EU quite different from those well established national states that have been losing some of them. I will also admit that there is a lot of variation in stateness among such units and that the actual and aspiring members of the EU have moved much further in this direction than others. Europe is unique as a region in which supra-national policy cooperation has been generating norms – some 80,000 pages of them it is alleged – that condition the choices and organizations of its member polities, literally on a day-to-day basis. Nothing remotely like this has come from NAFTA, MERCOSUR, CACOM or ASEAN. Nevertheless, the polities of North America, South America, Central America and South-East Asia are all ensnared in a growing network of supra-national norms and sometimes even adjudication

mechanisms that call into question many of the nine “imaginary” dimensions set out above. “Conditionality” may be a vague term and its efficacy is often doubtful, but no one can question that organizations such as the United Nations, the World Bank, the International Monetary Fund, the World Trade Organization and a myriad of other global and regional “inter-governmental organizations” are having some effect “beyond the nation state.” And this is not to mention the much more numerous non-governmental organizations with direct links to political groups “beneath the nation state.”

Before comparative politics can embrace complexity, it will have to admit to a much wider variety of types of decision-making units and question whether those with the same formal status are necessarily equivalent and, hence, capable of behaving in a similar fashion. As we shall now see, this is very likely to involve paying much more attention to “patterns” than to “variables.”

### **Patterns not (just) Variables**

Contemporary comparative politics has tended to focus on variables. The antiquated version tried to explain the behaviour of whole cases – often one of them at a time. The usual approach has been to choose a problem, select some variable(s) from an apposite theory to explain it, decide upon a universe of relevant cases, fasten upon some subset of them to control for other potentially relevant variables, and go searching for “significant” associations. Not only were the units chosen presumed to reproduce the underlying causal relations independently of

each other, but each variable was supposed to make an independent and equivalent contribution to explaining the outcome. We have already called into question the first assumption, now let us do the same with the second.

Complexity requires that one attempt to understand the effect(s) of a set of variables (a “context” or “ideal-type” if you will) rather than those of a single variable. And, normally, the problem or puzzle one is working on has a multi-dimensional configuration as well. In neither case is it sufficient simply to standard score and add up several variables (as one does, for example, with such variables as economic or human development, working class militancy, ethnic hostility, quality of democracy, rule of law, etc.). The idea is to capture the prior interactions and dependencies that form such a context and produce such an outcome. In other words, the strength of any one independent variable depends on its relation with others, just as the importance of any chosen dependent variable depends on how and where it fits within the system as a whole.

There is another way of expressing this point. In the classical ‘analytical’ tradition, you begin by decomposing a complicated problem, institution or process and examining its component parts individually. Once you have accomplished this satisfactorily, you then synthesize by putting them back together and announce your findings about the behavior of the whole. But what if the parts once de-composed change their function or identity and, even more seriously, what if the individual parts cannot be re-composed to form a convincing replica of the whole? In complex



political arrangements, the contribution of the parts is contingent upon their role in an interdependent whole. We comparativists have long been aware of the so-called “**ecological fallacy**,” namely, the potential for error when one infers from the behavior of the whole, the behavior of individuals within it. For example, just because electoral districts in the Weimar Republic with a larger proportion of Protestants and farmers tended to vote more for the Nazi Party (NSDAP), is no proof that individual Protestants and farmers were more likely to have voted for that party. This can only be demonstrated by data at the apposite level. But what is more important in today’s complex world is the inverse, i.e. “**the individualistic fallacy**.” This consists in simply adding up – usually without any weighting or multiplying – the observations about individuals and proclaiming an explanation for what they do together. Hence, the more “democratic” the values of sampled persons; the more “democratic” their polity will be.<sup>39</sup> Rational choice analysts do this routinely (with the ever present caveat that their subjects always act “under constraints”). While I would admit that this may work reasonably well where the political process being studied is itself additive, i.e. voting, it can lead to serious fallacies of inference when ‘rational’ individuals interact unequally within pre-existing institutions and networks. Just try to imagine the re-composition of individual preferences and rational choices into a model that would try to predict, say, the level of public spending or the extent of redistribution across social classes!

These, admittedly primitive, thoughts about complexity imply a very significant change in conceptualization – how one defines and circumscribes what it is one

proposes to use as independent, intervening or dependent variables. The classical advice is to make these concepts as precise as possible, so that they can be recognized inter-subjectively: and measured unequivocally. Applications beyond these limits are said to be “stretched” out of shape for historically or culturally reasons and, therefore, invalid for comparative purposes.<sup>40</sup> Of course, the ultimate simplification is to reduce the variable to a single dimension that can be accurately represented by numbers, by symbolic logic or by mathematical equation. Most rational choice and a good many institutional comparativists are quite proud of their ability to do this – and, thereby, to insure both clarity and parsimony in their work.

A “complexifier” is more likely to be interested in a set of variables whose boundaries and interrelationship are intrinsically unclear. What does the explaining or is the object of explanation is something “fuzzy” that may not take on the exact same observable properties in every case. His or her concepts reference something that is similar but not identical. They may share certain properties but not all of them and when they are broken down into sub-types, the categories may be nominal and not ordinal, i.e. they may cluster according to different and not the same criteria. Just think of some of the concepts that political scientists use almost everyday: power, state, democracy, legitimacy, pluralism, capitalism, competition, hegemony, accountability, responsiveness, bureaucracy *e così via*. These and many more are ideal-types and they are blurred, radial and fuzzy in nature. A qualified observer knows what they are and grasps what they may have in common, but hesitates before specifying them in identical “non-stretchable” terms. Take, for example, the

label, democracy, when applied to the United States. We find it relatively easy to dismiss the fact that this polity did not accord full citizenship to women until the 1920s and yet still deserves to be classified as a democracy before that time, but what about the “Single-Party South” and the systematic (and widely tolerated) suppression of the right to vote for Afro-American “citizens.” By any rigorous standard, the prevalence of this condition and practice should disqualify American democracy until after the Civil Rights Movement of the 1960s and, yet, I suspect that most American political scientists would strongly contest such a “de-classification” of their democracy. I have yet to find a comparative study of “stable Western liberal democracies” that does not include the United States – whatever the time frame. The same generic argument could be made about the political manipulation of the judiciary by the executive power in France until quite recently or the “bizarre” survival of the un-elected (but no longer hereditary) House of Lords in Great Britain or the non-rotation in power of Swiss political parties. There have been quantifying simplifiers who scored Guatemala and Colombia as democracies when the military in the former had veto power over elected civilians (as well as being an egregious violator of civil rights), and the latter was ruled by martial law over 1/3 of its territory.– just because regular competitive elections continued to be held.

My contention is that fuzzy “ideal-typical” concepts are virtually indispensable in political science, even if attempts (and there have been many) to pin them down to identical, least of all quantifiable, measures have failed. Moreover, in a world of steadily increasing “complex interdependence,” comparativists will have to rely more

and more on such concepts, both to do the explaining and to specify what has to be explained. The quest of simplifiers for precise definition and uniform measurement (not to mention parsimonious explanations) will mean that some of the key aspects of politics in our times will be excluded conceptually and dismissed as potential subjects of research. Just think of all those elements of contemporary politics that involve lengthy chains of causality, the intervention of indirect or delayed agents, the impact of un-intended consequences, the possibility of multiple equilibria, the cooperation of several layers of authority, the emergence of new (and, often, contradictory), properties, the ‘chaotic’ effect of minor variations, the concurrent presence of discrete causes and their compound impact, the un-expected resistance of entrenched habits and standard operating procedures, the effect of random or unique contingencies, the role of anticipated reactions, the ‘invisible constraints’ imposed by established powers, not to mention, the inability of any actor to understand how the whole arrangement functions.

Let me illustrate this point by invoking two concepts that I have used to describe and analyze the European Union – my assumption again being that this regional polity represents an extreme case, even a *reductio ad absurdum*, of the direction in which all polities are heading (admittedly at very different paces):

**Multi-Level Governance:** an arrangement for making binding decisions that engages a multiplicity of politically independent but otherwise interdependent actors – private and public – at different levels of territorial aggregation in

more-or-less continuous negotiation, and that does not assign exclusive policy *compétence* or assert a stable hierarchy of political authority to any of these levels.

**Poly-centric Governance:** an arrangement for making binding decisions for a multiplicity of actors that delegates authority over functional tasks to a set of dispersed and relatively autonomous agencies that are not controlled – *de jure* or *de facto* – by a single collective institution.

To the extent that the polities you are comparing have something like these characteristics, they are both spatially and functionally complex. *Nota bene*, these are not just “supra-national” properties, even if they are most prominently on display in the EU. A polity may be relatively free of constraints imposed by global or regional organizations (or by hegemonic neighbors) and still find itself in a situation of multi-level and poly-centric governance due to the autonomous behavior of sub-national units or functionally specific agencies. Consider, for example, the United States which under its present government is most emphatic about its lack of accountability to international law and organizational constraints (that it does not control) and yet on the issue of stem-cell research it has been unable to exert sovereignty over sub-units and agencies that oppose the policy of its central state. Infra-national units can contribute just as much to the complexity of politics as supra-national ones.

In addition to major implications for the operationalization of indicators – i.e. simple aggregate measures of individual or collective behavior will not do – complex interdependence within and between polities raises the prospect of major fallacies of inference between different levels of analysis. Properties reliably observed at the micro-, meso- or macro-levels may be poor predictors of what happens at other levels, lower and higher. Just as one cannot simply add up a large numbers of “civically cultured” and “democratically minded” citizens in order to produce a democracy, one cannot guarantee that conditionalities imposed by regional organizations or hegemonic democratic neighbors will suffice to ensure a successful transition from autocracy to democracy – or even to have an important impact. The correct inference may depend on context, i.e. upon the multiple layers involved and the possible existence of a plurality of competing centers of authority.

It is no coincidence that both of these descriptive qualifiers are attached to the same substantive concept: **governance**. No student of comparative politics over the past two and a half decades can have ignored the amazingly rapid and widespread diffusion of this concept and yet few concepts have ever been as fuzzy and polysemic. Whatever its opportunistic origins or the many (all ambiguous) meanings attached to it, I am convinced that behind the notion of governance hides an important message about changes in the practice of “authoritatively allocating values” (as David Easton so eloquently put it). Government, i.e. doing so through a hierarchically disposed and legitimately recognized set of public institutions, is less and less capable of making such allocations– especially when confined to a single

level of spatial aggregation. What it takes is something much more complex which I have elsewhere described as:

a method/mechanism for dealing with a broad range of problems/conflicts in which actors, private as well as public, sub-national and supra-national as well as national, regularly arrive at mutually satisfactory and binding decisions by negotiating and deliberating with each other and cooperating in the implementation of these decisions.<sup>41</sup>

The core of such a complex arrangement rests on horizontal forms of interaction between actors who have conflicting objectives, but who are sufficiently independent of each other so that neither can impose a solution on the other and yet sufficiently interdependent upon each other so that both would lose if no solution were found.<sup>42</sup>

In both modern and modernizing societies, some of the actors involved in governance are non-profit, semi-public and, at least, semi-voluntary organizations with leaders and members; and it is the embeddedness of these organizations into something approximating a civil society that is crucial for its success. These organizations do not have to be equal in their size, wealth or capability, but they have to be able to hurt or to help each other mutually.

Also intrinsic to governance is the notion of regularity. The participating organizations interact not just once to solve a single common problem, but repeatedly and predictably over a period of time so that they learn more about each other's preferences, exchange favors, experience successive compromises, widen the range of their mutual concerns and develop a commitment to the process of

governance itself. Here, the code-words also tend to be fuzzy: trust and mutual accommodation – more specifically, trust and mutual accommodation between organizations that effectively represent more-or-less permanent social, cultural, economic or ideological divisions within a society or across several of them.

Note also that governance is not just about making decisions via deliberation and negotiation, but also about implementing policies. Indeed, the longer and more extensively it is practiced, the more the participating organizations develop an on-going interest in this implementation process since they come to derive a good deal of their legitimacy (and material rewards) from the administration of mutually rewarding programs.

Governance is not a goal in itself, but a means for achieving a variety of goals that are chosen independently by the multiple layers of actors involved and affected. *Pace* the frequent expression, “good governance,” resort to it is no guarantee that these goals will be successfully achieved. It can produce “bads” as well as “goods.” Nevertheless, it may be a more appropriate method than the more traditional ones of resorting to public coercion or relying upon private competition.

Moreover, it is never applied alone, but always in conjunction with state and market mechanisms. For “governance” is not the same thing as “government,” i.e. the utilization of public authority by some subset of elected or (self-) appointed actors, backed by the coercive power of the state and (sometimes) the legitimate support of



the citizenry to accomplish collective goals. Nor is it just another euphemism for the “market,” i.e. for turning over the distribution of scarce public goods to competition between independent capitalist producers or suppliers. It goes without saying that, if this is the case, the legitimacy of applying governance to resolving conflicts and solving problems will depend upon different principles and operative norms than are used to justify the actions of either governments or markets.

This concept of “governance” will not suffice to bear all of the weight imposed upon future comparativists who turn toward complexity. But it is definitely one starting point for those setting off in that direction.

### **Concluding Thoughts**

First, I conclude with three disciplinary suggestions:

1. Political scientists should abolish the distinction between comparative politics and international relations and re-insert an ontological one between political situations that are subject to rules, embedded in competing institutions and not likely to be resolved by violence, and those in which no reliable set of common norms exists, where monopolistic institutions (including but not limited to states) are in more or less continuous conflict and likely only to resolve these conflicts by force or the threat of force. It used to be believed that this line ran between politics within states and politics between states. This being no longer the case – the probability of war has been greater within the former than between the latter for some time – there is no generic reason

that these two “historical” sub-disciplines should be kept apart. How about separating the students of politics into those working on “ruly” and on “unruly” polities, whether they are national, sub-national, supra-national or international?

2. Comparativists should attempt to include the United States in their research designs when it seems apposite, but they should not expect their Americanist colleagues to join them – at least, not for some time. The present direction of politics in the US is virtually diametrically opposed to the trends I have noted above. Americans (or, better, their present leaders) have reacted with hostility to the prospect of “complex interdependence” and made all possible effort to assert both their internal and external sovereignty. They have repeatedly denied the supremacy of supra-national norms and the utility of international organizations by refusing to regard those legal or organizational constraints that do exist as binding when they contradict or limit the pursuit of so-called national interests, and by withdrawing from them when it seems expedient to do so.
3. , Comparativists – whether of ruly or unruly politics – should be equipping themselves to conceptualize, measure and understand the great increase in the complexity of relations of power, influence and authority in the world that surrounds them. Admittedly, “complexity” is still only a specter haunting the future of their sub-discipline and the answer to meeting this need probably cannot come only from within their own ranks. Hopefully, comparative politics will attract successful “grafts” of theory and method from disciplines in the

physical and mathematical sciences that deal with analogous situations, but in the meantime the challenge should be met and the opportunity seized by us.. Just picking up a few scattered concepts from within political science, such as multi-layeredness, polycentricity and governance – as I have done – will not carry comparativists far enough. Although, if my experience in studying what must be the most complex polity in the world, the European Union, ‘real-existing’ politicians and administrators who have to cope with all of this contingency and complexity are inventing expressive new terms everyday. We should be listening to them, as well as to scholars in other disciplines, to pick up on these emerging arrangements, specify them more clearly where this is possible and search for points in our theoretical frameworks where they can be inserted. Contrary to the “simplifiers” who are genetically opposed to dialoging with their subjects, we “complexifiers” have a need and obligation to take seriously what they tell us they want and what they claim they are doing.

I cannot escape the conviction that this is the most promising path forward for the sub-discipline. The emergence of a new instrument, Qualitative Comparative Analysis (QCA), has given it some initial methodological momentum. Its originator, Charles Ragin, has been quite articulate about the implications it has for theory, in general, and the comparative method, in particular.<sup>43</sup> Especially in its “fuzzy set” and “two step” versions, QCA offers a far more appropriate estimation technique for analyzing complex causality than the usual ones from social statistics.<sup>44</sup>

And it also seems uniquely capable of explaining something that I think will become more and more salient in the future, namely, **equifinality**. Since its Aristotelian origins, the comparative method has been applied mainly to explaining differences. Why is it that polities sharing some characteristics, nevertheless, behave so differently? This has allowed the sub-discipline largely to ignore what John Stuart Mill long ago identified as one of the major barriers to developing cumulative social science: the simple fact that, in the “real-existing” world of politics, identical or similar outcomes can have different causes. Perhaps, it is only because my recent research has focused on two areas where this phenomenon has been markedly present: European integration and democratization that I am so sensitive to this ontological problem. In both of these sub-fields, the units involved had quite different points of departure, followed different transition paths, chosen different institutional mixes, generated quite different distributions of public opinion and, yet, ended up in roughly the same place. Granted there remain significant quantitative and qualitative divergences to be explained – presumably, by relying on the usual national suspects – but the major message they suggest is that of equifinality, i.e. convergence toward similar outcomes.

Of course, not all of the world’s polities are converging toward each other either in institutions, policies or behaviors. Neo-neo-neo-institutionalists will have plenty of differences to explain into the distant future. Simplifiers will no doubt come up (*ex post*) with plenty of arguments why actors have rationally chosen different rules and

policies. In other words, there will still be lots of room in the broad canopy of comparative politics. All I have been trying to do in this essay is to ensure that it will have a secure place and adequate rewards for those who choose to embrace complexity.

Second, I conclude with some personal lessons gleaned (retrospectively) from my career as a “card-carrying” comparativist. Had I run across them at an early stage of my career, I probably would have ignored them -- even rejected them. Nonetheless, I offer them to those younger scholars who might wish to take more time to reflect on their future and that of their sub-discipline than I did.<sup>45</sup>

- (1) Do not imagine that comparative politics is something invented by North Americans in the 1950s. It has a long and honorable history and the further back in it that you can ground your research, the more you will be protected against “ethnocentrism” and “presentism” of limited and fleeting importance;
- (2) Do not be afraid to take “natives” and “practitioners” seriously, even when they write in strange languages, come from backward (*sic*) countries and profess political values that are an anathema to you. You may learn more from such exotic sources than from all your collègues, camarades, concitoyens and compagnons de route;
- (3) Pick for your dissertation committee scholars who know nothing (or relatively little) about the country or countries you have chosen to work on, but who know a lot about social and political theory, have strong paradigms of their own, and are nevertheless still willing to take you seriously;
- (4) Do not be embarrassed by the fact that your initial research is likely to be on a single country and be prepared to offer to the discipline “models” and

- "concepts" that have been generated by that single case -- provided that you are confident at having captured its most generic properties and are willing to spend a lot of subsequent effort defending that assumption;
- (5) If, heaven forbid, you find yourself with a conceptual albatross around your neck, as I did, make sure it is a big and important one that your colleagues and competitors cannot afford to overlook and then make yourself a moving target -- but try, at all costs, to avoid concepts that have too many unpleasant associations with the past or whose current usage is too far removed from the connotation you wish to give it. To invert the immortal saying of de Lampedusa, learn "to change without appearing to change" by defending the essential of your insights while modifying their applications and implications in response to your critics. Typologies, fourfold tables and periodizations are especially useful in this regard;
- (6) Move back-and-forth between théorie and empirie, between the library and the field, between deduction and induction, between abstract concepts and concrete observations without allowing yourself to imagine that only one side of this dialectical process of inquiry will produce truth or, worse, science. This is another way of presenting a moving target -- and of adapting your insights to the real world faster than your critics;
- (7) Be prepared to accept invitations to give talks and attend conferences on obscure topics in obscure places. You never know when you might learn something quite unexpected and you are much more likely to do so "out in the bush" than in the more predictable confines of your national political science or area studies association. If you really wish to be a comparativist, you must be prepared to live a comparative life (although it doesn't hurt to select your longer research stays carefully for climatic, cultural and human comforts).

- (8) Try to learn as many languages as possible, preferably without wasting a lot of time in the classroom. Not only will this facilitate your being invited to obscure places, but you can never tell when the political jargon spoken by the actors in weird and remote systems will alert you to some relationship that you might otherwise have ignored;
- (9) Co-authorship is a good idea, especially when it crosses national and disciplinary lines. Never, however, agree to write anything with a scholar whom you do not regard as your intellectual equal and never try to resolve your (inevitable) disputes with him or her by opting for a lowest common denominator solution. After a truly successful collaboration, it should be impossible for either of you to tell who contributed the best ideas. In any case, readers will impute the not-so-good ideas to you;
- (10) Forging close personal links with other scholars across national and even continental divides can be an important component in the "design" of comparative research. It permits one not only to include more cases and to capture a wider range of variation, but also to control for idiosyncratic or ethnocentric perspectives.
- (11) If you must become a political scientist, think protractedly and carefully before choosing to specialize in comparative politics. It is by far the most demanding (and the most rewarding) of sub-disciplines.

## ENDNOTES

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<sup>1</sup> And when the comparativist does this, he or she is inevitably speaking in terms of "probabilities," not invariant constant connections (i.e. "laws of politics"). No matter how closely cause and effect seem to follow each other across the cases you are analyzing, there are probably going to be exceptions or "deviant cases" somewhere out there. And there will always be another comparativist waiting to hit you with the "Pago-Pago Ploy," i.e. his or her knowledge of some exotic setting in which your most

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treasured finding is contradicted – maybe even inverted! In my limited experience in Latin America and Europe, Costa Rica and Switzerland have often been useful for this purpose.

<sup>2</sup> This may help to explain why such a high proportion of comparativists have come from Scandinavia or have used data on these countries. There are so many common elements that these countries share – and a few that they do not – that cross-national inferences about causality in politics are likely to be more convincing when drawn from this ‘sample.’ Hopefully, something of the same will be the case with the post-Communist countries of Central and Eastern Europe. Needless to say, a similar logic lay behind the practice of “Area Studies,” even if it turned out that Latin American, Sub-Saharan African, Middle Eastern and Asia countries did not to have as much in common as area specialists often assumed.

<sup>3</sup> If you have any doubt about whether a given piece of research is comparative, I suggest that you apply “Sartori’s Test.” Check its footnotes and compare the number of them that are devoted exclusively to the country or countries in question and those that refer to general sources, either non-country specific or that include countries not part of the study. The higher the ratio of the latter over the former, the more likely the author will be a genuine comparativist. If the citations are only about the country or countries being analyzed, then, it is very unlikely that the author has applied the comparative method – regardless of what is claimed in the title or flyleaf! “Comparazione e Metodo Comparato,” Rivista Italiana di Scienza Politica, Vol. XX, No. 3 (Dicembre 1990), p. 400.

<sup>4</sup> Gabriel Almond, A Discipline Divided. Schools and Sects in Political Science (Newbury Park: Sage, 1990).

<sup>5</sup> Although the European reader may wish to consult a recent special edition of the journal, European Political Science, where it is convincingly argued that “the American Science of Politics” has deeper roots in European culture (and European exiles) than is usually recognized.

<sup>6</sup> If you doubt the existence of this assumption of superiority, consult the recent A New Handbook of Political Science, edited by Robert Goodin and Hans-Dieter Klingemann (Oxford: Oxford University Press, 1996). In their introduction, the editors explicitly (and uncritically) assume that the best that one can expect for the future is to imitate contemporary trends in American political science. The notion that Europe (and, needless to say, the rest of the world) might have a different tradition of comparative analysis is not even raised – much less taken seriously.

<sup>7</sup> “Comparative Politics at the Crossroads”, Estudios-Working Papers, 1991/27, Centro de Estudios Avanzados en Ciencias Sociales, Instituto Juan March de Estudios e Investigaciones (Madrid), 1991.

<sup>8</sup> Here, considerable credit has to be given to the widespread use by comparativists of Gary King, Robert O. Keohane and Sidney Verba, Designing Social Inquiry (Princeton: Princeton University Press, 1994) and, more recently, to its critical counterpart, Henry E. Brady and David Collier (eds.), Rethinking Social Inquiry: Diverse Tools, Shared Standards (Lanham, MD: Rowman & Littlefield, 2004)

<sup>9</sup> Big Structures, Large Processes, Huge Comparisons (New York: Russell Sage Foundation, 1984).

<sup>10</sup> G. Almond et al., Crisis, Choice and Change: Historical Stories of Political Development (Boston: Little, Brown (1973).

<sup>11</sup> For example, by encouraging everyone to adopt a similar syllabus for introductory courses that is strongly skewed to promoting the new graft from economics. David Laitin, “The Political Science Discipline,” paper presented at the Annual Convention of the American Political Science Association, San Francisco, CA, 2001). The very notion that political science – comparative or not – should be



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rooted in a single theoretical orthodoxy would seem to me to do violence to its subject matter, all the more so in an epoch of radically increasing complexity.

<sup>12</sup> They prefer to think of themselves as “positive political theorists,” although it is a mystery to me what is so positive about their approach and (presumably) negative about all of the others. And they are definitely not “positivists” given their frequent reliance on stylized facts or mathematical proofs.

<sup>13</sup> “Seven (disputable) theses concerning the future of 'transatlanticised' or 'globalised' political science”, in European Political Science, Spring 2002, pp. 23-40

<sup>14</sup> One ‘appeal’ of this exclusive dependence on individualism I find especially puzzling. Some of its devotees seem to be normatively excited by the notion that this method somehow revives the prospect for re-asserting “human agency” into the study of politics. Individual choices (however smothered by those of a multitude of other individuals) can finally make a difference. What this blatantly ignores is the dependent phrase that is always (if often implicitly) attached to every one of their deductive propositions – namely, “under constraints.” Individuals act rationally, but within a pre-established system of privilege and power. The approach makes no effort to understand these constraints (which constitute most of the ‘darker side’ of politics). They are simply given. In other words, “human agency” is postulated and, then, immediately circumscribed by “the powers that be” – whatever they may be. I personally fail to find that such an approach has any normative appeal. It should be precisely the function of a ‘critical’ and ‘democratic’ political science to discover what these “constraints” are, how they came about historically, and what can be done to make them less constraining on citizens’ exercise of “human agency.”

<sup>15</sup> And if the actors do not confirm the initial suspicion that their purpose is to acquire more wealth or material goods opportunistically by optimizing at the margin in each political exchange, the rational choicers will simply substitute another preference and, if necessary, yet another preference until the individual’s rationality has been proven. I have yet to find an article that manipulates the preference order until “other-regardingness” becomes the dominant one, but this cannot be far away. What I doubt will ever be admitted is that the individuals in question acted simply “irrationally” according to the terms set by the initial restricted assumptions. If you want to observe a ‘classic’ example of this “bait-switching” by rational choice theorists, read what they have to say about “the voter paradox,” where it seems irrational for any individual to vote unless the anticipated margin is very very narrow. Nevertheless, citizens do vote and even in elections whose outcome is a foregone conclusion. Just watch them hunt around *ad hoc* for a preference configuration that makes this collective behavior seem rational.

<sup>16</sup> This is a maxim they have inherited from the discipline of neo-liberal economics. As argued most prominently by Milton Friedman, the producers or consumers themselves have a ‘rational’ incentive not to admit to their ‘true’ preferences and, moreover, are likely to be biased into giving the interviewer more culturally or normatively respectable reasons for their choices. It is, therefore, a waste of time (and a potential source of confusion) to ask them why they are doing something. To the prospective comparativist, this can relieve him or her of some very heavy research burdens – even from the need to leave his or her desk.

<sup>17</sup> One of the most presumptuous assertions of those adopting this approach is that they have “micro-foundations” – something that all theories, deductive as well as inductive, are supposed to need and all others are said to lack. Their foundations rest on a radical form of individualism: no other actor or unit counts and all forms of action consist of simple aggregations of choices made by individual persons calculating rationally the costs and benefits of acting. Incidentally, it is by no means self-evident that the individual human being is **the** irreducible unit of social, economic or political analysis. One could just as well assume that most persons in our contemporary “layered” societies have multiple identities and plural interests that do not always form stable and transitive

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hierarchies. What such an individual wants or is trying to maximize may be contingent on many intervening spatial, temporal and/or functional factors.

In fact, the study of politics has built upon many different “micro-foundations” other than individuals. International relations in its orthodox “realist-rationalist” version rested on the presumed unity of sovereign national states acting to maximize their relative power and, thereby, protecting or promoting their national interests. This approach was manifestly indifferent to how these large and complex units were composed or governed internally. Comparative industrial relations – at least, in some versions – has been built upon the assumption that organizations (e.g. trade unions, employer associations and, sometimes, government agencies) were the irreducible units of analysis. Marxist approaches presumed a similar “foundational” role for social classes; anthropological ones did the same for homogenous cultural units such as tribes or clans.

<sup>18</sup> Moreover, this approach has acquired some powerful allies from the political right in the United States who have correctly understood its fundamental hostility to politics in general and to state action for solving policy conflicts in particular. Public/rational choice analysis provides respectable academic support for market-based ideological preferences and this goes a long way to explaining why neo-liberal think-tanks and foundations are so involved in financing work from this perspective. With a very few exceptions (the Santa Fe Institute is the only one I can think of), there are no equivalent sources of support for those who embrace complexity.

<sup>19</sup> The Perestroika Movement has had some success in countering the seemingly unstoppable advance of rational/public choice in American political science, especially within its professional association and journals. However, my (sporadic) reading of its extensive e-mail correspondence suggests that this movement remains too fixed upon the divide between “quantitativists” and “qualitativists” which I regard as being less and less relevant in the future. It also has occasionally become a vehicle for the defense à outrance of “area studies” which, at least in its “culturalist” version, can be just as simplifying and simplistic as rational/public choice. Nevertheless, no one contemplating becoming a comparativist should miss the opportunity of consulting its website: [http://groups.yahoo.com/group/perestroika\\_glasnost\\_warmhome](http://groups.yahoo.com/group/perestroika_glasnost_warmhome).

<sup>20</sup> There are those, political scientists of a normative vocation, who would go even further in their criticism of rational choice. Some of them argue that “individual self-regarding utility optimization” is not the solution, but the major problem in contemporary politics – and that this seemingly academic approach risks becoming self-fulfilling in the sense that the more that rulers and ruled are told that it is ‘normal’ for them to act according to its precepts, the more they will do so – and the worse off everyone will be. See S. M. Amadae, *Rationalizing Capitalist Democracy* (Chicago: University of Chicago Press, 2003) for an application of this critique to the conception of democracy.

<sup>21</sup> (Princeton: Princeton University Press, 1997). Todd La Porte (ed.), *Organized Complexity* (Princeton: Princeton University Press, 1975) is an early and neglected work that may bear revival. My own, very schematic, understanding of this literature has been improved by the articles in a special issue of the journal, *Democracy & Nature*, dedicated to “Systems Theory and Complexity” (Vol. 6, No. 3 (November 2000)). Its introductory chapter by Arran Gore is especially useful to anyone looking for a non-technical introduction to the “central ideas and approaches” of the Santa Fe Institute. An un-identified reader of an earlier version of this article has suggested the following sources: Cilliers, P. 2001 ‘Boundaries, Hierarchies and Networks in Complex Systems’ *International Journal of Innovation Management*, 5(2):135-147  
Reed, M. and Harvey, D.L. 1996 ‘Social Science as the study of Complex Systems’ in Kiel, L.D. and Elliott, E. (eds) *Chaos Theory in the Social Sciences*, Ann Arbor: Univ. of Michigan Press 295--324

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<sup>22</sup> Power and Interdependence: World Politics in Transition, 2<sup>nd</sup>. Edition (Boston: Little Brown, 1989).

<sup>23</sup> Rediscoveries and Reformulations: Humanistic Methodologies for International Studies (Cambridge: Cambridge University Press, 1996).

<sup>24</sup> Multi-Level Governance and European Integration (Boulder: Rowman & Littlefield, 2001).

<sup>25</sup> Activists beyond Borders: Advocacy Networks (Ithaca, NY: Cornell University Press, 1998).

<sup>26</sup> As I write these words, I am reminded of the forgotten masterwork of Karl Deutsch, another IR specialist: The Nerves of Government (New York: The Free Press, 1963). It has been along time since I read it (and when I did I could not make any productive use of it), but I wonder if its “cybernetic” approach to politics might be just the sort of comprehensive founding perspective that “complexifiers” need.

<sup>27</sup> I was delighted to discover, after writing this passage, a recent comment by Robert Dahl – arguably, the most distinguished “senior statesman” of contemporary political science – in which he too urged scholars coming into the profession to recognize that “highly consequential historical contingencies add immeasurably to the complexity of the world with which we must deal – a complexity on which we must not only base our descriptions but, so far as possible, our explanations, generalizations, and predictions.” He went on to admit (as I just have) “How ought we to deal with a subject of such daunting complexity? Alas, I not only don’t have a good answer, I’m afraid I don’t even have a plausible answer.” “Complexity, change and contingency,” in Ian Shapiro, Rogers M. Smith and Tarek Masoud (eds.), Problems and Methods in the Study of Politics (Cambridge: Cambridge University Press, 2004), p. 378

<sup>28</sup> I owe the quoted comments to Carles Boix – except for the insertion of “game theory.”

<sup>29</sup> The quotation is from Peter A. Hall, “Aligning Ontology and Methodology in Comparative Research,” paper presented at a Workshop on Comparative Historical Analysis, Harvard University, November 10-11 2000, p. 23.

<sup>30</sup> There is nothing new about this strategy of focusing on “ideal types.” The great social and political theorists of the 19<sup>th</sup> century -- all comparativists – used it in their efforts to capture the complexities of their time, e.g. Benjamin Constant with *la démocratie des anciens* and *la démocratie des modernes*, Karl Marx with capitalism and class conflict, Ferdinand Tönnies with *Gemeinschaft* and *Gesellschaft*, Emile Durkheim with mechanical and organic solidarity, Max Weber with types of legitimate authority and bureaucracy. What is distinctive of most of the contemporary ones listed above is their reference to relations between levels of social and political power, rather than relations at the same level of aggregation.

<sup>31</sup> Op. cit., FN 22.

<sup>32</sup> So named for Sir Francis Galton who raised it at a meeting of the Royal Anthropological Institute in 1889. The obvious solution to it is to include unconscious diffusion and conscious imitation across units as potential explanatory variables – much as one should test for the spuriousness of any observed relationship. The major contemporary difference is the existence of multiple trans-national organizations – governmental and non-governmental – that are in the continuous business of promoting such exchanges at virtually all levels of society and the occasional existence of regional or global organizations that can back up these efforts with coercive authority or effective ‘conditionality.’

<sup>33</sup> A wonderful example of inventive conceptualization is Fritz Scharpf’s use of the term, *Politikverflechtung*, to capture the complex, “overlapping” nature of policy-making between different layers of the German political system.

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<sup>34</sup> One of the repeated paradoxes of comparative politics is that scholars have a propensity for discovering and labeling novel phenomenon “at dusk, when the Owl of Minerva flies away,” i.e. at the very moment when the phenomenon is declining in importance. I suspect that this is because it is precisely institutions and practices that are in crisis that reveal themselves (and their internal workings) most clearly. Nevertheless, having been involved in “owl-chasing at dusk” several times, I can testify that it is a frustrating experience.

<sup>35</sup> It is intriguing to note, for example, the frequency with which the metaphor of “neo-medieval” has emerged to describe (crudely) recent developments in the relations between previously sovereign polities.

<sup>36</sup> This is **not** an appeal to engage in so-called “pooled data analysis” in which all the observations from individuals or collectivities are gathered indiscriminately across continents, countries or counties – without regard for contextual or contingent properties they may not share. For ‘simplifiers’ this is not only acceptable; it is practically mandatory. For ‘complexifiers’ this would only be justified after explicit sampling to control for variation in such contextual or contingent factors.

<sup>37</sup> No one has insisted on this more consistently than Stanley Hoffmann. :”Obstinate or Obsolete: The Fate of the Nation-State and the Case of Western Europe,” Daedalus, Vol. 95, pp. 862-915.

<sup>38</sup> “Imagining the Future of the Euro-Polity with the Help of New Concepts”, in Gary Marks, Fritz Scharpf, Philippe C. Schmitter and Wolfgang Streeck, Governance in the European Union, (London: Sage Publications, 1996), 121-150.

<sup>39</sup> An example I love to use in my “Research Design” seminar is that of European identity and the European Union. According to a Eurobaromètre survey, the highest proportion of those who answered “yes” to the question “How very often do you feel European?” was 85%. The individuals who produced such an enthusiastic response were ... Albanians! Those actually members of the EU or, then, candidates to join it did not exceed 20%.

<sup>40</sup> Giovanni Sartori, “Concept Misinformation in Comparative Politics,” American Political Science Review, Vol. LXIV, No. (1970), pp. 1033-53.

<sup>41</sup> “What is there to legitimize in the European Union... and how might this be accomplished?” in C. Joerges, Y. Mény, J. H. H. Weiler (eds.) Mountain or Molehill? A Critical Appraisal of the Commission White Paper on Governance, Jean Monnet Working Paper series, no. 6/01 of Harvard Law School

It should be noted that the actors involved in such arrangements are frequently referred to as “stakeholders.” That is a concept that may be even more intrinsically fuzzy than governance itself.

<sup>42</sup> One frequently encounters in the literature that focuses on national or sub-national “governance” another blurred concept, that of **network**, being used to refer to these stable patterns of horizontal interaction between mutually respecting actors. As long as one keeps in mind that with modern means of communication the participants in a network may not even know each other – and certainly never have met face-to-face – then it seems appropriate to extend it to cover transnational and even global arrangements.

<sup>43</sup> Charles C. Ragin, The Comparative Method (Berkeley: University of California Press, 1987); *ibid.*, Constructing Social Research (Thousand Oaks: Pine Forge Press, 1994).

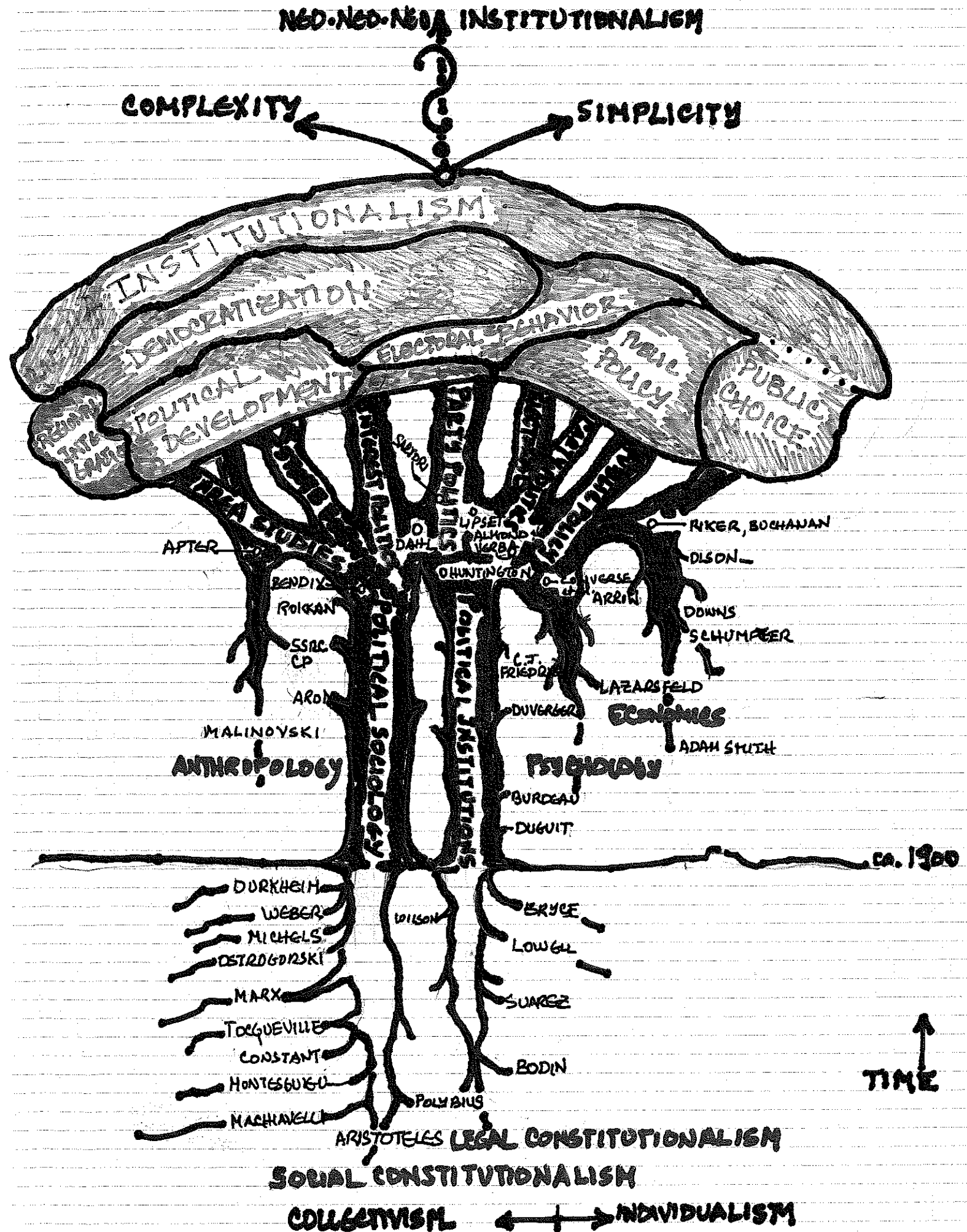
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<sup>44</sup> Charles C. Ragin, Fuzzy-Set Social Science (Chicago: University of Chicago Press, 2000). For two empirical examples of the application of this technique plus a two-stage design, see Carsten Schneider, and Claudius Wagemann.

<sup>45</sup> These have been taken (and slightly modified) from my "Autobiographical Reflections: Or How to Live with a Conceptual Albatross around One's Neck," in Hans Daalder (eds.), Comparative European Politics: The Story of a Profession (London: Pinter, 1997), pp. 295-27.

# THE 'FAMILY TREE' OF COMPARATIVE POLITICS

FIGURE ONE



## CHAPTER SEVEN

### THE WINDS OF POLITICAL SCIENCE ARE 'A CHANGING'

Mao Tse Tung anticipated the provocative theme of this congress. I will begin by paraphrasing him – and in so doing will change somewhat his geographic coordinates and his substantive claims.

He began with an ancient Chinese saying: “Either the East Wind prevails over the West Wind or the West Wind prevails over the East Wind.” I would only add South to East and North to West. I believe that in the discipline of political science it is characteristic of the situation today that neither the East/South nor the West/North Wind prevails. The Winds of change in our discipline have become variable, even unpredictable. They no longer come overwhelmingly from a single direction (as Mao predicted when he proclaimed that the Wind of Eastern socialism would henceforth prevail over the Wind of Western imperialism). Today, innovations in theories, concepts and methods can come from any direction. Moreover, they do not have to originate from large sovereign national states, but they can even come from small, subordinate provincial units or even larger, regional supra-national ones.

Which is not to say that political science is going to be blown about aimlessly by fads and fashions of random origin. There are still a few poles around which

political scientists gather in greater numbers and to which graduate students will be more attracted. But the identity and variety of these poles have proliferated over recent decades. The wind of North/Western imperialism blowing from the United States of America is still strong, but it has become increasingly challenged, not by the polar opposite of South/ Eastern socialism (as Mao would have it), but by eddies of varying strength and substance coming from an alternative North/West pole, namely, an increasingly unified European political science, and diverse centers of innovation in the South, namely, in Latin America and Africa, and the East, namely, Asia. Having first incorporated these exotic places into its imperial domain after World War Two under the guise of area studies, American political science now faces the prospect of being marginalized by practitioners coming from its former academic colonies who are calling into question many of its basic assumptions. For, "Americanists" have long suffered from a fundamental schizophrenia: On the one hand, they insisted that the US was 'exceptional' in its favoured political status and, on the other, that everything they observed about American politics – including the methods they applied to making these observations – was 'universal' and, therefore, could not only be applied as a model for use in comparison with these other, less exceptional countries, but could also be taught to graduate students from these countries who were



expected to transport these same assumptions back to their Southern or Eastern homelands. Ironically, some of the strongest critiques of American political science have been coming from Europeans, Latin Americans, Africans and Asians who were trained in the orthodoxy of American political science.

So, what is important is not just that political science as an academic discipline is now being practiced “tout azimuth” – in every direction, as the French would put – but that the assumptions, methods, concepts and theories it employs are coming less and less from the same source. We are still a very long way being a universal or global discipline in which the training of students, the employment of graduates, the sponsorship of research and the authorship of books and articles no longer reflects national restrictions, cultural distortions and/or hegemonic pretensions. Some nations and regions, however, have become notably more open to the influence of outsiders. As I have shown recently in an analysis of major PS journals in the United States and Europe, by far the most parochial was the APSR, followed by the UK’s Political Studies.

The journals of continental Europe were much more likely to include articles on non-national polities written (or co-written) by non-national authors.

No doubt that a similar comparative analysis would reveal that many students in political science when given the choice to pursue graduate studies in the discipline will still prefer doing so at one of the major departments in the

United States, but my experience at the EUI in Florence and the CEU in Budapest suggests that this is less likely than in the past. Thanks to EU programs such as Erasmus, an increasing proportion of Europeans at least will choose to do their doctorate at the EUI, the CEU or another major European department. In that regional location, it has now become almost a career requisite to spend part of one's graduate education in political science in another country – and that country is less and less likely to be the United States.

My career has hardly been typical – but it may become proto-typical. The future political scientist will be more likely to be educated in and to teach in several different countries – and not all of them within the West or the North. He or she will have teachers and colleagues “tout azimuth” and it will be routine to both design and execute research in collaboration with Easterners or Southerners. And, if that proves too burdensome for financial or other reasons, his or her footnotes and citations will increasingly contain works in other languages and from other continents. Even more important, he or she will be working with assumptions, concepts, theories and methods derived from other, more exotic, political experiences and may even do so without an awareness of having borrowed from the South or East.

Quite accidentally, I found myself involved in this process of shifting polarities on several occasions and on several different topics. Ernst Haas sent me off from Berkeley to the Central American Common Market and the Latin American Free Trade Area to discover whether his Euro-centric, neo-functional approach to trans-national integration could be applied in such exotic places. Admittedly, I ended up concluding that the approach could not without substantial modification because these efforts engaged quite different actors and motives – indeed, I also concluded that they were very unlikely to succeed in attaining even their more modest objectives. Nevertheless, the comparison proved to be insightful not just for understanding the distinctive characteristics of regional integration in these ‘Southern’ settings, but it also led me to a substantial modification of key hypotheses and assumptions with regard to Western European integration which I only got around to applying empirically twenty or more years later when I moved to Italy.

Next, I assimilated into my dissertation research on Brazilian interest politics a numbers of insights and core concepts from the literature on dependency theory – perhaps, the first major transfer in modern political science from the South to the North – and one that subsequently affected both scientific knowledge and political perception within many countries on the periphery of Western Europe. Only subsequently did I discover that the center-periphery

conception and its core mechanism, the uneven evolution of the terms of trade between manufactured products and raw materials, actually had originated in the 1920s in Eastern Europe (more particularly, in Romania) and had been transferred to Latin America after WWII by the founding fathers of the ECLA.

Then, I discovered corporatism in a used-bookshop in Rio de Janeiro and in the writings of a number of Brazilian social scientists of the inter-war period.

Again, the concept (and practice) was originally Western, i.e. European, but it had been obliterated from polite political discourse by the defeat of the Axis. I can well remember how, when I started to apply the concept to some Western European polities with otherwise impeccably democratic regimes, I was bombarded by angry letters and assaulted in reviews claiming that I was defaming the systems of interest intermediation in Sweden, Norway, Denmark, Austria and the Netherlands by accusing them of being clandestine Fascists.

Everyone knew that interest politics in all of the North and West was "pluralist" – even if, a few Scandinavians (e.g. Stein Rokkan) qualified this prestigious concept with the adjective "limited" or "coordinated." Spain and Portugal were regarded as quaint relics where this arrangement was still on display, but they were not really in the right place and were effectively banned from comparison with their neighbours. Somehow, the label corporatist stuck

(along with its juxtaposition to pluralism) to the analysis of Western European politics and is applied today without sensitivity to its Southern or Eastern origins – although it is usually sanitized by placing the prefix “neo-” or the adjective “societal” before it.

Finally, and most importantly, there was the study of democratization. The Northern and Southern literatures were preoccupied with the stability of already-existing democracies and research into how these polities became democratic was left to the particularizing propensities of historians working on individual countries. To the extent that a “model” of this process existed, it followed the British configuration of gradual, sequential reforms in contestation and inclusion (to use Robert Dahl’s coordinates). Those that had manifestly not followed this historical pattern – France, Italy and Germany – were regarded as intrinsically unstable (and inferior) democracies. My work previous to the mid-1970s was impeccably concerned with non-democracies – whether in the Latin American or the European South. However, when the Portuguese revolution of 1974 quite unexpectedly forced me to pay attention to democratization, the most inspiring article I could find initially was by Dankwart Rustow, a political scientist who had gone East and South to Turkey and then returned North and West to Sweden and somehow developed during this peculiar trajectory a more dynamic, contingent and actor-centric

conceptualization of how democracies came about. When Guillermo O'Donnell and I were encouraged to put together a working group on this topic, we did not hesitate to recruit scholars from Southern Europe and Latin America – and I am convinced that a great deal of the subsequent success of the *Transitions from Authoritarian Rule* volumes was due to its inter-regional comparative nature. This certainly forced me – helped admittedly by a strong affliction with Niccolo Machiavelli – to think generically and to quite deliberately express myself in a new “Southern” vocabulary, rather than to fall back on the prevailing assumptions about alleged ‘pre-requisites’ for democracy or about the pivotal role of political parties. The neo-logisms of “Transitology” and “Consolidology” may have made a bit of a laughing stock of me – Guillermo explicitly defected from the latter – but they did serve to demarcate the distinctiveness of my approach –and this was picked up and used extensively in the analysis of subsequent democratizations in Eastern Europe, the former Soviet Union and Asia. Again, the South Wind – even if it was the South of the West Wind – made a contribution to changing the direction of political science.

And just now, I have returned from yet another trans-regional experience – this time and for the first time with being buffeted by the East (and not the South) Wind, although I definitely brought some of the West Wind with me. I

was invited to teach a full semester course at Fudan University in Shanghai and I chose the topic of “The Theory and Practice of ‘Real-Existing’ – i.e. Western Liberal Representative Constitutional – Democracy.” I also travelled extensively to other universities and research centers in China (including the Central Party School in Beijing) – almost always lecturing on some aspect of democracy or democratization. What made this all a greater contrast was the fact that I did this knowing literally nothing about contemporary (or, for that matter, historical) China. I defended my ignorance by hiding behind de Tocqueville who also proclaimed that he had read very little about the United States before going there and then writing one of the greatest books on comparative politics of all time. Sometime, the shift in wind direction can be more insightful when one has not been previously exposed to it.

While it would be an exaggeration to proclaim that the future of political science – even its vocation as a universalistic discipline in the social sciences – will depend on its development in China, I am convinced that the next really big gust of wind is going to come from there. The government (I should say the Party) has declared its ambition to make political science into an important academic discipline and has allocated very substantial sums to creating departments of political science all over the country. Research institutes are proliferating on more specialized topics and the Chinese Academy of Social

Sciences has a very substantial political science component. Thousands of Chinese graduate students are being sent abroad to do PhDs and they are being carefully distributed to avoid an exclusive concentration on the United States. And they are returning to their home country and providing most of the staff in these new departments and institutes – although there has also been a substantial develop of 'native' PhD programmes. At the present moment, Americans represent the largest numerical concentration of professional political scientists – by my calculation almost as much as all Europeans put together – but I would not be surprised if in the relatively near future Chinese political scientists will outnumber Americans. Whether they will succeed in having as much of an influence on the discipline elsewhere is another matter, but who knows?

The Party/Government's motive for this is unusual and distinctively Chinese. Its leaders are acutely aware that the collapse of the Soviet system was precipitated by the Party's lack of information about real-existing social and economic conditions, and political reactions to these conditions. One party states have an intrinsic propensity to rely on unreliable information, especially that provided by its own monopolistic agents in the field. The incentives for career success within the Party literally compel these agents to send on mis-information about what is going on in their district or enterprise. And this



pyramids up the hierarchy and can be responsible for making self-destructive decisions at the top. Democracies with their competitive parties, associations and movements generate lots of good information – while complicating greatly the process of aggregating this information into “authoritative allocations of values” as Easton put it.

Chinese rulers are experimenting at the local level with more competitive forms of nomination, consultation and even election, but there are obvious limits to this and retaining the Party’s monopoly over state power. The emergence of civil society and – believe it or not – corporatist forms of interest representation are very major topics of discussion, both inside and outside the Party. Survey research is being conducted on a massive scale and even covering some politically sensitive topics (although I was told that issues such as public opinion with regard to human rights, federalism, nationalist sentiments on the periphery and the desirability of multi-party competition are still out of bounds). And what is the academic discipline that can generate the highest degree of accurate information on these topics without threatening the Party’s hegemony: political science, of course – especially if it is practiced exclusively within state institutions of higher education and research where appointments and projects can be monitored.

A bit to my surprise, I encountered no resistance or reservations about giving a course on democratic theory and practice. No one vetted my syllabus or blocked access to the reading I placed on the internet or distributed in class. As far as I could tell, no government official monitored my classroom although several junior colleagues did sit in occasionally. What I did experience was a great deal of interest in learning how 'real-existing' Western democracies function – and incidentally a lot of interest in how newly-existing Southern and Eastern democracies are functioning. Taiwan, in particular, but also South Korea and more recently Indonesia are regarded as especially significant "others" – if only because there exists an almost mystical belief in the distinctiveness of Asian political culture in its varied national configurations.

However, my net impression from the course at Fudan and the lectures elsewhere was that the desire to learn from me how these Western, Eastern and Southern 'real-existing' democracies are functioning had little or nothing to do with imitating them. Chinese social scientists are supremely confident in the strength and uniqueness of their own "several thousand year old" civilization and in the inappropriateness of Western democracy for governing this enormous and culturally different society. They were more interested in the mistakes and failures of 'real-existing' democracy than its successes – and hoped, I believe, to learn from me what to avoid rather than what to imitate!

The central debate going on as we speak among this expanding and ambitious group of political scientists is whether to sponsor the creation of “Political Science in China” or “Chinese Political Science.” Whether China should simply do its best to assimilate the concepts, theories and methods as best practiced in the North and West, or to create a new political science rooted in the distinctiveness of Chinese culture, society, economy and polity. I did not feel competent to enter into this debate – being obviously biased in my experience and ignorance in favour of the former alternative. The easiest way out – which I took – was to plead for a “Political Science with Chinese Characteristics.”

Whatever the eventual resolution of this dispute will be – and much of the outcome will probably depend on the critical reception that those hoards of PhD and MA candidates now studying abroad will bring back from their exposure to Western and Northern political science – the very tension played out on such a vast scale with this embryonic discipline will be conducive to the generation of novel assumptions, procedures and standards which cannot avoid influencing those of well-established political science communities. The Southern Wind has already proven strong enough to have affected special niches in the West and North; the coming East Wind may not prevail exclusively as Mao predicted but I am convinced that it will blow away at least some of our main-stream assumptions and hypotheses – not the least of which

is that for political science to flourish the polity within which it operates has to be democratic.

## **CHAPTER EIGHT**

### **The Confessions of a Repeat Offending and Unrepentant Conceptualist**

When I was informed of this Award, my initial reaction was “Why Me?” Guillermo O’Donnell was the first winner of it and I know why he was chosen, but I am not Guillermo.

I thought and immediately rejected several of the usual suspects:

#### **1) Service to the Profession**

I have not been active in either IPSA or any other national political science association – and especially not in my own, the APSA. I do not regularly go to their annual meetings. I have been virtually allergic to all institutional aspects of “the discipline.” For six painful months, I did once serve as the chair of my department at the EUI, but I managed to get out of that as soon as I could. At Chicago and Stanford, no one – not even my closest colleagues -- would have considered making me their chair.

#### **2) Publications in Prestigious, Peer-Reviewed Journals**

I have never published an article in the APSR. In fact, I have never even submitted a manuscript to it or to many other highly ranked American journals! I am, however, proud of the fact that I have published pieces in the national political science journals of some 13 other countries – some of them in very obscure places and languages – but since when has that been regarded as so important?

### 3) 'Scientific' Knowledge in the Discipline

I am also convinced that it has not been my net contribution to improving the scientific status or adding to the empirical content of political science. I admit to having conducted a lot of interviews, gathered systematically data from public sources in several countries, designed and applied a few questionnaires, run early versions of IBM card-sort machines to produce cross-tabulations, regressed mountains of aggregate data on each other and even having tried my hand at factor and small space analysis. These days I am deep into a topographic analysis of the relationship between varieties of capitalism and types of democracy. I am not apologetic about having done this and am still proud of the associations I discovered and the inferences I made based on them. But, again, there are many out

there in this audience who have more to show for such efforts than I have.

#### 4) Follower of Trendy Theories

I have never been nor am I presently a card-carrying member of any of the fashionable theories that have swept through the discipline during my career. I have neither been a behaviourist, nor a structural-functionalist, nor a systems theorist, nor a rational-choicist. Indeed, I have been manifestly sceptical about all of them. I would even refuse to accept the presently appealing designation of "historical institutionalist," although it comes closer to identifying me than any other.

So, this leaves only one plausible reason for awarding me the Mattei Dogan Prize: I have been a conceptualist. I have been in the business of identifying patterns of political phenomena, sticking labels on them and exploring their consequences as well as their causes.

In this regard, I have been fortunate to have worked on three weakly related and profoundly ambiguous topics and to do so in a wide variety of settings, both temporal and spatial. This has compelled me to think laterally – to search for analogies that stretched from one to

the other and for concepts coming from other substantive issues or academic disciplines. I have had to jump around rather than to forge ahead in concentrated pursuit of one research objective.

The first, regional integration, was a novel political phenomenon lodged uncomfortably between two separate literatures (international relations and comparative politics) that could not be explained by the premises, concepts or mechanisms of either. But fortunately for me, it was already being masterfully explored by my mentor at Berkeley, Ernst B. Haas. All I had to do was follow in his footsteps (even after he had declared them "obsolescent") and also to push inquiry into regions other than Western Europe. It was precisely the failure of trans-national integration in Central America and South America that led me to re-conceptualize the bases for its success in Western Europe, as well as the range of its possible outcomes.

The second was the politics of organized interests which was a well-established field of research completely dominated by a single paradigm associated with a single country, the United States.

Pluralism offered me as a graduate student a varied and powerful set



of assumptions, concepts and findings which I was fortunate enough to take to Brazil in my dissertation project – where they manifestly did not fit! Again, it was the failure to conform to expectations or established wisdom that provided just the stimulus I needed to conceive of an alternative mode of interest intermediation which I called corporatism. The fact that in order to find this concept and to extract all of its attributes and implications I had to move back in time and elsewhere in space made the effort all the more “lateral,”

The third, democratization, was hardly a novel phenomenon, but it was surprisingly “under-conceptualized” and “under-theorized.” What Guillermo O’Donnell, our collaborators in the Woodrow Wilson Project and I discovered in the early 1980s was an enormous and highly influential literature that stressed the static relationship between so-called structural and cultural pre-requisites and the stability of a few “Western” liberal-democratic regimes. There was virtually nothing theoretically intuitive and generally relevant (except for an obscure article by Dankward Rostow) about how these regimes came into being! Moreover, the countries that then interested us in Southern Europe and Latin America had none of these alleged pre-requisites. This time it was not frustrated empirical

expectations but frustrated normative desires that led us to develop an alternative “possibilist” conceptualization of Transitions from Authoritarian Rule. And, I admit a bit to my surprise, our “thoughtful wishing” turned out to be empirically and not just normatively correct.

So, I think that I am being honoured primarily for my role as a “conceptualist.” This has caused me to reflect on how I have gone about this unusual task. What are the “Tricks of the Trade” of such a rare specialization in political science? Why is one concept better than another? How does one size up an existing literature and improve on it? And, what do you do if there is virtually nothing to begin with?

I will condense my retrospective thoughts on this matter into nine sets of observations.

1. I begin (why not?) with Aristotle: “It is the mark of an educated man to look for precision in each class of things (read: discipline or sub-discipline) just so far as the nature of the subject admits”

Nicomachaen Ethics, Book 1, Chapter 3, 2-3.

In our times, I have taken this to mean that conceptualization in political science must acknowledge and, therefore, reflect the growing complexity and contingency of its various subject matters. (Reference to EPSR article, Vol. I, No. 1). This means, in my view, that the most useful new concepts will be multi-variate in their composition and fuzzy in their boundaries. Ideal-types as developed by Max Weber or constructive-types as I prefer to think of them are the most obvious instruments. These cannot be “dis-aggregated” into simple modules of individual action or rational choice and subsequently “re-aggregated” in order to explain -- always *ex post* -- collective institutions or decisions. Their utility depends specifically on the sum of their contingent effects, i.e. on changing linear into non-linear relations, on evoking dormant or suppressed motivations, on adjusting preferences to merging conditions, or on turning anticipated into unanticipated outcomes.

2. The search for conceptual innovation usually begins with the perception of an empirical anomaly – especially one that affects more than one unit. Singular anomalies tend to be dismissed or explained away as the product of “political culture” or some unique historical configuration. But when there is a disparity between theoretically

expected behaviours or between actor assumptions about anticipated outcomes in several locations – or in a “crucial case” where such expectations were initially very high -- then, then there is an obvious opportunity for finding a new or revised concept that seems to explain that anomaly. Not infrequently, these ambiguous spaces tend to open up between the specialized and well-defended sub-disciplines of political science – in my case, between comparative politics and international relations. These spaces can also emerge from the sort of “lateral thinking” that comes from working simultaneously on several substantive problems and reading widely in adjacent literatures. Focusing on a single issue from the perspective of a single approach – however appealing to the scholar and rewarded by the discipline – is not the usual way to come up with conceptual innovations.

*NB that the conceptualist is rarely a theorist – at least, not at the beginning. The urge is to find a lexica that is not incompatible with a pre-existing schemata – a new word not a new grammar – but one that “fits better” within the more encompassing pre-existing paradigm. However modest the initial intention, new concepts can develop into highly contentious “Kuhnian paradigm shifters” and, thereby, bring*

*down the wrath of "main-streamers" within the discipline. I learned from Albert Hirschmann (he called it the Streeten Maxim) that "no theory or paradigm is ever defeated by data, only by another and better theory or paradigm."*

3. To be convincing and work effectively, a new or re-furbished concept should not be just a descriptor of an empirical or normative anomaly. It should also include a reference to a plausible political mechanism or process that connects with the unexpected or the unwanted. Rarely will this be an entirely novel functional connection, intentional logic or normative reaction. Usually, it involves the identification of some ignored or overlooked arrangement that brings to bear on a given subject-matter the usual causes, reasons and motives but in a novel combination. For political scientists almost always this means a relationship of power that endures long enough to produce observable and predictable effects, i.e. an institution or, more modestly, a rule of prudence. They are not only the most visible manifestations of anomalous behaviour, but they are also the most likely to produce consequential outcomes.

*NB that not all conceptual innovations are linked to institutions, but new concepts that purport to refer to major transformations in values, shifts in material preferences, changes in the basis of political calculation, and so forth are usually met with greater professional and public scepticism. Compared to institutions or processes, they are much more difficult to observe in a direct and convincing fashion – although the recent success of “social trust” suggests that this is not always the case. Sometimes a concept is useful precisely because it can only be inferred vicariously and not measured precisely.*

4. The simplest tactic for a conceptualist is just to add “neo-” or “post-” in front of an existing term – implying a temporal shift in its meaning or consequences. Another I have used frequently is to make creative use of antimonies. Take the existing causal assumption and invert it, for example, by making pre-requisites into post-requisites. (e.g. “civic political culture”). Or explore further a prominent ideal-type by imagining its opposite, with each of its sub-components taking on an inverse property – and then see if that fits the observed anomaly. That, of course, is what I did with pluralism and corporatism – and Juan Linz did with his distinction between totalitarian and authoritarian regimes.

5. All concepts in political science are historical and, hence, draw their relevance from the temporal sequences and bounded contexts in which they are discovered and placed by the scholar. Which does not mean that one cannot “fish them out” of another epoch – I have done this several times – but only with the explicit understanding that they are bound to be different when resuscitated this time around. It is wise to assume that all but very few and very recent political phenomena have already been identified and labelled – somewhere. The trick is to discover who said it, when and in what context. In so doing, the conceptualist will learn a lot about surrounding normative assumptions and socio-political pre-conditions, and this should be of assistance when it comes to re-inserting the old concept into its new setting.

Moreover, once this has been done successfully, the concept will continue to change – often in ways that are unexpected by its author. For a good political concept acquires its own life – and half-life. The more often it is used (which inevitably involves placing it in different contexts and associating it with other concepts), the more its meaning will change from the original. Often, I have discovered, this

is evidenced by the placing of an increasing number and variety of adjectives in front of it.

*NB Do not try to fight against this. One can remind the profession repeatedly of the 'pure' meaning originally attached to the concept and continue to defend the prescribed scope of its applicability, but there is no way conceptualists can "legislate" the eventual use of their products. Perhaps this is why many of the most useful concepts in political science have been and continue to be "essentially contested." The conceptualist should take a certain pride in the multiplicity of (distorted) meanings and (mis-)uses that emerge in response to his or her innovation. This is an unobtrusive (if annoying) indicator of success. My experience suggests that, at some point, one is better off simply walking away from one's creation and focusing attention on some other subject of inquiry.*

6. All concepts – new, old or revived – are embedded in a wider set of prior assumptions and other concepts. They are "building blocks" that are only as plausible and valuable as the foundations upon which they rest. No one of them works alone, even though it may take considerable theoretical explication and empirical research to bring out how wide and extensive these foundations really are. My



experience suggests that the conceptualist is often not aware of the extent of this embeddedness at the beginning and only learns about it once the concept is being used (and criticized) by others.

The most difficult skill to acquire is not that of identifying the core meaning of a concept, but that of learning when to assert closure around it – especially if, as I have argued above, the most valuable concepts should be capable of reflecting the increased complexity and contingency of contemporary politics. Precisely because they are so embedded, it is difficult to separate any one concept from its context. Just because concepts have to be multi-dimensional, it is difficult to decide what should be included as one of their multiple defining elements and what should be excluded. Where does one draw the line between endogeneity and exogeneity with regard to such concepts? These are strategic choices for which there are few guidelines that I know of. Collaboration with another scholar – especially when it involves very intensive verbal interaction (i.e. arguing with each other) – can be helpful in drawing these lines. Once two minds have settled on a compound definition and its fuzzy boundaries, it is much more likely to survive the subsequent scrutiny of less well-disposed others. But there is no way to avoid the need

for reification, i.e. to settle eventually on a prescribed set of attributes that are presumed to cohere and to persist at least long enough to produce some effect – whether one does this alone or with someone else. To deny this (and I think many “post-modernists” would do so) would be to assert that politics takes place in such a state of constant flux and mutation that all we can aspire to do would be to produce momentary snapshots or idiosyncratic descriptions of that reality.

7. All concepts are doubly normative in the sense that they evoke positive or negative reactions from those described by them and invoke positive or negative evaluations from the scholar who uses them. Try as hard as one may, no political concepts can be completely neutral and, the more important they are, the more likely their deployment will be perceived as having a differential impact upon the “real-existing” political process. Unless the analysts and protagonists all come from the same unusually homogeneous culture, there is a high likelihood that these concepts, especially the more innovative ones, will provoke different responses and, therefore, that the work of conceptualists will become controversial. Indeed, if it is not, their concepts are probably so bland or non-committal as to be worthless. In other words, the conceptualization of politics tends to

become part of the process of politics itself and nowhere is this more the case than in liberal democracies where actors are free and even encouraged to take up and work out the implications of any linguistic innovation. Without the ability to argue about words (and to reach compromises based on them), the only way that political conflicts can be resolved would be by force or the threat of it. However, it is this intrinsic controversiality that is so valuable for conceptualists because it virtually guarantees that all of their significant innovations will be scrutinized for meanings and implications by a variety of actors – and that competition (plus their application by scholars in comparative research) will ensure that only the fittest of them will survive -- one can hope.

*NB This Darwinian optimism may often be misplaced since it ignores the presence of powerful groups within both the polity and the discipline of political science with not just normative but also vested material, career and status interests in the persistence of established conceptualizations (and the methodological tools so often linked to them). When and where these two groups are de facto allied with each other and the prevailing concepts benefit them both, innovations may*

*not get a fair hearing and only those that reinforce the dominant paradigm may get through.*

8. Almost all political science concepts – even the most innovative ones -- have an ordinary language origin and, therefore, prior meaning. Scholars often use the device of placing quotation marks or inverted commas around the normal term as if that would be sufficient to separate it from the scientific one. It seems to me better to assume that the two are indissoluble and to use this to advantage. The everyday language invented by politicians and those who directly observe them (i.e. journalists and editorialists) to explain to themselves and others what they are doing should be treated as a major potential source of inspiration. [Reference to "Les Intraduisibles"] One should never forget that most of politics is about words – their use or misuse and not their clarity or precision – and that the outcome of these struggles for linguistic dominance can have real consequences. I have been fortunate to have done research and to have lived in two countries, Brazil and Italy, with unusually creative political lexicons and this has proven very useful to me. No one who studies the process of European integration can afford to overlook the extraordinarily rich "supra-national" language that has

grown up around its practices – sometimes referred to as “Euro-speak.” Knowing several languages and immersing oneself in the context in which they are spoken thus can be of obvious advantage to the conceptualist. Admittedly, the overlap between ‘normal’ and ‘scholarly’ languages can lead to mutual confusion, but it can also be an important source of potential insight and further development since the reactions of “ordinary” observers (especially of political participants and politicians) can alert the conceptualist to the presence of substantive meanings (and normative reactions) that would be missed by a purely academic audience. The conclusion is unescapable (at least, to me) that academics can only re-conceptualize political reality within the admittedly vague and movable parameters imposed by those who practice within it – whether they are in or out of power.

*NB Beware of concepts that fail to “resonate” beyond the academic audience to which they are originally addressed. They are probably based on unrealistic assumptions or unrealizable principles. [Dahl’s polyarchy” as an interesting marginal example. Bar in Krakov] The dialogical process of acceptance and rejection by other (sub-) disciplines and wider publics that tends to surround the reception of*

*conceptual innovations constitutes an important instrument of "self-correction." The strongest test comes when a concept is "stretched" and applied to cases it was not originally intended to cover. This point also suggests the proper place to be on the "ladder of abstraction," namely, on the highest rung upon which one can communicate to both specialists and generalists, to professional students of politics and amateur practitioners of it. Needless to say, the more of these that "pick up on" a given concept (even if negatively), the greater its success – even if that probably entails a dissolution and even a distortion of its original intent.*

9. The *mestiere* of conceptualist in political science should be a part-time one – unlike the more prestigious full-time ones of theorist, methodologist, empiricist and (more recently) modelist. To do it well requires frequent interaction with "real-existing" political actors, reading lots of newspaper articles and editorials, immersing oneself in ancient texts by sometimes obscure authors, working on several topics at the same time, engaging in periodic bouts of data-gathering and crunching and accepting invitations to give talks in exotic places. Not only will these "distractions" provide inspiration for coming up with new (or renewed) concepts, but they also help to correct for the

inevitable distortions and abeyances in one's artefactual products. No doubt it helps to know one's own language well and there is no better way of doing so than to learn as many other languages as possible. Incidentally, using (and in my case abusing) Latin can be especially useful since it helps to distance one's concepts from their original national associations – and, of course, it makes one sound more learned. Living and teaching in different places during one's career is also not a bad idea. Collaborating closely with scholars who know more than you and who come from different countries and intellectual traditions – Guillermo O'Donnell, Wolfgang Streeck and Claus Offe, for example – has kept me going laterally. My most important and enduring collaborator has been Terry Karl, who for 30 years has made critical contributions to my work and has come up with many creative concepts of her own. My doctoral students at Chicago, Stanford, the EUI, the CEU and other more occasional places have also been frequent sources of inspiration – and I think that they know this. They have come from an unusual diversity of places and backgrounds – and this has been of inestimable value. Some of my best thoughts about politics come from discussions with them during office hours.

For those of you who are just starting in the profession, your career prospects are not great should you choose this line of work. No department or faculty that I know of has a designated slot for a “conceptualist.” You will have to make it at first under some other *rubrique* of the profession and you will need to have acquired a general knowledge about politics, history and language before coming up with something valuable. Moreover, your contributions – once you make them – will be criticised from diverse perspectives – not only by other academic specialists who will think that you are intruding on their turf, but also by politicians and activists who will accuse you of distorting their motives or helping their opponents. Worse of all, your best concepts will eventually be appropriated and assimilated into the mainstream – not infrequently without attribution. The keepers of orthodoxy in the discipline may even pay you the ultimate insult and accuse you of merely “having put new labels on (their) old bottles.” Theories have authors; methods have schools; models have status – concepts have only “sources” and they tend to fade away with time. The better a given concept “fits” and becomes useful within the existing *corpus*, the less relevant becomes the identity of the person who created it.



I have no regrets. I have enjoyed being a part-time conceptualist. And the Mattei Dogan Award from IPSA comes as an unexpected, but very gratifying reminder that my peers have recognized me as the source of a few conceptual innovations. I thank all of you who thought I was worthy of receiving this Award ward – and for your attention to this talk.

## CHAPTER TEN

### CORPORATISM, DEMOCRACY AND CONCEPTUAL TRAVELING IN

Gerardo L. Munck and Richard Snyder

Passion, Craft, and Method in Comparative Politics

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Philippe C. Schmitter is widely regarded as the most influential analyst of corporatism—a pattern of interest group politics in which the state plays a significant role in the formation and activities of interest groups—a leading theorist of democratization, and an important contributor to the study of regional integration and the European Union (EU). His research has spanned many regions of the world. He established himself first as a Latin Americanist and Europeanist, and then authored numerous cross-regional studies in which he avidly sought to extend insights drawn from cases in one region to cases in other regions.

At the outset of his career, Schmitter challenged the then dominant pluralist approach to interest groups, criticizing it for seeing these groups as springing autonomously from civil society and thus failing to recognize how their formation and functioning are shaped largely by the actions of the state. He proposed the concept of corporatism as an alternative to pluralist conceptions of interest group politics. The first formulations of Schmitter's thinking on pluralism and corporatism were published in *Interest Conflict and Political Change in Brazil* (1971) and his widely read article "Still the Century of Corporatism?" (Review of Politics 1974). Thereafter, his research focused on Portugal, which he analyzed, like Brazil, as a case of "state corporatism," and then shifted to multiple Western European countries, which were understood as instances of "societal corporatism."<sup>1</sup> His research on societal corporatism addressed questions such as, What role does the state have in setting up and maintaining corporatist arrangements? This interview was conducted by Gerardo Munck in Notre Dame, Indiana, on December 4–5, 2002.

1. According to Schmitter (1974, 102–3), in "societal corporatism" the legitimacy and functioning of the state are primarily or exclusively dependent on the activity of singular, noncompetitive, hierarchically ordered representative corporations, whereas in "state corporatism" similarly structured "corporations" are created by and kept as auxiliary and dependent organs of the state. Who benefits from corporatism? What is the relationship between corporatism and democracy? How viable is corporatism in a changing international economic context?

The study of democratization has been a second key concern in Schmitter's career. His coauthored book with Guillermo O'Donnell, *Transitions from Authoritarian Rule: Tentative Conclusions about Uncertain Democracies* (1986), was one of the most widely read and influential works in comparative politics during the 1980s and 1990s. It also initiated a debate, in which Schmitter actively participated, concerning the possibility of extending ideas about transitions from authoritarianism developed through an analysis of the Southern European and Latin American experiences to Eastern Europe and the Soviet Union, as well as to the case of South Africa. He has also contributed to the literature on the international dimension of democratization, emphasizing international efforts at democracy promotion and protection, and he has written seminal articles on democratic consolidation and the quality of democracy.

Finally, Schmitter has published extensively on regional integration in Europe and the political characteristics of the EU. This research, published in *How to Democratize the EU . . . and Why Bother?* (2000a) and various articles, considers the possibility of democracy at a level above the nationstate, the unit of governance assumed by classic democratic theory.

Schmitter was born in Washington, D.C., in 1936. He received his B.A. from Dartmouth College in 1957 and his Ph.D. in political science from the University of California, Berkeley, in 1967. Schmitter taught at the University of Chicago (1967–82), Stanford University (1986–96), and the European University Institute (EUI) (1982–86, 1996–2004). He became a professor emeritus at Stanford University in 1999 and was named Professorial Fellow at the EUI in 2004. He was vice-president of the American Political Science Association (APSA) in 1983–84.

#### Intellectual Formation, Training, and Dissertation Research

Q: How did you get interested in studying politics and comparative politics in particular?

A: I lived in a somewhat political family, and I grew up in an environment where comparison was a daily event. My mother was French, and my father was American with Swiss origins. Though I was born in the United States, my family lived initially in Switzerland. My father was working for the League of Nations in the International Labor Organization (ILO). When the war broke out, we moved to the United States. So I spent the war years in the United States. My father worked for the International Organization section of the State Department during the war. So I came in contact with people from all over the place, most of them refugees. After the war, we moved briefly back to France and I went to school there.

I was forced to live a comparative life. I lived in a milieu where you never knew the nationality, religion, or even the ethnicity of the next person

walking into the house. I regarded this as quite normal. I never thought of the comparative part as a choice. I also never thought about political science or social science in general until much, much later. Let's put it this way: I did not choose comparison; comparison chose me. And I eventually chose political science.

Q: How did you come to study politics?

A: I got a B.A. in international relations at Dartmouth College and, after taking a break to study art in Mexico, I went to Geneva to pursue graduate work. I studied international relations, international public law, and international political economy. I found these most uninteresting subjects, and I found Switzerland a most uninteresting place. I was bored to death with academic matters and didn't know what to do, when Stanley Hoffman, a Harvard professor, showed up and gave a semester course on French politics. Stanley was a very French-thinking person, and it was a very French course—basically organized around a comparison between the Fourth and the Fifth Republics. Stanley was an outrageous Gaullist at the time, whereas I came from a socialist and anti-Gaullist family. I remember arguing a lot with Stanley. This was about 1959, around the time the Fifth Republic had been set up. Stanley was full of great man theories about how De Gaulle had providentially saved France. I thought what De Gaulle had done was a big mistake. Still, this was my first real exposure to comparative politics, and I learned two things. First, I realized domestic politics was a lot more interesting than international politics. International politics was very boring, and the theory of international politics did not appeal to me. That reaction may have stemmed from my formation, because my family was Quaker, pacifist, and very much oriented toward international organizations. Indeed, my father had devoted his life to them. Therefore, I had an idealistic vision of what international relations could be like. And, of course, the world wasn't like that.

Second, my contact with Stanley Hoffman led me to think about going to graduate school in the United States. As a student in Geneva, I had briefly worked for the ILO and found it the most stifling environment imaginable. Even though I had gone to Geneva to prepare myself for a career as an international civil servant, I was discovering that I was not constitutionally capable of becoming one. Yet I had never thought of an academic career at all up to this point. Rather, the alternative I had considered was to be a painter. I still wanted to be a painter. In fact, I had a long beard and was a sort of beatnik, which was not your typical college appearance back then. So, I looked, and probably acted, a bit weird for the times. In any case, I took this seminar from Hoffman. What I liked about Hoffman was that you could disagree with him, and he actually seemed to like it. Now, if you went to university in Europe then, that was not normal. I had had a bunch of pontificating, non-dialogical professors. Here comes this illustrious Harvard

professor, whom you could argue with, and he gave me some important advice. I'll never forget the day I went to his office at the end of his seminar. I said, "I'm about to get my degree, and I don't know what I'm going to do. I may stay here and do a doctorate in Switzerland." He looked at me and said, "You don't want to do that." He somehow had learned enough about me from my behavior in his classroom to realize I wasn't going to do well in the Swiss environment. "You should go back to the United States. You have to get a doctorate in the United States. That's the only place." Then he said, "You should go to Berkeley and you should study with Ernst Haas." I had read some of Ernst B. Haas's work in another course, and I had liked it. Somehow Stanley Hoffman made that connection.\

Q: Did you follow this advice?

A: Yes. Hoffman explained that you had to apply to four or five U.S. graduate schools, because you never know where you were going to get admitted. I applied to Berkeley, Harvard, Columbia, and probably Princeton. I don't remember if I applied to Yale. I didn't know much about these schools or the American system in general. As it turned out, Berkeley was the only school that took me. I had actually lived in Berkeley before, as a painter. I had studied painting at an academy in Mexico City. From Mexico I went to San Francisco and then Berkeley. I was painting there and sort of knew the Bay Area scene. I had started a friendship with a man named Judd Boyton, an architect who was building houses up on Panoramic Way above the campus. Now, I used to build houses during the summers before I became a painter. I had been a carpenter, a bricklayer, a cement worker/hod carrier, and a rod buster/welder. I liked physical work. So, while I was painting in Berkeley, I made money on the side building a house with Judd. I had no money at all when I applied to graduate school, but I knew that if I went back to Berkeley, I would somehow survive on the meager assistantship they offered me and working, if need be, on the side with Judd. So I went off to Berkeley.

Just before moving to Berkeley, I had an accidental experience that turned out to be quite important. When I was preparing for my final exams in Geneva, I went to Venice to visit a friend. She had a beautiful apartment on one of the side canals. My visit happened to coincide with the Biennale. Every year they picked one country as the centerpiece of the exhibit, and that year it was Brazil. I simply fell in love with Brazilian art. They also had Brazilian music playing in the background. I decided that Brazil must be an absolutely fascinating place. I didn't know anything about Brazilian politics, nothing, but I formed a powerful image of the country as a syncretic mixture of all types of different cultures and as a place capable of producing astonishingly lively and beautiful art. I decided I wanted to live in Brazil at some point. I went to Berkeley in 1961 with the objective of working with Ernst Haas and figuring out some way to go to Brazil.

Q: What are your recollections about your studies at Berkeley?

A: When I arrived at Berkeley, I had only vaguely heard of political science. While studying in Geneva, I'd been a periodic visitor to Paris and sat in on courses at the Sorbonne. There, science politique was quite formalistic and strongly linked with the study of law. This was very different from what I encountered at Berkeley. At that time, Berkeley had perhaps the best combination of Sociology and Political Science departments in the States. I took advantage of this by enrolling in as many courses in sociology as political science, which turned out to be a good thing. I learned a lot from people like Seymour Martin Lipset, Reinhard Bendix, David Apter, and, of course, Ernst Haas.

Early on I started talking to Haas. I would never have called him Ernie in those days, about what I really wanted to do. I told him I knew Spanish—I had learned it while painting in Mexico—and that I wanted to work on Latin America and international relations. He suggested I try to apply European integration theory to Central America and Latin America. At that time, in the early 1960s, the most successful Third World integration organization was the Central American Common Market (Mercado Común Centro Americano). There was also a promising thing called ALALC,<sup>2</sup> that nobody had studied. So, I worked with Haas as a research assistant on Central America. I also went there and to Mexico to conduct interviews. The first paper I published was on the Mexican decision to enter ALALC (Schmitter and Haas 1964), which was a bit of a puzzle, because Mexico had virtually zero trade with the rest of its member countries.

Q: Though you were working initially on international relations, you eventually shifted to comparative politics.

A: In the process of doing research on integration in Latin America, I got more and more interested in domestic politics. Haas recognized that I really wanted to do comparative politics, and he supported me. Haas was a maverick within the international relations field, because he approached European integration not as an international relations issue, but as a problem at the intersection between domestic politics and international relations. His book, *The Uniting of Europe* (Haas 1958), was not only about diplomatic, country-to-country relations, but also about what was happening to internal political structures, especially the domestic and transnational interest groups that were forming around the European Coal and Steel Community. In the 1950s, Haas was already breaking the artificial boundary between international relations and comparative politics. He did not mind at all when I defected from international relations to comparative politics. There is something else I should explain about Haas. During the period I was at Berkeley, we graduate students were in the middle of the anti-Vietnam War movement and he, like many other European academic refugees in the United States, was very strongly pro-government and anticommunist. I was on the opposite side, but

this did not ruin our relationship.<sup>4</sup> Despite the fact that on a day-to-day political basis we were absolutely opposed, we were still able to work together.

Q: What comparative politics courses did you take at Berkeley?

A: The first course I took in comparative politics was from David Apter. Apter really got me excited about comparative politics. He taught from a political anthropology and systems theory perspective, which I didn't really take to. But David is very charismatic. He also allowed you to disagree with him, which I have always appreciated. The most important course I took, as far as comparative politics was concerned, was a seminar on political sociology with Seymour Martin Lipset. After Haas, Lipset was the second biggest influence on me at Berkeley.

Q: What other courses did you take?

A: I took several courses with Sheldon Wolin, who was also a major influence. He taught a two-quarter obligatory sequence on the history of political thought that began with Aristotle and Plato and ended with Marxism. It also covered some existential political thought, because Sheldon was very much into that. I also took a seminar with Hanna Pitkin, who had just finished a book on representation (Pitkin 1967), and her course got me very much centered on that concept. In the Sociology Department, I took a seminar with Neal Smelser on the theory of revolution, which I took at the

3. The European Steel and Coal Community, established in 1952 by the Treaty of Paris, is the precursor treaty organization of what has become the European Union (EU).

4. Schmitter's first publications were coauthored with Haas: Schmitter and Haas (1964) and Haas and Schmitter (1964).

same time as a formally identical one with Chalmers Johnson. Now, that was a contrast! I also took a sociology course on "The Politics of Mass Society," or something like that, from William Kornhauser and one from Reinhard Bendix on historical sociology.

Q: Lijphart was at Berkeley at that point. Did you take any courses with him?

A: Lijphart was there, but I didn't take any courses with him. In those days, Lijphart was very Eurocentric. The last thing I wanted to study was Europe. Even though I knew several European languages and had a European background, I was thoroughly bored with Europe.

Q: In terms of methodology, what training did you receive at Berkeley?

A: I arrived with zero knowledge of methodology. I had never taken a course on statistics, least of all, research design. I'd had some background in mathematics and had enjoyed it. At one point, early in my undergraduate career, I was even thinking of majoring in physics, but I completely let go all of the "hard" sciences. There was no obligatory course on statistics at Berkeley that I can remember. So, in terms of methods, I didn't have any training when I arrived, and I never took a course in it at Berkeley. But I was reading a lot of American social science, and I knew I would want to use statistics in my research, because you couldn't just interview people, and you certainly couldn't just use secondary sources and documents to tell a story. The notion of "qualitative methods"—for example, of compiling a so-called analytical narrative—never occurred to me. I suppose, in retrospect, you could label me a "mixed method" scholar. Moreover, I considered most of what I read about Latin American politics as "substandard" and saw no reason why it had to remain so (Schmitter 1969). So, I learned statistics by reading how other political scientists, mostly students of American politics, used them.

Q: Did you take any courses on Latin America?

A: I sat in on a course on Latin America, but the person who taught it didn't excite me, so I left. However, in the process of doing research on economic integration in the region, I had come across Raul Prebisch's work, and I got interested in CEPAL and their work.<sup>5</sup> Albert Hirschman was another very important influence. I read his *Journeys Toward Progress* (Hirschman 1963).

5. CEPAL (Comisión Económica para América Latina y el Caribe—Economic Commission

for Latin America and the Caribbean) is the economic think tank established by the UN in 1948 and known for its unorthodox approach to economic development.

Q: Were you reading the classics in social theory, such as Marx and Weber?

A: Absolutely. I read Weber mostly through Reinhard Bendix, who both inspired and frightened me. I suppose he was the most erudite professor in both the political science and sociology faculties that I came in contact with. Behind that mild-mannered and soft-spoken façade, there was this immense fount of knowledge. He, along with Sheldon Wolin, is responsible for my deep-seated conviction that social science has to be historical. This posed a problem since I may have had a better than average knowledge of European history—thanks to my studies in France and Switzerland—but I had absolutely no grasp of the history of Latin America and, most specifically, of Brazil, where I knew I was going to do my doctoral research. Interestingly, it was mainly through Lipset's courses that I engaged



Marx. I had already acquired some knowledge of Marx's writings while studying in Europe and, of course, a superficial version of him was very much a part of the antiwar movement. What Lipset did was convert my existing political interest in class relations into an academic interest in class self-organization. So I got very deeply into Marx. The critical thing, for me, was that I saw Marx and Weber as contemporaries. I didn't think of them as remote historical figures.

Q: Do you have any other recollections about your studies at Berkeley?

A: Yes, and it concerns the most unpleasant part of my graduate career. My biggest problem was political parties, and the problem had a name: Herb McClosky. He was the "parties person" at Berkeley, even though he worked exclusively on the United States. One of the obligatory exams at Berkeley was on political parties. I resisted this, partly for intellectual reasons, because I was rebelling against the centrality accorded to the study of political parties and elections in the discipline of political science. But the core of the problem was that McClosky's course on political parties was essentially a prerequisite for passing the exam. It wasn't a formal requisite, but everybody knew you had to take this course. Anyway, I refused to take it and was the only one taking the exam that year who had not done so. I got an A on my exam from David Apter and a C from McClosky. It was pretty clear that McClosky was punishing me because I didn't take his course. The case had to be adjudicated by a third person and I believe he or she gave me an A-. So, I passed. I'd been lucky until then at Berkeley. All the people I had worked with were sure enough of themselves that they could afford to let students disagree with them. But McClosky wasn't that way. He expected you to regurgitate the material in the way he understood it. I'd had plenty of that in Europe, and I wasn't about to do it again.

Q: Were there any fellow graduate students you were close to?

A: There was a group of students around Haas and also a group working on Latin America. But I didn't have much to do with the latter, probably because Berkeley didn't have a real program on Latin America at that time. Moreover, as I mentioned above, I was "under-impressed" by the quality and methodological sophistication of the studies being done on that area. There was also a group of people working in comparative politics, including Ken Jowitt, Sid Tarrow, and Clement Moore, who was then a young assistant professor. But Ken was working on Romania and communist studies; Sid was working on Southern Italy; and Clem was a Middle Eastern specialist working on Tunisia, Egypt, and so on. Given my interests in Brazil, I was quite alone. I like to point out to my students that not a single member of my dissertation committee knew anything about the country I was studying, and this may have been an advantage since it forced me to think and write in generic terms—in lowercase rather than uppercase terms, to use the expression of Adam Przeworski and Henry Teune (1970, 7 and

26–30).

Q: Turning to your dissertation (Schmitter 1968), how did you decide to work on interest representation in Brazil?

A: I can remember the exact moment when I decided on my dissertation topic. It came during my second year at Berkeley, in Lipset's class on political sociology, where, for some reason, I was picked to present a critique of an article by him (Lipset 1960b). There is a sentence in the article that says something I think 99 percent of political scientists would have agreed with at that time, namely, that political parties are the mechanism for the representation of social interests in democracies. They virtually alone provide the link between citizens and their rulers. Now, this was the period of the maximum influence of systems theory, à la Gabriel Almond, which held that, even if a country had associations and movements articulating interests and passions, what counted was the aggregation of their claims by political parties. Ergo, political scientists had to study political parties and, of course, the elections in which they competed. In my critique of Lipset's article, I said in effect: "You are wrong; political parties are not necessarily the main channel of social representation." This is one of the rare instances where I had learned something from Switzerland. I knew that in Switzerland interest groups were much more important than political parties, which were at best façades for interest groups. I knew this because I had done some research and learned that 75 percent of Swiss deputies were actually paid functionaries of interest groups. Although they ostensibly sat in parliament as members of the Liberal, Radical, or Populist parties, all were, in fact, sitting there as representatives of interest groups. This was due largely to the simple fact that, in Switzerland, deputies did not earn a salary, just a per diem. If you wanted to make a living as a politician, you had to earn your salary elsewhere. So Swiss deputies were lawyers, administrators, or spokesmen for interest groups. I knew this, and I brought it up in Lipset's class. I argued that it is incorrect to say that political parties are the exclusive or even the most important mechanism for the representation of social interests. This should be regarded as a hypothesis, which may fit some cases, but it is not valid as an a priori conclusion.

Then, I remember making the outrageous further assertion that the role of political parties as mechanisms of representation would decrease in the future, because there was a tendency away from reliance on party channels toward more reliance on interest associations and social movements. Frankly, I don't know why I said that. To this day, I can't figure out the empirical basis I was drawing on to make this assertion. Maybe I said this because I was peripherally involved in the movement against the Vietnam War. So, it might have been pure wishful thinking. Or it could have been the result of having spent so much time in Europe, where interest groups were very well organized and entrenched

within the state apparatus. In any case, we had a little bit of a debate in class, and when I went home that evening, I knew I had my dissertation topic.

Q: How did your ideas evolve thereafter?

A: I read American pluralist theory, which I had been exposed to in Lipset's and Kornhauser's seminars. I read Lewis Coser (1956). And, of course, I also read de Tocqueville. I had read de Tocqueville before in Switzerland, where I discovered his famous hypothesis that as societies become more developed they acquire more complex and pluralistic interest group structures. I realized this was a perfect topic to study in Brazil. At that time, Brazil had one of the fastest-growing economies and was clearly experiencing a major, sustained transformation. Brazil also had a very diverse population, ethnically, religiously, and regionally. I didn't know about the concept of a "critical case" then, but later I discovered that is exactly what Brazil was. If there was any country in the Third World where you would expect to see a pluralist interest group system emerging, it should have been Brazil. So, I decided to go to Brazil to study the emergence of organized interests.

Q: How did you get to Brazil?

A: It was not a straightforward matter. At Berkeley, I did not take any courses in Spanish, because I already knew it. And I decided not to take any courses in Portuguese. I figured, why waste my time learning Portuguese when I could learn it when I got to Brazil? This created a problem. The Ford Foundation, which was the main source of funding for doctoral research in Latin America at the time, refused to give me a grant. Nobody considered me a bona fide Latin Americanist because I hadn't passed through the standard hoops. In fact, there was not a single course—graduate or undergraduate—in either Latin American history or politics in my curriculum vita and no visible indication that I knew anything about Brazil. I do not blame them for rejecting me, although at the time, I was devastated. So, there I was in Berkeley, with no money to get to Brazil. But then somebody—I don't know who, but it probably was Haas—said, "Wait a minute, we have money from the Rockefeller Foundation to send young professors abroad for one year. If we name you an assistant professor at Berkeley, we can send you immediately." So they made certain arrangements—I would have to teach in Brazil—that entitled me to go.

I went to Brazil in 1966 ostensibly not to do doctoral research but to teach political science at the Instituto de Ciências Sociais da Universidade do Brasil (Social Sciences Institute, University of Brazil) in Rio de Janeiro. This arrangement turned out to be great for several reasons. It gave me a higher status and a better salary than I would have had if I had just been a graduate student poking around on a fellowship. It also put me in the middle of a contentious and interesting intellectual milieu. I taught the very first course ever in political science as such in Brazil, and I had about eighteen lively students, five or six of whom later became professors of

political science. Finally, teaching in Brazil helped me learn Portuguese fast. Indeed, one week after arriving I had to give a lecture in Portuguese, a language I had never even studied. I would speak in French, Spanish, or whatever came into my head, and the students would correct me in Portuguese.

Q: How did the dissertation research go? You certainly did not find a pluralist pattern of interest representation in Brazil. How did your views shift?

A: I quickly realized my main expectations were not working out. It was clear that Brazil had a state-recognized, state-subsidized, monopolistic, and hierarchical system of interest representation going back at least to 1943. The puzzling thing was that the period of democratic politics from 1946 to 1964 had done nothing to do away with this system; you would have at least expected strong tensions or pressures to have emerged during that period. Even after the military came to power in 1964 it seemed, at least at that moment, they were not doing anything to change the system of interest representation. In short, the system or, as they themselves called it, o sistema, had started earlier, more or less formally in 1943, and was still in effect. It didn't take me long to figure that out and say, "Wait a minute, this doesn't fit."

I started by interviewing trade unionists, and they were quite happy with this arrangement. They did complain a lot about the dictatorial regime; nevertheless, the system of interest representation assured their formal importance and guaranteed them certain financial benefits. That's when I discovered the concept of corporatism when, looking through a used bookstore in Rio de Janeiro, I came across a copy of Mihail Manoilescu's *Le Siècle du Corporatisme* (1934). After that discovery, I started reading everything I could get my hands on about corporatism.

For my dissertation I gathered a great amount of data on associability of various kinds, including membership in labor unions, which I used in my analysis. One of the main parts of the study was to try to use Brazil as a comparative setting. So I took the twenty-six Brazilian states and ran regressions to answer a variety of questions. That was my first exercise working with aggregate data, and I was basically self-taught.

In broad terms, I came to several conclusions. It was clear that interest representation in Brazil was inextricably shaped by the state. Moreover, interest representation was also closely associated with a peculiar dynamic of capitalist development.

Q: You transformed your dissertation into a book, *Interest Conflict and Political Change in Brazil*, which was published in 1971. That same year

you went to do research in Portugal. But before that, in 1969, you had actually done a good deal of research in Argentina, though you never published anything on Argentina. Why did you decide to go to Argentina and why didn't your research there result in any publications?

A: I wanted to see how corporatism in Argentina was different from corporatism in Brazil. I went to Argentina in 1969, and, for a change, I had a nice fat grant from the Social Science Research Council (SSRC) and the American Council of Learned Societies (ACLS). I got time off from the University of Chicago and spent around six months in Argentina. I found it a wonderful place to live, much like being back in Europe. I interviewed more people than I had interviewed in Brazil. I did seventy-five or eighty interviews. Working in Argentina was easier because everything was in Buenos Aires, though I did go to Córdoba, Rosario, and Mendoza. By that time, I had developed the idea that different sectors of capitalists are organized differently. So, I interviewed business leaders in addition to trade union leaders. Interviewing business elites was easier than interviewing trade unionists, because at that time the latter were killing each other. I think four of the trade unionists I interviewed were killed while I was still there. I started to wonder whether someone had my interview schedule and was knocking them off.

I did a lot of work and gathered a huge amount of data. My data on Argentina were actually better in many ways than my data on Brazil. It was easier to work there at that time, despite the dictatorship. But not a thing came out of that research. That's one of my great failures. The fundamental problem was that I didn't understand Argentina. When I finished my research in Brazil, I felt confident about my analysis. I really thought I had Brazil nailed down. But after I finished my work on Argentina, I looked at all these data and said, "I still don't understand this country." That was, for me, a crisis.

Q: What was the reason your research project on Argentina did not work out?

A: By the time I went to Argentina, I was already a reasonably well-practiced Latin Americanist. I knew half of the social scientists in Argentina, and they were very helpful to me. So I can't blame the failure of my project on the resistance of the environment. And, as I just said, I had great data. So I certainly cannot blame it on a lack of data. I had everything going for me; it should have been easy. But I just couldn't use the same methods and the same way of thinking that had worked so well for me in Brazil. To this day, I still don't know what I missed.

One of the reasons my project failed—and this just popped into my head—was the perversity of the Argentine party system. In Brazil, I didn't

pay any attention to parties at all. I could not have cared less about them. I quickly decided that parties were an unimportant part of the picture in Brazil. It was immediately apparent from all my interviews that parties were largely irrelevant for class and sectoral leaders. They worked directly with the bureaucracy or the offices of the president. In Argentina, by contrast, parties were somehow important, yet important in a way that was hard to see. Brazil had no party system, but things still worked. Capitalists weren't threatened by the party system, because it was incapable of articulating the interests of the working class or any other subordinate group. The problem with Argentina, as Torcuato Di Tella (1971–72) argues, is that it's a conservative nation without a conservative party. There was no business-oriented, liberal, or, for that matter, traditional conservative party. Hence, Argentine capitalists were threatened by the party system. That's why it is harder to understand interest group politics in Argentina, because it is shaped by something that is not there, namely, a conservative party. It's like a dog that's not barking.

I think my problem with Argentina—and this is the first time I've ever thought about this in these terms—may have been that I didn't understand the party system, and I couldn't understand the interest group system without first understanding the party system. So, there is a pretty important lesson here. When you are a rebel against the orthodoxy, you may find yourself in trouble because there may be more truth in that orthodoxy than you're willing to concede. In my case, I was so fixed on putting an argument together that focused on the direct relationship between interest groups and the state apparatus, that I missed the importance of parties.

Q: After this experience in Argentina, you focused next on the Portuguese case. How did Portugal affect your thinking?

A: The failure to understand Argentina led me to go to Portugal. I saw it as a living remnant of corporatism, as an item of "political archeology." I wanted to go there because it was my only chance to look back into the 1930s. And I understood Portugal perfectly well. When I went to Portugal, the first thing I learned was that the Portuguese had a so-called dominant party that didn't really exist. It was a totally fraudulent organization. It sent people to parliament, and that was all. The party was totally unimportant in terms of policy making. The nice thing about Portugal also was that it was an easy place to do research. There were very few Portuguese social scientists. I met them all, and I could have done it in an afternoon. In the early 1970s, Portugal was a country without sociology, let alone political science. I had talked to the few Portuguese living in exile, in Geneva or Paris, who had some knowledge, albeit not very direct, about the country. But, precisely because nobody was doing any social science research, I faced few obstacles gaining access to information. The funny thing about Portugal was that people were enormously helpful, because they were flattered that someone who

had worked on a really important country like Brazil wanted to work on a country like Portugal. This allowed me to gain access to mountains of data and documents that no one had looked at. I also did some interviews with officials in the extensive formal system of *gremios*, *casas do povo*, *sindicatos*, and *confederacoes*. I wanted to know what their activities were, how they gained access to public officials and, of course, what their sources of finance were. In very little time, I understood exactly what was going on in Portugal. And the most important thing going on was what was not going on. I concluded that the Portuguese corporatist system was extremely important for understanding the perpetuation of authoritarian rule in Portugal, not for what it accomplished, but for what it prevented from happening. The corporatist institutions occupied this space of representation, which made it easy to deny any others entry into that space. I wrote several articles on Portugal based on this research (Schmitter 1975, 1978, 1979a, 1980). Portugal also put me back in Europe.

If it hadn't been for Portugal, I probably wouldn't have returned to Europe. Well, I would have been going back to Europe for family reasons, because I married a German woman. Also, my mother's family was in Southern France. So I would have been going back and forth, but not for academic reasons. Indeed, while doing my research on Portugal I realized I had an important theme for future research on Europe. I was reading a book by Sedes Nunes, virtually the only sociologist in Portugal then tolerated by the government. He was an apologist, but no fool. He made the interesting point that while it was true that Portugal had a corporatist system with its origins firmly rooted in an authoritarian regime, though he did not use this expression, this was also true of other European countries, including some of the very ones that most criticized "fascist" Portugal. He was essentially saying that Portugal's corporatist system was a feature of modern societies. It's what the Swedes are doing; it's what the Finns and Norwegians are doing. I read that and thought, "That's kind of funny. He's absolutely right when you look at the organizational structure of interest representation in Sweden." I didn't know at the time that the main difference was that the Swedish system worked and the Portuguese system didn't. That insight came later.

So, I had this idea in the back of my mind, and one day, when I was a visiting professor at the University of Geneva, I was reading the *Tribune de Genève* and it described the workings of the interest system in Switzerland, specifically the arrangements for determining the price and quantity of milk. I thought, "This is really interesting." I then remembered Manoïlesco's (1934) distinction between state and societal corporatism. I'd forgotten that, because every country I had studied—Brazil, Argentina, and Portugal—had been characterized exclusively by state corporatism. I didn't have any research materials with me, so, when I remembered Manoïlesco, I ran across the street to the *Bibliothèque Publique et Universitaire*, where I found a whole section of cards on "corporatisme, Suisse" and, there, an unpublished dissertation on corporatism in Switzerland during the 1930s. I don't remember the author's name, but this dissertation described Swiss

corporatism and drew a contrast that was strikingly similar to the one Manoïlesco had developed in the 1930s to distinguish the Mussolini version of state corporatism from the societal corporatism of other countries. The idea that the concept of corporatism could be applied to contemporary Western European cases was a core insight I developed in my 1974 article, “Still the Century of Corporatism?” (Schmitter 1974), and it helped launch my research on societal corporatism. This insight also helped me develop corporatism as an alternative model to pluralism. Based on my research in Brazil, I saw state corporatism as an alternative model to pluralism in developing societies. I thought that capitalist development under different conditions produced different forms of class conflict, which, in turn, produced different configurations of organized interests. In making this argument, I drew on works by Alexander Gershenkron (1962), Albert Hirschman (1963), Guillermo O’Donnell (1973), CEPAL (Prebisch 1963), as well as Manoïlesco (1934). So, I was aware that corporatism gave me an alternative model for the Third World, that is, for countries that were “late dependent developers,” such as Romania in the 1920s, Brazil in the 1950s and 1960s, and, for that matter, Argentina and Mexico. This framework also seemed to work well enough in Portugal. Still, I had not realized that corporatism, in its societal variant, could also provide a model for advanced industrial European countries. Thus, the key shift was to move from peripheral and historical cases, where I found corporatism in its statist variety, to consider the applicability of corporatism in its societal form to contemporary Western European cases.

Q: Your explanation of corporatism in Latin America differed from that of other authors who provided a cultural explanation.

A: After I did my research on Brazil, I discovered there existed a literature on corporatism in Latin America that offered a cultural explanation. It said that corporatism was somehow produced by or, at least, consistent with Iberian culture. This argument was proposed by political scientists like Howard Wiarda (1974) and historians like Ronald Newton (1974). I just couldn’t believe it. I didn’t understand how anybody could possibly study corporatism without recognizing it was a state-imposed arrangement. I also disagreed with the notion of an overarching “Iberian” political culture. I had had enough experience living in Mexico before I went to Brazil that I could immediately recognize that, in cultural terms, Brazil was not Mexico. The day-to-day life experience was different, and conceptions about politics were very different. To say that these countries had the same political culture was ludicrous. It was clear to me that corporatism in Brazil had absolutely nothing to do with Brazilian political culture.

Q: Your article “Still the Century of Corporatism?” (Schmitter 1974) had a large impact. Why do you think this was so?



A: The article was published in an obscure journal. So I didn't think it was going to have any impact whatsoever. But I can tell you both the cause and the intermediate, triggering mechanism of its impact. The real cause was that the study of interest groups had been monopolized by the pluralist paradigm, and this American perspective was being transferred from the United States to Europe. The work of Joseph LaPalombara (1964) is a good example. He studied Italian institutions as if they were pluralistic. Even though he does a good descriptive job, there was something fundamentally wrong with his approach. There were many single country monographs on interest politics in Europe which, lacking an alternative conceptual model, described things as "imperfect" pluralism. Hence, I think the corporatism framework filled a yawning analytical gap. The triggering mechanism that explains the success of my 1974 article was that I was not alone. Other people in European countries were starting to think about alternatives to pluralism, even if they often didn't call it corporatism. Gerhard Lehmbruch, who had studied Switzerland and Austria and was beginning to talk about "liberal" corporatism, was especially important in this regard. The combination of offering an alternative model with the fact that others were beginning to work along similar lines helps explain the impact of my article.

A final factor that explains why my work on corporatism was picked up is that, for once in my life, I was entrepreneurial. Lehmbruch and I discovered each other, and we started organizing successive meetings over several years at the ECPR (European Consortium for Political Research). That made a big difference, because many young people were interested in the topic, and the corporatist model helped people organize their thinking in a way that moved beyond the notion of degrees of pluralism.

Q: Starting most visibly with the two volumes you co-edited with Gerhard Lehmbruch (Schmitter and Lehmbruch 1979; Lehmbruch and Schmitter 1982), you published a large number of articles on corporatism in Europe. What were the main issues you sought to address, and what are the main conclusions you drew from this research?

A: This research had several strands. One focused on the question, what difference does corporatism make? I'd been busy simply trying to understand how interest groups were organized, how they emerged, and how they interacted with state agencies. But I hadn't really thought about their impact. Lehmbruch was interested in this issue, and we started focusing on the impact of corporatism on political outcomes, such as governability (Schmitter 1981), and on macroeconomic outcomes, such as fiscal deficits, inflation, employment, and wages. One of the most salient challenges to corporatism came from Marxists, who saw it as some sort of capitalist trick. So, I also addressed the consequences of corporatism at the level of classes and sought to see who benefited. That led me to the question of whether the state had a class bias (Schmitter 1985). Marxists thought the state intervened to fashion corporatism because corporatism subordinates the working class and keeps it from achieving its true revolutionary goals. I reached exactly the opposite con-

clusion: the problem with corporatism is that it benefits the working class, and, therefore, is vulnerable to defection by capitalists. The big challenge is for the state to keep capitalists in the game. This research also pointed to other important conclusions. One thing that kept capitalists in the game was the presence of a Social Democratic Party in power. As a result of my longstanding blindness toward political parties, I didn't pay much attention to the interweaving between parties and interest associations. But Lehmbruch always insisted on this point. I learned from him to think not only in terms of the state/interest group nexus but also in terms of the state/party nexus. What emerged was a more triangular way of thinking that incorporated the three-way relationship among the state, interest groups, and parties. Beyond this, I would point to two other issues addressed in my research on corporatism. The most important conclusion regarding the viability of corporatism was: be careful about capitalists. As soon as I realized that, I said, "No one is thinking about capitalists." This insight led to an independent project on the organization of business interests. With Wolfgang Streeck, I sought to understand how capitalists organize and why they organize differently in different countries and sectors, that is, why capitalists in some countries have very strong peak organizations, why they bargain at different levels across sectors, and so on. That was a whole new line of work that came out of my research on corporatism (Streeck and Schmitter 1985; Lanza-Laco and Schmitter 1989; Schmitter 1990; Hollingsworth, Schmitter, and Streeck 1994). One other, much less developed, line of research concerned the relationship between corporatism and democratic theory (Schmitter 1983). The problem, as I saw it, was that definitions of democracy assumed a pluralist component. I started thinking about this question but didn't get very far, because I was getting increasingly involved in research on democratization.

So I tried to drop corporatism. I wrote a last article, "Corporatism is Dead! Long Live Corporatism!" where I said that corporatism may be momentarily declining in importance, but it will come back, and I explained why I thought so (Schmitter 1989). I dropped corporatism with that article and started working on democratization almost full-time.

Q: How did your interest in democratization evolve?

A: With the Portuguese revolution in 1974 and then with the beginning of the political transformation in Spain after the death of Franco in 1975, I started to invest more in learning about the regime-level changes in these two countries. I also began an ongoing interaction with Juan Linz in 1974, 1975, and even 1976. I don't know how important our interaction was to him, but it was very important to me. I had known Linz for a long time already. In fact, I'd met him in the 1960s when he came to a conference in Rio while I was a graduate student working on Brazil. I remember sitting for two hours with him in a little restaurant—I could still find it now just off the Copacabana Beach—talking about his concept of an "authoritarian regime" (Linz 1964). That meeting established our relationship. We were interested in very similar things, and with the events in Portugal and Spain in the 1970s, we suddenly found ourselves together on the conference circuit.

It's an experience sitting down with Linz. He has encyclopedic knowledge, and because I'd known him for a long time, we both had a lot of confidence in each other. So we spent a lot of time going back and forth with ideas, trying to figure out the different processes occurring in Portugal and Spain. Grappling with these two cases sensitized me to the idea of modes of transition, of distinct paths to democracy. Still, I was very convinced, right from the start—I never wavered on this point—that, despite their different modes of transition, Portugal and Spain would end up in the same place. I was much more convinced than Linz that Spain would become a perfectly normal, even rather boring, country. I was also convinced that Portugal, too, was going to make it and become an even more boring polity in ten years. So, even though the process of getting there was different—Spain was obviously having a much more controlled, pacted transition than Portugal—I was convinced they were both going to end up becoming routine European democracies.

Q: You started a project with Guillermo O'Donnell that involved a series of conferences at the Woodrow Wilson Center in Washington during 1979–81. These conferences later resulted in the publication in 1986 of an influential collaborative volume on transitions from authoritarianism (O'Donnell, Schmitter, and Whitehead 1986). How did your collaboration with O'Donnell alter the way you were studying democratization?<sup>6</sup>

A: Until the opportunity to work with O'Donnell arose, I was moving exclusively within a circle of Southern European scholars. Above all, the experience of repeated interactions with Linz, trying to explain to each other what was happening in Spain and Portugal, was critical to shaping my thinking. Indeed, I see these interactions as an important prelude to the Woodrow Wilson Center project I did with O'Donnell. Then O'Donnell and I met at a conference at Yale. We had become friends before and had

6. See the interview with O'Donnell in Chapter 9 for his perspective on the project on regime transitions.

seen each other fairly often. We discussed the idea of bringing together the Southern Europeans and Latin Americans. Now, at that point in time, you had the Portuguese, Spanish, and Greek cases of democratization. But things weren't changing much in Latin America. One famous effort at political reform had been made in Brazil in 1974, with President Ernesto Geisel's proposal for a policy of *distensão* (distension). But none of us took it seriously.

At least when we started our project, we had no particular reason to be optimistic about democratization in Latin America. My collaboration with O'Donnell made me think about studying democratization through much broader comparisons. I was the Southern European part, and he was the Latin American part. Still, this forced me to learn more about Spain and also Greece, about which I knew a lot less. Also, I was interested in both the European and Latin American sides. Working with the large, interregional working group that O'Donnell and I put together was a real experience. I

was the only person at these meetings who initially knew everybody in the room. I knew the Southern

Europeans, and I had also worked with almost all the Latin Americans.

O'Donnell obviously knew all the Latin Americans but only one or two of the Southern Europeans. Few of the rest of the group knew each other. Still, we were able to develop a capacity to understand each other, even when someone was using a different language. For one, we had all read each other's work. Moreover, from the very start, we all shared the view that we had a very big problem before us, because according to the orthodoxy of that time, the countries we were interested in should not be successful at democratizing. Actually, our assumption was that, in light of existing theories about the prerequisites of democracy and the big problems these Southern European and Latin American countries faced, most were going to fail, and, at best, one in three would succeed in becoming democracies. This is where Albert Hirschman's (1992) concept of possibilism came in. We agreed to think possibilistically, not probabilistically, about what could work to achieve democracy in the countries we were studying.

Q: Could you elaborate on how you understood the literature on democratization at the time and the innovations you sought to introduce?

A: We were facing two literatures. One, to which I just alluded, emphasized the social structural and cultural "prerequisites" countries needed to fulfill before becoming democratic (Lipset 1959; Moore 1966). We responded to this literature by emphasizing possibilism. The second body of literature stressed the implications of the model of democratization fashioned in Britain, the Low Countries, and Scandinavia (Rustow 1970). One implication of this model was that democratization was a long, slow process based on the gradual enfranchisement of citizens and the progressive widening of political liberties. A second implication was that, in order for democratization to occur, the previously dominant group in a country had to tolerate, for an extensive period of time, the mobilization of excluded groups. A third implication, which I was not aware of at the time, concerned the international context. A common argument in this literature, for example, is that the process of democratization in Britain was shaped by what had occurred before in France, especially the French Revolution. In this regard, this literature emphasized the antirevolutionary, reformist aspect of democratization, drawing attention to the recomposition of the old ruling elites alongside the mobilization of excluded groups from below.

We responded to this second body of literature by trying to find an alternative model, or set of models, for regime change that were neither reformist nor revolutionary, and which could work even if a country lacked a national bourgeoisie and an organized working class. Terry Karl's (1986) work was very important in this regard, because she had been working on Venezuela and had already developed an alternative model of pacted transitions.

Her model fit nicely with the democratization experience the Spanish were going through at the time.

Q: Your own efforts to develop a model of democratization strongly reflect the influence of Machiavelli (Schmitter 1979b).

A: It suddenly occurred to me that Machiavelli was the theorist of regime change. I don't know why this came to me—maybe because I was teaching at the European University Institute and living in Florence at the time. I plunged into Machiavelli and read everything by him I could find, including his letters. And I came up with a pure Machiavellian interpretation of regime transitions. Quite self-consciously, Machiavelli did not consider himself a theorist of normal politics. He calls transitions “female times,” that is, extraordinary contexts in which you can't trust anyone and when there are no agreed-upon rules. To study such moments, he said, you need a new political science. What I got from Machiavelli was that a distinctive set of assumptions about politics is required to study transitions.

Q: Your working assumption circa 1980 that only one in three of the cases you and your collaborators in the regime transitions project were studying would become democracies turned out to have been overly pessimistic.

A: Democratizing turned out to be much easier than I thought. At the time, I certainly expected most of these countries were not going to make it, and I was clearly wrong in thinking only one in three would have a successful transition to democracy. We all misunderstood the change in context that

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made democratization a much more likely outcome of attempted regime change. We have since learned that democratizing is easier, yet also much less consequential in socioeconomic terms, than we used to think. Today, the non-elite groups that historically struggled for democracy make compromises where they accept a great deal less than they would have in the past, perhaps because of previous failures and a process of collective learning. As a result, inequitable systems of property rights survive many transitions to democracy without a scratch. And in some cases, for example, in Eastern Europe, income inequality has even gotten worse—deliberately worse—after democratization. So, democratizing is easier today precisely because it's less consequential. This is not terribly heartening. It's not what the people who struggled for democracy had expected. They compromised and accepted maybe even their third best alternative, because they had learned that going for the first best option by pushing immediately for socioeconomic redistribution can bring disaster.

Q: Adam Przeworski was brought into the transitions project and wrote a chapter for *Transitions from Authoritarian Rule* (Przeworski 1986). What do you see as his contribution to the analysis of transitions?

A: Przeworski made fundamental contributions. One of the things he is most responsible for is the argument that legitimacy is neither the problem nor the solution of democratization. Many people were arguing that the reason authoritarian regimes collapsed was their lack of legitimacy. Later, people argued that a democracy requires legitimacy to become consolidated. Well, that argument is completely circular. One implication of his argument was that attitudinal variables, legitimacy-oriented ones, do not tell us much about the consolidation of democracy. Przeworski was very clear in his critique of these assumptions. O'Donnell and I were probably thinking along these lines already, but Przeworski has a nice way of capturing these insights and getting them into apodictic, declarative form. That was a very important contribution. The other thing Adam did was emphasize the fundamental role of uncertainty in democratic regimes, a point he continued to develop afterward and summarized nicely in the phrase that “democracy is a system in which parties lose elections” (Przeworski 1991, 10). My view of the idea of “contingent consent” is something quite similar (O'Donnell and Schmitter 1986, 59–61). Essentially, you consent contingently to let your opponents rule, because you think the rules are fair and that you will get the chance under those same rules to come to power in the future. But I also see

7. See the interview with Przeworski in Chapter 13 for his view of the project on transitions from authoritarian rule.

this uncertainty as more bounded than does Przeworski. The way to guarantee that uncertainty is tolerated is by making sure it's not very consequential, that is, by putting limits around it that restrict the range of choices facing politicians to a very narrow set of options.

One of the nice things about working with scholars like Przeworski and O'Donnell is that in many cases I cannot honestly tell you who is responsible for which ideas. This was especially the case with O'Donnell. There are a few parts of our book, *Transitions from Authoritarian Rule* (O'Donnell and Schmitter 1986), which I know he wrote and which I know I wrote. But I couldn't tell you who was really responsible for 80 percent of the book. We produced it through a process of going back and forth and arguing with each other. That kind of genuine collaborative work, that yields a final product in which you can't identify specifically where the ideas came from, is great. Terry Karl and I achieve that when we write together.<sup>8</sup> We don't know afterward who had an idea first. We simply know that the idea was generated by both of us. With Przeworski, the interaction has been much less intense, but I also feel sometimes that I do not know where a certain idea originated. He and I have different ways of expressing things, and we use different concepts, but many of our ideas are the same.

Q: But you and Przeworski also have different views on many issues.

A: Yes. Still, we both characterize transitions as periods of uncertainty, and we both emphasize the choices of the actors involved in transitions. Przeworski and I differ in that he uses the concepts and accepts the prior assumptions of rational choice theory, whereas I don't. I believe that this theory is inappropriate for analyzing transition situations, because certain of its assumptions, especially the proposition that actors fully understand the consequences of their actions and those of their opponents, simply do not hold in such instances.

I also differ from Przeworski on another matter: whether the mode of regime transition has a lasting effect on the outcome of transitions.<sup>9</sup> He thinks the mode of transition does not have a lasting effect, whereas I think it does. Let's take a good example, Spain and Portugal. These countries had very different modes of transition. Both are now stable, Western, liberal democracies, but they are nevertheless different types of democracy, especially in terms of institutionalized power relations. For example, in Portugal 35 to 40 percent of the workers, a relatively large share of the working force, are members of the trade union movement, whereas in Spain the corresponding figure is only 5 to 8 percent. Remarkably, the situation was

8. Schmitter's collaborative work with Terry Karl includes Karl and Schmitter (1991) and Schmitter and Karl (1991, 1994).

9. Compare Przeworski (1991, 95–99) and Karl and Schmitter (1991, 1992).

exactly the opposite before the transition, when the Spanish trade unions were larger proportionately than the Portuguese. There are some serious continuing differences across the two cases that, I believe, can be explained by the contrasting mode of transition.

Q: Did you expect *Transitions from Authoritarian Rule*, and, in particular, your volume with O'Donnell (O'Donnell and Schmitter 1986), would have a huge impact?

A: No. The project was cross-regional, which was rather novel, and I guess we assumed it would have an impact in research on the two regions on which the project focused, Southern Europe and Latin America. But we didn't have the faintest idea that studying regime transitions would become the growth industry of political science in the subsequent decades. There is not a word in our book that predicted that transitions from authoritarian regimes would happen elsewhere. We had no reason even to believe that large parts of Central America and Latin America would democratize. Moreover, the collapse of authoritarian regimes in Eastern Europe came as a total surprise to me, as it was to the people in that region. I and, I suspect, Guillermo, too, never imagined the sheer scope and extent of the emerging

wave of democratization. Now, I do not think the fact that all these other democratizations occurred is the only thing that explains the book's success. I also think we put something back into political science that Machiavelli knew a lot about, but had been lost. The "Little Green Book" sought to capture something about a crucial political moment as seen from the point of view of politicians, not from the point of view of academics.<sup>10</sup> O'Donnell and I tried to

think like politicians and put ourselves in their shoes. This allowed us to reach a wide audience beyond academia. I have heard that many politicians have read the book and recognized in it the situations and choices they were making. This made the book part of the political process, not just something external to it. Our book may even have helped some countries get through their transitions. I was told that the book became a sort of bible to the South Africans and made a direct contribution to their process of regime change. I was told the same thing in Hungary, where the elite on both sides seem to have read it. And Nelson Mandela told somebody to tell somebody to tell me that he had read the book when he was in prison, and it inspired him to hope a transition could happen in South Africa. That's when I thought, "Yeah, I guess we did a good job." Still, whether the book makes a lasting contribution to political science

10. The "Little Green Book" refers to O'Donnell and Schmitter's *Transitions from Authoritarian Rule* (1986), which had a green jacket.

will depend a great deal on whether these moments of regime change make a significant difference in the long run. If, as Adam Przeworski suggests, the mode of transition does not have a lasting effect, and structural/cultural variables such as the level and rate of development or the prevailing religion are determinant instead, then our ideas about transitions will not prove that important.

Q: In the wake of your 1986 book with O'Donnell, you have explored whether the model, or parts of the model, presented in that book can be extended to new cases and regions beyond Southern Europe and Latin America.

A: I have been trying to see how much I can extend those arguments to determine what sticks and what doesn't, what travels and what doesn't. I have done this in several articles with Terry Karl (Karl and Schmitter 1991; Schmitter and Karl 1991, 1994). I started this exercise as I came in touch with people from all over the world who were becoming interested and involved in the process of democratization. There is a lot of interest in these kinds of broad comparisons. But there is resistance, too, of course. The idea of including the central European countries and the former republics of the Soviet Union in the same universe is difficult for some people to swallow. Resistance comes especially from those who believe these cases differ in terms of their historical point of departure or have radical cultural differences. For example, most of the literature on the Middle East begins



with the presumption that Islam is so different that the Middle East cannot be fruitfully compared with other regions. I don't believe this (Schmitter and Hutchinson 1999). More important, my approach is to include these cases in my overall sample, so I can then partition my data and see whether relationships vary from region to region. The important thing is to start with a large N if you can get good data, which is not easy to come by. Generating these data is very time-consuming and expensive.

Q: Is this work part of a book-length project on democratization?

A: Yes, but so far, all I have is bits and pieces. I've been trying to finish this project for a very long time and get it off my desk. But I keep coming up with different things I feel I should include. For example, someone once mentioned to me, "You can't possibly write a book on this topic without including women." Then, by coincidence, somebody invited me to a conference on women and democratization. So I wrote a paper and that became another chapter. Then, of course, there is the international dimension, which I never paid enough attention to earlier on. I do not think O'Donnell and I were mistaken when we said that the processes of democratization we were analyzing were fundamentally driven by domestic, not international, forces. The situation is different now. Today, when a country democratizes, it gets invaded, not just by NGOs, but also by the European Union, the United States Agency for International Development (US AID), and all these different democracy promotion programs. They commit substantial resources, and they meddle in the internal politics of democratizing countries in ways that were unthinkable in the late 1970s and early 1980s, when we were studying the Southern European and Latin American cases. So now I am working on the international dimension of democratization (Schmitter and Brouwer 2000; Offe and Schmitter 1998).

Q: This emphasis on the international dimension of democratization dovetails nicely with your research on the European Union (EU).

A: The driving edge of my research now concerns issues of democratization in the context of the EU, a question about which I have recently written a book (Schmitter 2000a).<sup>11</sup> This continues my personal effort to promote democratization wherever it rears its benevolent head. I am treating the EU as a transitional polity, a polity in formation, which has not reached an institutional equilibrium. The question is whether democracy can be inserted into this process of producing an entity composed of previously sovereign states. That's absolutely the top political issue in Europe today. I may not think the EU should constitutionalize itself, but I do think the transitional mechanisms that should be applied in the EU are exactly the opposite of the ones that are desirable in national-level transitions. I am firmly convinced that the earlier a country constitutionalizes its politics, through a Spanish-style process of genuine consensus building and popular ratification, the better. But I argue just the opposite at the EU level. The EU should not constitutionalize itself right away; rather, it should do so gradually and only after public opinion has manifested its support for such a process. I think the EU should take fifteen

years to get itself a constitution and become a democracy.<sup>12</sup> Only after Europe has actually felt the impact of a large-scale transfer of authority to supranational institutions and only after controversies about this have created a distinctively “European” public sphere should the EU make the effort to define its *finalité politique*.

Q: To summarize, what do you see as your main contributions over the past twenty-five years to the study of democratization?

A: I am not sure I can answer this question, certainly not objectively. Moreover, I have been repeatedly surprised by how other scholars have re-

11. Schmitter’s writings on the European Union also include Schmitter (1997a, 2000b, 2003).

12. Subsequent to this interview, the proposed EU constitution was rejected by French and Dutch voters in mid-2005.

sponded to my work on this subject. Some of this must be due to my convoluted style of writing, my uncontrollable inclusion of foreign words and even invention of new ones, and my insistence on stretching comparisons across rather than confining them within regions. Hence, what I see as my major contributions probably does not track with what others would judge them to be, positively or negatively. This is what I hope to have accomplished:

1. To have helped convert the study of democracy from a static to a dynamic enterprise.
2. To have undermined the myth that democracy is a luxury that only rich countries with an Anglo-Saxon “civic” political culture can afford.
3. To have tried to convince scholars that democracy is not always produced by democratic means and that it is possible for non-democrats to make a positive contribution to democratization.
4. In a similar line, to have pointed out that elites acting “from above” can be just as important, if not more important, than citizens acting “from below” to the success of transitions from autocracy.
5. To have shifted the discipline of political science from its obsession with political parties, territorial representation, and competitive elections toward increased attention to interest associations, functional representation, and policy making by pressure and/or concertation.
6. To have encouraged political scientists to think in interregional, rather than intraregional, terms and to consider democratization as a generic process, despite its obvious national and subnational peculiarities.

7. Finally, following Machiavelli, to have defended the notion that there can be no one science of politics or even of democratic politics, but that its core assumptions, concepts, hypotheses, and methods must vary according to the context in which politics is practiced. At a minimum, this means one science for orderly situations in which actors know and more or less accept the rules of the game, and another for those in which the actors do not even know who they and their allies are, even less what the rules are. Machiavelli called these “male” and “female” times. Political science still pretends that this is what separates its sub-disciplines of comparative politics and international relations.

Q: Your research has had three major strands: one focused on corporatism, a second on regimes and democratization, and a third on EU integration. How are these research strands related? Do you connect them in your mind and have a way of synthesizing them?

A: I’ve been asked this question a few times before. I once met someone who thought I was three different “Schmitters,” because she didn’t understand how I could make these topics fit together. I do see a common thread to my research: interests, politics understood as the pursuit of interests, and the emergence and resolution of conflicts related to the pursuit of interests, at multiple levels of aggregation. In this sense, I’m an old-fashioned social scientist who does not follow recent trends. I really believe you have to study capital and labor, and I see class, sectoral, and professional associations as the core of civil society. So, for example, when students tell me they want to study social movements and “the grass roots”—and I have had many such students—I say, first study capitalists, because they are the anchors of civil society, then study workers. Maybe I’m wrong, but I have never been a social movements person. I don’t care about bowling clubs and even less about “bowling alone.”<sup>13</sup> I’ve never given much credence to the idea of movement-based democracy. I always regarded that notion as an illusion. I can imagine having a corporatistbased democracy, with very little significant political party activity. But the core of democracy is missing unless the bargaining relationship between capital and labor is nailed down and somehow institutionalized.

Q: What do you think are your best ideas?

A: In a sense, I don’t have any new ideas. My best ideas are ones I get from somebody else, often by disinterring them from an obscure source. I really mean that. Everything there is to say about politics has already been said, somewhere. I can’t think of anything I have said that I would call genuinely original, though maybe I am using too high a standard. I may have brought things together in an original way or introduced preexisting ideas into a different context. Your question reminds me of my frustrated career as a painter. I didn’t do anything original with painting, but there, at least, I knew what originality was, and I wanted to produce something that nobody had done before. I failed. In the social sciences, by contrast, it’s very

hard to do anything really original. You can combine old things in new ways or take something and put it in a different place. You can say things in a way people haven't heard before. But real originality . . . there isn't much.

Q: Still, your work on corporatism did alter the way interest groups were studied by providing an alternative to the dominant pluralist paradigm. Your contribution in this area could be seen as part of the influential literature that "brought the state back in" (Evans, Rueschemeyer, and Skocpol 1985).

13. The allusion here is to Putnam's *Bowling Alone* (2000).

A: Let me give a two-part answer to this question. First, the literature on corporatism critiqued pluralism and did introduce some new elements into the discussion. Gabriel Almond, in a piece in *World Politics*, stated that many of the ideas in the literature on corporatism were already developed in the work of pluralists (Almond 1983). I disagree. Maybe it's my own failing, but when I was working on my dissertation, I read the pluralists and did not come across the ideas that were later associated with a corporatist approach to interest groups. For example, Harry Eckstein went to Norway, one of the most corporatist nations in Europe, and yet did not find corporatism (Eckstein 1966). Instead, he came back from Norway with his congruence theory, a cultural theory positing that stable democracy is based on the congruence between cultural norms and authority patterns. Eckstein completely overlooked the fact that democracy in Norway was based on a well-organized system of capital-labor relations that had operated since the 1930s. Eckstein went to Norway to study interest groups, yet he failed even to mention this basic fact! This omission is quite incredible. So, yes, the pluralists did have an interest in interest groups, but they did not conceptualize interest groups in a corporatist way at all.

Second, I find exaggerated the claims of works such as Theda Skocpol et al.'s *Bringing the State Back In* (Evans, Rueschemeyer, and Skocpol 1985; Skocpol 1985a). You could say I was a member of that movement without knowing it. Obviously, the state was part of what I was addressing when I talked about corporatism as opposed to pluralism. Even societal corporatism has an extremely important state component. Those institutional configurations were not possible without the coercive power of the state. But I remember my first reaction when I saw the title, "bringing the state back in." I thought, "What do you mean? The state has been there all along. Who needs to bring it back in?" Only an American could write something like that. Anybody working on Europe or Latin America, almost without exception, didn't need to be told to bring the state back in.

Q: Do you have regrets about anything you wrote or did not write?

A: Let me mention two regrets, both related to my work on corporatism.

One concern is the term corporatism itself. I once met Norberto Bobbio, the dean of Italian political thought, and a marvelous man. He was eighty-five years old at the time, and he came up to me, grabbed me by the lapels of my jacket, and said, “Ah, you’re Schmitter. I find your work very interesting. But why did you have to call it corporatism?” Bobbio had lived through and struggled against an earlier version of corporatism during Mussolini’s time. To apply the term corporatism to contemporary, democratic polities must have seemed to him to be stretching the concept beyond recognition. He had a point. One has to be careful about choosing one’s terms, and it would have been much better if I had been more imaginative in coming up with a new label instead of borrowing a normatively loaded, previously existing term. The use of the suffix neo- in front of corporatism, a practice adopted by several people, probably helped, though I didn’t think of that. Instead, I used the terms state corporatism and societal corporatism. The other side of this issue is that the use of the term corporatism was a great merchandising tool, because it annoyed people. The term did not have this effect in Brazil or Argentina, because a literature existed in those countries that talked about it. But, when the term corporatism was applied to developed European countries, it became provocative. That effect was positive, because it forced people to pay attention to aspects of interest group systems in Europe that simply could not be dismissed.

My second regret concerns how I conceptualized corporatism. I included a very large number of variables in my definition.<sup>14</sup> That seems excessive now, and I think it was a mistake. It probably would have been better if I had not been so elaborate, if I had focused instead on a smaller number of dimensions, say, four or five, rather than the nineteen I have been told are there. This would have been an improvement from an operational standpoint. At the time, I thought I could use my elaborate definition in a narrative, descriptive, “ideal-typical” way and that it would be self-evident whether, say, Austria, Finland, or the Netherlands was corporatist. But, later on, when I started to study corporatism quantitatively, I realized my definition was difficult to operationalize.

Q: After working on corporatism in Brazil, you shifted to Europe, but you never carried out an explicitly cross-regional comparison focused on corporatism. Indeed, the literatures on corporatism in Europe and Latin America are somewhat divorced. Did you ever attempt to launch a cross-regional project on corporatism?

A: No, and I have a twofold explanation for this. First, in 1974, the Portuguese revolution happened, and my interests shifted from corporatism to democratization. Second, in Europe, I worked with Gerhard Lehmbruch and all sorts of other people in the European Consortium of Political Research (ECPR). Though the ECPR was very effective both at organizing workshops to discuss different aspects of corporatism and at spurring the writing of monographs on different countries, we did not have the money

14. Schmitter's (1974, 93–94) definition is as follows: "Corporatism can be defined as a system of interest representation in which the constituent units are organized into a limited number of singular, compulsory, noncompetitive, hierarchically ordered and functionally differentiated categories, recognized or licensed (if not created) by the state and granted a deliberate representational monopoly within their respective categories in exchange for observing certain controls on their selection of leaders and articulation of demands and supports."

to organize something like the Woodrow Wilson project, which brought together the Southern Europeans and Latin Americans to address regime transitions. Remember, initially corporatism was not a fashionable topic. Besides, our ECPR group did not have an entrepreneur like Abe Lowenthal, who got the money for the Woodrow Wilson project. I must confess that I am very lazy when it comes to fund-raising. I don't like to spend my time writing proposals and administering projects. So, I didn't even try to put together an interregional working group on corporatism. Still, the main reason I never tried to launch a cross-regional project on corporatism was that I got completely swept up in the Portuguese revolution, and then the Woodrow Wilson project on transitions enticed me into focusing on a topic that I thought was at least as important as corporatism, maybe more.

Q: Do you think any of your ideas have been misinterpreted?

A: One constantly experiences frustration with the way others use one's writings. But the only matter I truly worry about is a misinterpretation of my work with O'Donnell on transitions (O'Donnell and Schmitter 1986). Some people mistakenly think we were proposing a permanent shift to an actor-centered, strategic perspective as a new vision of politics for all times. What we said, in fact, is that an emphasis on actors' strategies is justified during a regime transition, a particular and very important moment of time. This emphasis does not apply to normal politics. Indeed, under these circumstances, I generally take a rather determinist historical-institutional view and don't pay much attention to the strategies followed by individual actors.

One other thing that annoys me is many people said O'Donnell and I were writing about transitions to democracy, when the title of our book explicitly states that we were studying transitions from authoritarian rule. This seems to be a persistent misunderstanding. Yet we very consciously sought to avoid a teleological view. Indeed, this was one of the first things on which O'Donnell and I agreed. We genuinely did not know whether the authoritarian regimes we were studying would end up as democracies, or, if they did become democracies, how long they might survive. I can't speak for O'Donnell, but I am genuinely surprised both by how long the democracies that emerged during the past thirty years in Southern Europe and Latin America have survived and by how well they have done.

Q: How have you responded to your critics?

A: I have received a lot of criticism from both the Left and the Right, but I only answered critiques that made me think again about what I had originally written. If I did not agree with an insubstantial part of the criticism, or if the critique seemed clearly unfounded, I would not bother to reply. I used to worry: "I'm not responding enough to my critics." Also, I thought that if I replied to more of them, it would help me clarify my own thoughts. But if I had responded to more critics, I simply would have had no time to do anything else.

#### The Process and Goals of Research

Q: What role do the classics of political and social theory play in your thinking?

A: For me, engaging the classics is almost automatic. I start by thinking about the nature of the problem on which I want to work, and then I ask myself, "Who's said something about this?" Sometimes it is simply a matter of having these classic works in your head, having read them. For example, with the issue of regime transitions, I almost immediately hit upon Machiavelli. It just made sense. My first instinct is to go through my own memory of what I have read in political thought. Another way to start thinking through a problem is to identify an analogous moment, situation, or structure in the past. For example, many people who study the breakdown of democracy start by thinking back to the Weimar Republic, a historic failure of democracy.

#### Cases and Concepts

Q: What role does knowledge of cases play in your thinking?

A: I get the most out of a case when I'm facing it initially and trying to figure it out. I'm a puzzle solver. As we discussed, I couldn't figure out the Argentine puzzle, but I did figure out other ones. I get the maximum return from that initial encounter.

Q: Once you get to know a country, do you keep up with events there?

A: Only to a certain extent. Take Portugal. I do not read regularly and systematically on Portugal. I would even forget about it for a while, but then something interesting would happen in Portugal, or an opportunity would arise, and I would catch up. Also, you can't just walk away from a country after you have done research there. You make friends and have a commitment to it. You may even write pieces that prove useful to people in that country. As a matter of fact, not long ago I collected all the things I had written about Portugal and ended up with an eight hundred-page book (Schmitter 1999). Still, there are diminishing marginal returns after the first encounter.

I try to dissuade students from working on a single country or region. You need to move around, even though working on a single country may have benefits from a purely professional point of view, because you can build a reputation as the best person working on, say, Argentina. I tell students, “Get the hell out of Argentina and go to Italy, Mongolia, wherever.” The potential downside of this strategy is that some people will say, “He moves around too much. He does not know as much as he should about the country he is working on.” But that is not an inexorable problem. In my own case, once I get a hold of a country, I work pretty hard to get into it, both historically and linguistically. I usually start with the secondary literature written by “native” social scientists, if it exists. And I read novels or even travelers’ accounts, if I suspect I will have to go farther back in time. Since I started out with autocracies—Brazil, Argentina, and Portugal—I paid relatively little attention to newspapers. But now that I work virtually exclusively on countries with a free and competitive press, I find reading them very important.

Q: One obstacle to pursuing a strategy of moving from country to country is the difficulty of doing fieldwork—an ideal way to get a feel for a new country—as one advances in one’s career.

A: When it comes to fieldwork, I don’t think it’s a matter of diminishing returns. Fieldwork continues to be both the most productive and exciting part of what we do. It is also a humanly interesting aspect of the work of a comparativist. I certainly feel that way, even though I get very frustrated when I’m in the field. Everything takes so much more time and the exact data one wants are almost never available. Also, fieldwork does become increasingly difficult to do, either because of family and professional obligations or the enormous amount of energy it requires. I’m not sure I could do it again at my age. I do fieldwork vicariously now, through my students. I get them to talk to me about their interviews, and I try to get a feel for that experience. Now that I have a certain standing inside the European Union, I do meet many illustrious people, including presidents and prime ministers. I learn more about politics by talking to people who have to make political choices than from anything else. But this kind of interaction is different from field research. It is not as much fun as doing interviews, as getting over the resistance of people to talk and tell you about things.

Q: Throughout your career you have played an important role shaping and clarifying concepts such as corporatism and democracy, which have been pivotal to large literatures (Schmitter 1974; Schmitter and Karl 1991). Could you discuss your approach to concept formation? Is there a trick to successful concept formation?

A: I wish I knew more about this matter. Somebody like Max Weber knew how to form complex, multifaceted concepts, that is, so-called ideal types.



In terms of myself, it's something that comes easily but un-self-consciously. What I try to do is detect an underlying pattern, to discern the common element that lies beneath all the surface variation in particular cases. For example, in my piece with Terry Karl on democracy, we try to develop a generic, institution-free definition of democracy. That's why we focused on accountability and the reciprocal roles of citizens and rulers. We don't say that representatives have to be elected, that you need parliamentary sovereignty, or even that you need a parliament at all. Rather, we emphasize the need for regular, reliable mechanisms of mutual accountability. In most instances, I am not inventing things ex novo. In the case of corporatism, the concept was lying around in older writings by authors such as Manoïlesco. It hadn't been used in a while, but it was there. I didn't have to invent it, though I did have to define it in a new way.

One thing I find useful in forming concepts is always to consider the antonym. If you are trying to develop a theory of integration, you must also have a theory of disintegration. You must understand both together. If you seek to define an integrated Europe, you must also imagine a disintegrated Europe. Thinking in terms of polarities is a useful means of concept formation. That's what I did with corporatism and pluralism. I put them in the same box, which nobody had done before, and then defined them in an antonymic way. This exercise helps nail down the "ideal-typical" ends, the extremes. Of course, everything in the real world is located somewhere in between, and it's an important and difficult challenge to imagine stable intermediary patterns. Still, nailing down the ends is a very useful device in forming a concept. The trick is to find an underlying pattern, imagine its opposite, and then nail down the ends.

I should add, however, that a lot of my conceptualizations derive simply from talking to real people, including politicians and interest group leaders. I listen closely to the words political actors themselves use in talking and describing what they do. For example, I've recently become a big enthusiast of so-called Euro-speak, the new language the European Union is inventing to talk about its politics, which are so different that they require a whole new vocabulary. Also, I have been lucky to have worked and lived in Brazil and Italy, two countries with marvelously imaginative political vocabularies. I am currently the editor of the Italian section of *Les Intraduisibles: The Dictionary of Untranslatable Political Terms*, and have identified eighty-seven untranslatable Italian terms.<sup>15</sup> I discovered a new one just the other day, *un pianista*. Do you know what a *pianista* is? Literally, it means a piano player, but the term also refers to the deputies in parliament who vote for

15. This dictionary can be accessed at [www.concepts-methods.org/dictionary—intraduisibles.php](http://www.concepts-methods.org/dictionary—intraduisibles.php).

their neighbor when he or she is absent. The Italian parliament has electronic voting, and each deputy punches a code and a key to vote. There are pictures of deputies with their arms extended to either side punching in the

votes of their absent colleagues as if they were playing a piano. Pianista is a perfect term for this; as soon as you hear it, you know exactly what it means. This aside is meant to suggest that one way of stimulating concept formation is to work or live in countries where politics is a lively matter and people are constantly inventing political terms. If you're lucky, people will use expressions you do not know, but when they explain them, you will discover that these expressions disguise important relationships that cannot be confessed openly or that they convey something distinctive for which no generic word exists. Listening closely to the words actors use to talk about their politics is an important way to study it.

### Work Modes and Collaborators

Q: Since your book on Brazil (Schmitter 1971), you have mainly written articles and book chapters. Is there a reason why you have published mostly articles, not books?

A: I move too fast. I am impatient and like being challenged by new things. As soon as I feel something is old or settled, I lose interest and move on. That way of working does not lend itself to writing book-length manuscripts. So I have mainly produced articles.

Q: You have also done a fair amount of collaborative work.

A: Over the years I have collaborated with Gerhard Lehmbruch, Wolfgang Streeck, Guillermo O'Donnell, Terry Karl, and now Claus Offe.<sup>16</sup>

Q: Why do you collaborate, and how do you pick your collaborators?

A: The goal of collaborative work is to end up with a product that is better than what can be produced individually. Several factors make this outcome more likely. I have sought to work with people who have a background different from my own. This means my coauthors are usually foreigners, and they often have a different academic background, though not always in the sense of being trained in a different discipline. I always collaborate with people whom I consider my intellectual equals. And I collaborate with people with whom I share many assumptions about

16. See Schmitter and Lehmbruch (1979), Lehmbruch and Schmitter (1982), Streeck and Schmitter (1985), Hollingsworth, Schmitter, and Streeck (1994), O'Donnell, Schmitter, and Whitehead (1986), Karl and Schmitter (1991), Schmitter and Karl (1991, 1994), and Offe and Schmitter (1998).

politics, which means people with "left of center" values. When you coauthor, you have to share many views, but you also should disagree. You want to make sure you will discuss your disagreements in a productive way,

rather than fighting over stupid or minor things. With all my collaborators, there is a huge number of things about which we initially seem to totally disagree. But we work on those disagreements and try to resolve them creatively. Let me give you an example. O'Donnell and I disagreed a lot when we started working on the transitions project. We had many fundamental agreements, but we also thought we had fundamental differences. Yet, in writing *Tentative Conclusions about Uncertain Democracies* (O'Donnell and Schmitter 1986), we never resolved our differences by resorting to bland compromises. We always tried to resolve them by coming up with a better solution than either of us started out with. That's the right kind of collaborator.

I've been very lucky. I've had incredibly good collaborators, and all have remained close personal friends of mine afterward. I know of collaborations where this was not the case. I am still very close to all the people with whom I have collaborated, and I would work with them again if the opportunity arose.

### Science, History, and Political Engagement

Q: How would you describe the goals of your research? Do you see yourself as a scientist?

A: I seek to produce generalizations. Yet, I also see my work as always bounded by time and space. I am usually very explicit about the time period and range of countries on which I am working at a given moment. I never talk about politics in general. I do not buy into the universalistic aspirations of behavioralism and rational choice theory. That's just not the way I think about science and about what I do. I think the human sciences are fundamentally historical. Thus, you must specify the temporal and geographical context within which the generalizations you are working toward are valid. As Aristotle says, one of the first things you should understand about a science is that it must be true to its subject matter. This means there are many kinds of science, not just one kind. For me, the subject matter of politics is historical. If political science is to become a science, it will be a historical science.

Q: How do you see the link between your academic work and the world of politics?

A: I have never been a regular party person, partly because I've always been on the run and never lived much in any one place. I feel a little bit more grounded now in Italy. I have a relationship to the democratic centrosinistra (Center-Left). But it's still as an outsider. The first and only time in my life that I agreed to advise a government—I now occasionally advise the European Commission, but I don't think of it as advising a government—was in 1974–75, when it was rumored that Henry Kissinger thought it would be a good idea if Portugal went communist. He thought

this would inoculate other Western European countries from communism and help destroy all the domestic communist parties in the region. Well, the State Department was about to send Frank Carlucci as U.S. ambassador to Portugal. Carlucci was a notorious CIA type who, as a matter of fact, had denounced me in Brazil as “an enemy of the Pan-American system.” I was asked to go to Washington to brief Carlucci on Portuguese politics. It was my view, and it still is, that my job is to produce works that are public. Indeed, I believe very much in the public nature of the social sciences, and I am opposed to having social scientists whispering things in the ears of rulers. But I did it this one time. I went to Washington and talked with Carlucci. Seeking to counter the perspective that aimed to foment a red scare and thus trigger a U.S. intervention, I told Carlucci that Portugal was one of the most conservative countries in Europe, it was not going to go communist, and the Portuguese could take care of things themselves. Soon thereafter, Carlucci went to Portugal and did the exact opposite of what I expected. He made sure we didn’t force the Portuguese into anything, and he essentially became the architect of the alliance among the United States, Germany, and Britain in support of the Portuguese Socialist Party. Carlucci deserves a lot of credit. He went into a situation where there was every reason to believe he would interpret things in a viscerally anticommunist way. Instead, he saw that Portugal was basically a conservative country and concluded that all the United States had to do was support the Socialist Party and wait. He was right.

#### Colleagues and Students

Q: You have basically worked at three institutions, the University of Chicago (1967–82), Stanford University (1986–96), and the European University Institute (1982–86, 1996–present). What was your experience like at these three places?

A: They are very different institutions. Chicago was by far the best from an academic perspective. It was a most stimulating department for three reasons. First, it was a very nonhierarchical department, despite having some very notable types like David Easton, Morton Kaplan, and Hans Morgenthau in it. Junior people had the same rights as seniors, and could teach the courses they wanted to. We were all equal in most regards, except for obvious things, like the ability to vote on tenure decisions. Second, Chicago had an unusually communicative department. This was due partly to an ecological factor: most of the faculty lived in the same neighborhood. As a result, we saw each other all the time and met in coffeehouses as well as in the classroom. We often ended up as best friends. Third, even though we were a very egocentric, strong-minded bunch of people, who liked to argue with each other, the lines of cleavage never became polarized. This had been the case at Chicago in the recent past, when a sharp divide existed between Straussians and others.<sup>17</sup> But that cleavage had been overcome by the time I arrived. At Chicago, you constantly found yourself agreeing and disagreeing with different people. Though arguments were strong, they never developed into an “Us versus Them” situation. That extended down

to the graduate students, who felt free to put together committees with different kinds of people and even benefit from our disagreements. Chicago was a place where the whole was greater than the sum of its parts.

Q: Who were your closest colleagues at Chicago?

A: Almost the entire department. But I would mention in particular Aristide Zolberg, Leonard Binder, Ira Katznelson, and Brian Barry. I should add John Coatsworth and Friedrich Katz, two Latin Americanists in the History Department, to this list. After a time, however, lots of us left. It started with Barry, then Katznelson, and then I left for the European University Institute.

Q: You overlapped at Chicago with Adam Przeworski.

A: I was instrumental in bringing him to Chicago. He became very much a part of the mix and stayed on many years after I left. We do things differently, which sometimes translated into a prickly relationship that would occasionally manifest itself in the way we worked with graduate students. Przeworski likes to work with graduate students who do what he does, which usually has a rational choice bent. In contrast, I work with students who are interested in doing a wide range of things. We have admiration for each other, and he was a very good colleague. We have a longstanding personal relationship that transcends our academic differences. That is why, as I said earlier, Przeworski was brought into the project on transitions from authoritarian regimes at the Woodrow Wilson Center right from the beginning, even though at that time he was neither a specialist in Latin America nor Southern Europe. I wanted to make sure he was part of that project, because I knew he would make an important contribution.

17. Straussians refers to the followers of Leo Strauss (1899–1973), a German Jewish émigré political theorist who taught at Chicago from 1949 until 1968.

Q: After a few years in the 1980s at the European University Institute in Italy, you moved back to the United States and took a job at Stanford in 1986.

A: I was happy in Italy, and I would have stayed there for as long as possible. But I had a personal relationship with Terry Karl, and she had a job at Stanford. I moved to Stanford because of her, and I guess it looked perfect on paper. Stanford supposedly had a very good department. But I soon realized I was in the wrong place for me.

Q: What was the problem with Stanford?

A: Stanford was exactly the opposite of Chicago. Stanford was a place where no two people talked to each other, even if they were on the same side of an issue. It had no social or intellectual life whatsoever. People came to campus

to work, if they came, at 9:00 a.m. and left at 5:00 p.m. Some people played tennis together. But if you were not one of them, you were not a member of anything. Moreover, at Stanford most of the department members were not very intellectual people; they were just professional academics doing their job. At Chicago, by contrast, we were not doing a job. Political science was our vocation, and we were thinking and arguing about politics all the time. At Stanford, they were more like businessmen than intellectuals. There was no interaction; the department was completely dead. A growing rift developed in the department at Stanford, as a coalition led by rational choice theorists pushed for the Americanization of comparative politics and simply left no room for people who knew languages, did field research, and brought in-depth knowledge of countries to bear on their research. Things became very polarized. Many of the people who did comparative politics ended up spending more of their time in various area studies and international centers. In general, these people were attacked and marginalized. It became a very closed environment. An overwhelmingly compact group became dominant, and this group always voted together. As a result, department meetings were very uninteresting. I eventually stopped going. Stanford was a tremendously negative experience. There was just no payoff from being there, except for the intellectual and personal company of Terry Karl.

Q: In 1996 you moved back to Italy and rejoined the European University Institute (EUI).

A: The EUI has been wonderful. The location, in Florence, is obviously an attraction. We have wonderful students. And the faculty is top-notch. At the EUI, all the faculty have temporary appointments with a maximum stay of eight years. So, it's a constantly revolving door, which means you are always in the business of finding new colleagues, and you have a lot of say in who they are. I am convinced we now have the best department in Europe in political science and sociology. It has become the place to go for graduate work in these fields. The EUI is also very stimulating because the student body is so diverse. It is a university with no dominant ethnic or national group. There is no other place on earth like it. We have the same number of Germans as Italians, French, British, and so on. That's exciting and challenging. I am exactly where I want to be, and I can't imagine a better job. I feel very lucky.

Q: Turning to teaching, what is your approach to training graduate students?<sup>18</sup>

A: As a matter of principle, I do not tell any graduate student what to work on. This is very unusual in Europe, because professors there usually tell their students what they should work on. Thus, students are surprised when I say they have to find a research topic themselves and decide whether that topic is sufficiently important to them. I tell them, "You should not worry about whether I think your topic is important. Also, forget about what the discipline might think. Make sure it's important to you." You have to care, very deeply, about the subject matter of your dissertation. Now, once a student

has picked a dissertation topic, then I become very intrusive. I place a lot of attention on theory, and I expect students to really know what they want to do from a conceptual point of view. I read literally every word they write, criticize it, and ask, “Why didn’t you do this or that?” I really grill them, especially on that critical first chapter. I expect a dissertation to go through several drafts. On the other hand, I’m not at all insistent about the kinds of methods or data they should use. I have supervised everything from survey and aggregate data studies to interview- and documentary-based research. I’m very eclectic when it comes to methods and data. That’s my basic philosophy. I imagine I am pretty difficult to work with. I am very demanding of students.

### The Achievements, Shortcomings, and Future of Comparative Politics

Q: What has comparative politics accomplished? What do we know now that we did not know when you were in graduate school in the mid-1960s?<sup>19</sup>

18. For a graduate student’s perspective on Schmitter as a teacher, see the interview with David Collier in Chapter 15.

19. For Schmitter’s overall assessments of the field, see Schmitter (1993, 2002). Schmitter’s efforts at synthesizing the literature on democratization include Schmitter (1995) and Schmitter and Guilhot (2000).

A: The field has made important gains. The most obvious ones concern what I would label the spatial dimension. Today, as opposed to forty years ago, you no longer feel you have knowledge about politics if you have knowledge about just one country. This is a big accomplishment, one that has helped transform comparative politics into a political science, though this transformation has perhaps gone less far in the United States than in Europe. We also study politics in a broader, more integrated way. For example, instead of studying elections or political parties in isolation, as was previously the case, we now look at the entire field of political interests and consider how they relate to political parties and different modes of public policy making. I am very proud to have been part of the efforts to re-shape the field in this way. But these changes are also unintended byproducts of structural functionalism. This approach got it wrong in the way it went about doing things. It postulated a highly abstract set of functions, derived largely from Talcott Parsons, that had to be performed for a polity to become and remain stable. The static bias was obvious, as was the strong assumption of systemic interdependence. Needless to say, to someone like myself, who was more concerned with change in regime, and in changing regimes, this was rather daunting. But the real bias in this apparently “universalistic” theory came when specific institutions or institutional complexes were assigned a privileged status in performing specific functions. At this point, for me, structural functionalism looked suspiciously like an abstract and crude description of the American political system. It would have been much more useful to have started with historical, “real-existing” institutions in different polities and then tried

to discover what functions they performed and whether or not their interdependence with other institutions actually led to non-coercive stability. In short, the whole approach lacked both historicity and stateness, precisely what I was looking for. Still, structural functionalism pushed us to take a broader, more holistic view of the political process. This is a new aspect of the discipline, and there are a number of generalizations that are linked to this broad-gauged research that we did not have in the decades before. For example, we now know that, pace Duverger, electoral systems do not alone produce party systems. Or, as I gradually and reluctantly discovered, that an understanding of different configurations of interest associations cannot be separated from the nature of partisan competition and the party or parties in power. On some topics, there is no single received wisdom. There may be two or three alternative views, which we haven't quite unraveled yet and which stand as rival hypotheses. But this is much better than the approximate kind of knowledge we had before. It is wrong when people say there has been no accumulation of knowledge in comparative politics. Indeed, there would have been even more accumulation of knowledge by political science in the United States if it weren't for this business of fads—behavioralism, structural functionalism, and now rational choice theory. Because of fads, academics seek to prove themselves by denying that the generation that preceded them produced anything of value. This tendency can be seen among those who pretend they are starting political science from scratch with rational choice theory or whatever it is they have just picked up. They even go so far as to say that everything written before them about a certain topic is junk. I disagree. We stand at a different place today than where we stood before. By and large, it's a better place, a place with a wider and more discerning perspective from which to observe politics.

Q: Your remarks remind me of Gabriel Almond's emphasis on the need for better professional memory (Almond 1990, Part II).

A: Almond certainly argues that there is not enough cross-referencing across generations and across time. Indeed, as we discussed earlier, he even made this point with regard to the corporatism literature (Almond 1983). I think I actually go out of my way to reference previous sources. If I'm guilty of anything, it is that I haven't paid enough attention to American political science. I'm not denigrating this literature. But if I am going to work on, say, Brazil, I am probably going to work with monographs and articles written by Brazilians, not Americans. Maybe once in a while I'll find something useful written outside Brazil. But I confess that I have not paid much attention to what has been written by Latin Americanists except, of course, by my colleague and companion, Terry Karl. I say this with a certain amount of reserve, because I, too, write about other people's politics, and what I am saying here about Latin Americanists could certainly be said about my own work. Still, I find I learn more by entering into relationships with local social scientists.

Area Studies

Q: Do you see any notable failures of comparative politics?



A: The most obvious one was the enormous failure to understand the nature of communist systems. This failure illustrates the fundamental mistake of allowing students of Soviet systems to form a cyst inside comparative politics. They did not read what comparativists were writing, and they simply did not pay attention to matters as basic as Euro-communism. I didn't read any of their work either, with a few exceptions, because they weren't speaking my language. The separation of Sovietologists from the rest of the field was quite stark. This episode provides an important lesson for area studies. Area studies have some very important payoffs, but they have to remain open. Students of particular areas have to read what other people are writing, and they have to be prepared to jump out of their respective regions to find points of comparison elsewhere. This is exactly what Sovietologists were unable to do. Their model held that communism was simply different, and, hence, communist systems were not to be compared with anything except each other. Indeed, there is still a group that says since communism was different, post-communism will also have to be different. I have argued strongly against this perspective.<sup>20</sup>

Students of Latin America—I was one of them—were never so insular. We were always interested in other areas. Latin America had an ambiguous intellectual position between the United States and Europe. There was always an implicit comparison being drawn between Latin American countries and these external reference points. This openness to other regions and areas explains why the literature on Latin America did a much better job of analyzing regime change. O'Donnell, and I, as well, had a lively sense of how vulnerable authoritarian regimes were in Latin America. We didn't predict exactly when democratization would occur, but we were not surprised by it, because our models of authoritarian rule gave us an understanding of the inherent contradictions of these systems. We were not locked into anything as hermetic as the totalitarian model that dominated the study of communist systems. It is a fundamentally misguided strategy to develop a different language and separate assumptions for a subset of countries.

#### The Future of Comparative Politics and the U.S. Model of Political Science

Q: In light of this assessment, what do you think about the future of comparative politics?

A: I wrote about this question in a paper published recently in *European Political Science* (Schmitter 2002). Many people—certainly Americans, British, and perhaps even a few Scandinavians—think this is an easy question to answer. In their view, the future of comparative politics and political science is already on display in the United States. The United States sets the standards, because of the number of political scientists in the United States, their high level of professionalization, the prestige and quantity of their journals, and

so on. As a result, it is only a matter of time before political scientists in other countries converge on the American model. My article challenges the thesis that the United States provides “the face of the future” for comparativists.

Q: What problems do you see with the way political science is practiced in the United States?

20. See Schmitter and Karl (1994) and Karl and Schmitter (1995).

A: One key problem is that too much of U.S. political science is based on the study of the United States, and the United States is a peculiar case because of its non-feudal past, the absence of hostile neighbors, the presence of multiple and overlapping cleavages in an immigrant society, and so on. Therefore, the kind of political science generated on the basis of the American case, an exceptional political situation, cannot possibly be a universal political science. Findings based on the United States are simply not likely to travel well. Americanists in the profession find themselves in a peculiarly paradoxical position: they insist that “their” polity is exceptional and that whatever they discover must be universally applicable. It would be another matter if U.S. political science were to treat the United States as a case, one just like any other. But this probably will not happen, because Americanists are very resistant to this suggestion. The situation is different in Europe. Nobody in Europe would resist the idea that Italy or Spain is just a case. In fact, in Europe, it is increasingly unclear whether Italy and Spain are still cases in the traditional sense, because the politics of these countries have become so intertwined with that of the European Union. European countries can no longer be seen as involving clearly independent units.

Another problem with American political science, to which I have already alluded, is that it is so prone to generating fads, something I see as a consequence of the vastly more competitive nature of the profession in the United States and the resulting tendency to exaggerate the importance of one’s approach and methods. Competitiveness is both the worst and best aspect of American political science. On one hand, it leads to a negative cycle of fads and the overvaluation of what a new approach, method, or theory has to offer. On the other hand, the competitive nature of the profession in the United States destroys the very fads it produces; a competing group inevitably forms whose main business is to poke its fingers in the dominant tradition. In Europe, by contrast, the political science profession is more intrinsically conservative and anticompetitive, partly because of the highly bureaucratized nature of the universities. As a result, European political science is much more resistant to innovation, but once an innovation manages to penetrate, it sticks and gets absorbed more thoroughly. Perhaps this is what produces a stronger sense in European political science that accumulation of knowledge is occurring.

Q: With regard to the dominant approaches in American political science during the past fifty years—behavioralism, structural functionalism, and rational choice—do you draw any distinctions in terms of

their impact?

A: In one sense they are exactly the same. Take the advocates of rational choice. They have been telling graduate students that they need to learn rational choice theory, they have exclusively promoted these students, and they have effectively formed a “club” within the discipline. That’s precisely what the behavioralists did. And it’s exactly what the structural functionalists did through the Social Science Research Council’s (SSRC) Committee on Comparative Politics. They all formed their self-admiring clique, and then everyone promoted each other. Still, I do see a difference among these approaches. Behavioralists, and to a certain extent the structural functionalists, helped produce a wealth of studies on the Third World. As a result, we have lots of people who became experts on various obscure parts of the world. They produced useful knowledge, for which there was a demand both inside and outside the academy. In contrast, I do not see rational choice scholars producing much of any practical utility, because the basic assumptions of rational choice theory about politics tend to be quite unrealistic. To be sure, there are some very interesting aspects of rational choice theory, or at least there could be, inasmuch as the limits of these assumptions are acknowledged and the theory is not applied to situations where it is completely inappropriate. But I perceive little willingness to understand and confront the limits of rational choice theory.

Q: What do you see, then, as the future of rational choice in political science?

A: I do not think it is dominant now, and I do not think it will become dominant, even in the United States, precisely because of the competitive nature of the profession there. I am very confident that the rational choice bubble will burst. As with previous fads, rational choice theory will leave some residue. After it passes, each successive fad leaves a little bit more diversity in departments.

#### Research Agendas

Q: Where do you think the big developments in comparative politics are likely to be in the next decade or so?

A: The problem with trying to answer such a question is that one tends to focus on the things on which one is working. At least I do, I have to admit. With this caveat, let me suggest two things that will be important. The first concerns transnational political processes that cannot be subsumed under international relations in the classic sense of processes driven by national interests and involving diplomatic bargaining, relative advantage, “self-help,” naked power struggles, and so on. The processes might be regional or global. A lot of this is already happening in Europe, because of the EU, but I also see such trends elsewhere, for example, in Latin America with Mercosur.<sup>21</sup> The second thing I would highlight involves the quality of democracy

and the possibility of developing a new normative democratic theory. The study of democratization has more or less run its course. Still, we need a new type of democratic theory that can grasp the fundamental changes that democracy itself is undergoing. Robert Dahl (1989, Chs. 1, 2, 15, 22, and 23) develops the wonderful insight that democracy has already undergone several revolutions. I think we are currently in the middle of another of those democratic revolutions. The nature of the actors is changing. The real citizens today are organizations, not individuals. Indeed, individuals act effectively in modern democracies, to the extent that they do at all, only through organizations. The types of organizations that are relevant have also shifted, as political parties become less important and interest associations and social movements more so. Thus, we need a more “organizational” theory of democracy. Moreover, the unit of government is shifting. We are now talking about supranational democracy and also paying more attention to subnational democracy. So we are going above and below the nation-state as the unit of analysis. This requires a different democratic theory, because 99 percent of democratic theory assumes the prior and consensual existence of a nation-state.

## Conclusion

Q: To conclude, what’s your advice to graduate students entering the profession?

A: Comparative politics is the most challenging and difficult, but also the most fun and rewarding, field of political science. To be a good comparativist, you have to be comparative yourself. That is, you must habituate yourself to living in different cultures and being on the outside. You have to structure your life comparatively, seeking out opportunities to go to different countries. This is not easy to do, especially if you have a family and other “fixed assets.” I’ve been lucky. I’ve had children who didn’t mind me moving around. Well, to tell you the truth, I’m not sure about that, because I forced my kids to live in eight different countries before they went to college. So they may tell a different story. In a nutshell, if you really wish to be a comparativist, you must be prepared to live a comparative life.<sup>22</sup>

21. Mercosur (Mercado Común del Sur—Southern Common Market) is a trading zone founded in 1991 by Argentina, Brazil, Paraguay, and Uruguay.

22. For further suggestions, see Schmitter (1997b, 295–97).