

CHAPTER 1

THE STATE OF THE DISCIPLINE, THE DISCIPLINE OF THE STATE

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A “handbook” is Germanic in both its origins and its ambitions. Handbooks invariably aspire to be comprehensive, systematic, exhaustive—and above all, in contemporary academic practice, *big*.¹ A handbook, at least in its academic instantiation, is definitely not a pocketbook. An editor of one particularly large volume of the *Oxford Handbooks of Political Science* wryly describes his as a “two-hand book.”

Weighing in around 1,000 pages, such handbooks usually manage to be exhausting. But exhaustive is something else. Even with 1,000 pages, handbook editors soon come to realize just how selective they must nonetheless be in their choices of topics and treatments. When even ten volumes of that size prove not enough, it becomes clear just how ill conceived any aspiration to comprehensiveness and exhaustiveness must surely be.

The best any handbook can do is to offer a bird’s-eye overview of the general shape of its subject, combined with some posthole exercises to show what riches might be found by probing deeper. That is the spirit in which this volume is offered. It is a schematic guide, and a sampler. There is much more by way of elaboration in

¹ Making academic usage deviant, judging from the *Oxford English Dictionary*’s definition of a “handbook” as “a small book or treatise, such as may conveniently be held in the hand.”

the ten volumes upon which this one draws. There is, however, very much of great consequence that is left out, not only of this volume but also of those other ten as well.

So this book offers a glimpse of the breadth, the depth, and the excitement of political science. It is an invitation to delve deeper into the underlying ten volumes that constitute the series of *Oxford Handbooks of Political Science*.² It is an invitation to delve deeper into the discipline that even those ten volumes can merely skim. It hints at the wide range of topics that have recently been preoccupying political scientists, the wide range of theories that they have formulated about them, the wide range of techniques that they have deployed in systematically examining them.³

In short, this book does not tell you everything you need to know about political science. What hopefully it does do is give some indication of why you should want to know, and how you might go about finding out.

1 THE DISCIPLINE

1.1 A Mission Statement

Political science is a discipline with a mission. The main task of this chapter is to describe the former. Before turning to the state of the discipline, however, let me say a few words about how I conceive its mission.

The most oft-cited definition of “politics” is Lasswell’s (1950): “who gets what, when, how.”⁴ Certainly that is a correct assessment of why we care about politics. If politics carried no consequences, if it made no material difference in the larger world, it would hardly merit serious study. At most, the study of politics would then amount to an exercise in purely aesthetic appreciation of courtly intrigues, deft maneuvers, clever gamesmanship, and such like: cute, but inconsequential.

We should, however, separate out “why politics matters” from “what it is.” Laswell’s “who gets what, when and how”—broadly construed, per Lowi (1964)—is a good answer to the former question as to why politics matters. As to what politics is,

² And an eleventh not formally part of that series but very much a companion volume: Sears, Huddy, and Jervis’s *Oxford Handbook of Political Psychology* (2003).

³ My focus in this chapter is primarily on recent tendencies, glancing backwards to the discipline’s past mostly just to ground prognostications as to its future. Those interested in more detail on the path to the present, particularly as regards some particular subject, can piece together the story from relevant chapters in the many handbooks surveying the discipline that have preceded this one. On the US discipline, which is this chapter’s principal focus, see: Greenstein and Polsby 1975; Finifter 1983; 1993; Goodin and Klingemann 1996a; Katznelson and Milner 2002. On developments outside the US see the sources cited in n. 12, and on the discipline’s history see those sources cited in n. 40.

⁴ Followers of Arendt of course dissent (Calhoun and McGowan 1997).

however, I suggest a better answer would be this: politics is the constrained use of social power.⁵

Power of course takes many forms, and is constrained in many interestingly different ways (Lukes 1974/2005; Scott 1986; 1997). Systematically mapping all that is the fundamental task of political science.

Political scientists are often seen as handmaidens to power. Some cherish that role. Machiavelli and his modern-day heirs style themselves as counselors to princes and parties, advising on how to seize and wield power (Morgenthau 1948; Schultze 1992; Neustadt 2000). Other political scientists, taking their inspiration from Marx's eleventh thesis on Feuerbach, adopt a more critical stance toward the powers that be.⁶ Self-styled policy scientists occupy points along the continuum, ranging from "accommodative" to "critical" (Wildavsky 1979; Dryzek 2006a).

Attempts at manipulating power always confront countervailing power and the constraints that come with that. Much though the strong might try to bend others to their will, their capacity to do so is inevitably limited. The weak have weapons of their own (Piven and Cloward 1979; Scott 1986). In politics, there is no such thing as a literally "irresistible force." Even the powerful cannot just dictate—they have to persuade as well (Majone 1989). As he was passing the US presidency on to a five-star general, Truman mused, "Poor Ike—it won't be a bit like the Army... He'll sit here and say, 'Do this, do that.' And nothing will happen" (Neustadt 1990, 10). But since the same is true even within a notionally hierarchical military chain of command, it turned out that Eisenhower already knew as much (Greenstein 1982). The essence of politics lies in strategic maneuvering (Riker 1986). Politics is a matter of pursuing your purposes as best you can, in the context of other purposeful agents doing the same, and with whom, through whom, or around whom you must work to accomplish your goals.

Mid-twentieth-century pluralists made much—perhaps too much—of the idea of polyarchy, of multiple centers of (implicitly, pretty nearly equal) power (Dahl 1961b; 1972; Polsby 1980). Critics rightly challenged their naivety in several respects (McCoy and Playford 1968; Bachrach and Baratz 1970; Lukes 1974/2005; Foucault 1991). They rightly emphasized how power might work behind the backs of agents, how structures channel agency (Wendt 1987; Jessop 1990), how social constructs enable and disable (Finnemore and Sikkink 2001), how ideas shape and obscure interests (Laclau and Mouffe 1985; Goldstein and Keohane 1993).⁷ Useful correctives, all. But the cumulative effect is of course to expand, not contract, the list of ways in which power might be constrained as well as exercised.

⁵ Tweaking Duverger's (1964/1966, ix) characterization of it as "organized power, the institutions of command and control."

⁶ "The philosophers have only *interpreted* the world, in various ways; the point is, however to *change* it" (Marx 1845/1972, 109). That has long been the stance of the Caucus for a New Political Science, for example (Anon 2007).

⁷ A propos the latter, Claus Offe tells me of a statue in the center of old East Berlin, with graffiti below depicting Marx saying to Engels, "It was just an idea..."

The general idea that politics is about the constrained use of power is not merely a pluralist preoccupation. It is endemic to liberalism more generally (Hume 1777), and to liberal democracy most particularly (Macpherson 1977).

Obviously, even in autocratic regimes it is of intense interest to the powerful how they can work around constraints to wield power effectively. Machiavelli envisioned himself advising a fairly ruthless Prince, after all. Politics has been studied in just that spirit in all sorts of societies for a very long time.

But it is no accident that political science as a discipline has grown up alongside and in the context of liberal democracy, with its very special emphasis upon checks and balances, separation of power, political accountability, and political competition.⁸ It is in that setting that political science acquired its distinctive mission: to elucidate how social power is, can be, and should be exercised and constrained.

Power is constrained not only by countervailing power and social structures. Power is also constrained by purpose (Reus-Smit 1999)—not just by the powerholder's actual purposes, but also (and in certain respects more importantly) by what those purposes *should* be. A central plank of the mission statement of political science lies in the elucidation of proper purposes, of worthy goals, and of rightful ways of pursuing them.

Political philosophers have sometimes felt marginalized by the scientific turn of the discipline (Storing 1962; Wolin 1969; Dryzek, Honig, and Phillips, this volume). But that is just one more—albeit perhaps the most central—of the false dichotomies I shall be bemoaning in this chapter. Far from being peripheral to the main mission of political science, normative concerns are absolutely central to it.

Of course, values are different from facts, and anyone studying them had better keep those differences straight. That it would be good for some fact to be true does nothing to make it true. To suppose otherwise is just plain wishful thinking. That it is a fact that people think something is good or valuable does not make it truly so, except in the shallowest supply-and-demand sense. You do not establish the truths of morality, any more than those of mathematics, by taking a vote (or sampling opinion, either).⁹

While different methodologies are clearly required for exploring each of those two realms, it is equally clear that both must be pursued in tandem if political science is to accomplish its mission as I conceive it. Consider an analogy: moral philosophers tell us that ethics is supposed to be “action-guiding;” but clearly ethics must connect some facts about the world to the values it recommends, if it is to provide any guidance on how to act in the real world. Equally clearly, from the other side, political

⁸ “For the greater part of its history, American political science has been tied to its political sibling, American reform liberalism” (Seideman 1993, 311). Something similar was true in the UK (Collini, Winch, and Burrows 1983), although in continental Europe the emphases were more statist-modernizing ones (Wagner, Wittrock, and Whitley 1991; Heilbron, Magnusson, and Wittrock 1998; Wallerstein 1998, ch. 1). See more generally Easton, Gunnell, and Stein (1995).

⁹ Useful though surveys of people's values are for the other quite distinct task of explaining and predicting their behavior, of course (Inglehart 1977).

science needs to connect up its empirical insights to some values in order to perform its own larger purpose.¹⁰

What is the point of finding out how things are, without wondering how they could and should be (Moore 1970; Geertz 1977)? Those are different questions, to be pursued in different ways perhaps by different people and certainly using different tool kits. Nonetheless, they are both clearly components of one and the same larger enterprise (Reus-Smit and Snidal, this volume). The mission of political science requires it to combine both.

1.2 The Discipline of a Discipline

When calling political science a “discipline,” pause to ponder the broader associations of that term. According to the *Oxford English Dictionary*, “discipline” has all the following connotations (and more):

- A branch of instruction or education; a department of learning or knowledge; a science or art in its educational aspect.
- Instruction having for its aim to form the pupil to proper conduct and action; the training of scholars or subordinates to proper and orderly action by instructing and exercising them in the same; mental and moral training; also used *fig.* of the training effect of experience, adversity, etc.
- The orderly conduct and action which result from training; a trained condition.
- The order maintained and observed among pupils, or other persons under control or command, such as soldiers, sailors, the inmates of a religious house, a prison, etc.
- A system or method for the maintenance of order; a system of rules for conduct.
- *Eccl.* The system or method by which order is maintained in a church, and control exercised over the conduct of its members; the procedure whereby this is carried out; the exercise of the power of censure, admonition, excommunication, or other penal measures, by a Christian Church.
- Correction; chastisement; punishment inflicted by way of correction and training; in religious use, the mortification of the flesh by penance; also, in more general sense, a beating or other infliction (humorously) assumed to be salutary to the recipient. (In its monastic use, the earliest English sense.)

Running through all those definitions is this underlying thought: To subject yourself to some discipline is to be guided by a set of rules for doing certain things in an orderly fashion, rules that are shared among all others subject to the same discipline. Those who share the discipline take a critical reflective attitude toward those aspects of their conduct that fall under those standards (Hart 1961), judging their own conduct and that of others according to those standards (Hughes 1958; Caplow and McGee 1961; Parsons 1968; Sciulli 2007).

¹⁰ The founding idea of American political science was one “of the discipline as a source of knowledge with practical significance” (Gunnell 2006, 485).

The discipline of political science is less fearsome than that of the church. “Mortification of the flesh by penance” is no part of standard induction into our discipline; and while some of its practitioners are denied tenure, few are literally banned from professing political science ever again. Still, it is an essential part of academic disciplines that they offer standards that can provide grounds for control, chastisement, and even occasional mortification. To discipline is to punish (Foucault 1977; Moran 2006), if only symbolically, if only occasionally or merely potentially.

Subjecting yourself to the discipline of a discipline is to accept constraints that are enabling in turn. A discipline imposes order. Its shared codes, traditions, standards, and practices give its practitioners something in common. A shared disciplinary framework channels the collective energies of the profession and facilitates collaborative attacks on common problems. It is what enables underlaborers to stand on the shoulders of giants, at the same time as enabling giants to stand on the accumulated product of underlaborers’ efforts in turn. The division of the universe of knowledge into disciplines and sub-disciplines facilitates division of labor among practitioners who inevitably cannot be expert in all things (Abbott 1988). A shared disciplinary framework is what unobtrusively coordinates all our disparate research efforts and enables the discipline’s findings to cumulate, after a fashion, into some larger synthesis.

An academic discipline is also, nowadays, a profession. But what our profession professes is, by and large, just its own professional competence. Professional associations serve to carve out an occupational niche for practitioners. So it was both with the American Political Science Association and many others around the world.¹¹ (In this chapter I shall concentrate primarily upon developments within the US discipline, which have such a powerful influence on how political science is practiced worldwide: developments elsewhere are canvassed in many other excellent collections.¹²)

The sociology of work regards “professional” as a high-status occupational grade, access to which is typically controlled by existing members of the profession (Abbott 2002). Many bemoan the ritual practices of these self-replicating cartels, both in indoctrinating newcomers and in defending their turf against outsiders (Wallerstein 1998); and in their purely self-defensive modes, professions can indeed be conspiracies against the public interest, academically as surely as otherwise.

But academic professions have another side as well. They are self-organizing communities of scholars dedicated to trying to find progressively better answers to the problems around which they are organized. Anyone in doubt needs merely reflect

¹¹ Gunnell (2006). On the UK see Chester (1975); Barry (1999). For an intriguing mid-century assessment of developments around the world, see Macpherson (1954). Supranational associations serve rather different purposes (Coakley and Trent 2000; Rokkan 1979; Newton 1991).

¹² Schmitter (2002) is right that there are sometimes interesting local variations. See Easton, Gunnell, and Graziano (1991) for a wide-ranging comparative overview. For developments in Western Europe, see Newton and Vallès (1991), Dierkes and Biervert (1992), Quermonne (1996), and Klingemann (2007); and, in Eastern Europe, Kaase and Sparschuh (2002) and Klingemann, Kulesza, and Legutke (2002). For developments in specific countries, see Hayward, Barry, and Brown (1999) on the UK, Leca and Grawitz (1985) on France, Beyme (1986) on Germany, and Graziano (1987) on Italy.

upon the dilettantism of the American Social Science Association that preceded the American Political Science Association (Kaplan and Lewis 2001; see further Seidelman and Harpham 1985, 20; Seidelman 1993), and what passed for political science in the first third-century of the APSA's own existence (Sigelman 2006*b*, 473). Compared to that, the increasing professionalization of political science from the middle of the last century onward has surely greatly enhanced the discipline's collective capacity to bring a systematic body of theoretically integrated insights to bear on important problems of politics and society.¹³

Over a decade ago the *New Handbook of Political Science* offered a similarly rosy prognosis (Goodin and Klingemann 1996*b*; Almond 1996). Some scoffed at that as an unduly whiggish view of the discipline's history. Doubtless scoffers were right, in part. Progress comes in fits and starts, at different rates in different times, different sub-disciplines, and different countries (Dryzek and Leonard 1995; Sigelman 2006*b*, 473).¹⁴ But there is movement in the right direction in most places. Let one example suffice. Writing in 1999, Barry (1999, 450–5) said he could see no evidence in the British discipline of the sort of professionalization the *New Handbook* described; less than a decade later, there has been a generational shift and a dramatic cross-fertilization of British political science from abroad (Goodin et al. 2007, 34–5; Goodin 2009).

1.3 Against Either–Or

There are many who think that the discipline of political science has, over the past half-century, taken wrong turns and gone up blind alleys, that too many eggs have been put in far too few methodological baskets. There are many who think that the disciplinary control exercised over the profession by certain sects has been far too tight.¹⁵ That was the complaint of the Caucus for a New Political Science against behavioralism in the 1960s and 1970s (Easton 1969; Anon 2007) and of the Perestroika movement against rational choice in the 2000s (Schram 2003; Monroe 2005; Rudolph 2005).

To some extent those were movements targeted more at organizations than ideas. Reform of the professional association was high on the agendas of both insurgencies. In both cases, the American Political Science Association fobbed off the insurgents with a quintessentially organizational ploy: giving the dissidents an official new journal all their own (*PS* for the Caucus, *Perspectives on Politics* for Perestroika).¹⁶ To a

¹³ As I wrote in commenting on the state of the discipline in Britain, “I defy anyone hankering for a return to the pre-professional past to say in all honesty that they wish they had written any of the chapters in for example the (pre-Royal) Institute of Public Administration's survey of *British Government since 1918* (Campion et al. 1950)” (Goodin 2009).

¹⁴ Sometimes in surprising ways: “how to become a dominant French philosopher” seems to be to get picked up by US comparative literature departments, judging from the case of Derrida (Lamont 1987).

¹⁵ The problem is not peculiar to political science, of course. Deirdre McCloskey (2006, 55) delights in quoting back to contemporary economists Oliver Cromwell's words to the Scottish kirk: “I beseech you, in the bowel of Christ, think it possible you may be mistaken.”

¹⁶ Many would say they didn't remain “all their own” for long, with *PS* soon becoming an establishment organ and *Perspectives* soon moving to the rational-choice base camp (Rochester).

large extent, however, those movements were targeted at credos of the profession, the Caucus bemoaning its insistent value-neutrality, Perestroika bemoaning its narrowness of vision. Those issues are harder to resolve.¹⁷

Here, however, I want to focus on one characteristic feature of these periodic “great debates” within the profession: their Manichean, Good versus Evil form. Nor is it found in only those major episodes that traumatized the profession as a whole. Even as regards the more substantive “great debates” within each of the various sub-disciplines, there is a remarkable penchant for representing the options in “either-or” fashion. Behavioralist or traditionalist, structure or agency, ideas or interests, realist or idealist, rationalist or interpretivist: you simply have to choose, or so we are constantly told.

On all those dimensions, and many others as well, the only proper response is to refuse to choose. Respond, insistently, “Both!” Both sides to the argument clearly have a point, both are clearly on to something. Elements of both need to be blended, in some judicious manner (not just any will do¹⁸), into a comprehensive overall account.¹⁹

The “tyranny of small differences” is a notorious hazard across all of life. Among academics on the make, the tendency to exaggerate the extent of their differences, so as to emphasize the novelty and distinctiveness of their own contribution, powerfully fuels that general phenomenon (Moran 2006). Still, those are the machinations of “youngsters in a hurry” (Cornford 1908, 5), not the settled judgements of seasoned practitioners confident of their place in the profession.²⁰

That may seem a strong conclusion, but it has history on its side. Remember the equanimity with which the behavioral revolution was originally greeted by those then ruling the profession.

If the behavioral revolution’s main tenets are behavior, science, pluralism and system, then “traditionalists” had little reason to oppose it. Research on behavior at the individual levels was already being done in the 1930s and 1940s...—and those who did not do it had little

¹⁷ Likewise the movement for a “public sociology” (Burawory 2005).

¹⁸ As Barry (1970, 183) says, “there is no *intrinsic* advantage in mixing up opposed ideas... the result can easily be a muddle.”

¹⁹ A point appreciated by writers as diverse as Bohman (1999; 2002), Hay (2002), and Katzenstein and Sil (2008). This is also the official ideology, if not always practice, of the Perestroika movement: “this new political science would not be one that is dedicated to replacing one method with another. Instead, such a discipline... would encourage scholars to draw on a wide range of methods from a diversity of theoretical perspectives, combining theory and empirical work in different and reactive ways, all in dialogue with political actors in specific contexts” (Schram 2003, 837).

²⁰ Let me quote two. Russell Hardin (2006, 5) writes that “the small group who think there is a Methoden Streit that shakes many social scientists are largely ignored by the far—indeed, vastly—larger groups of active social scientists who more or less constructively pursue their highly varied methods. They are not shaken. They are not even stirred. And broadside dismissals will not bring them into this debate.” Gabriel Almond’s (1988, 840) phrase “separate tables” gave a name to a phenomenon he denies more than he decries; he insists that “mainstream political science is open to all methods that illuminate the world of politics and public policy. It will not turn its back on the illumination we get from our older methodologies just because it now can employ the powerful tools of statistics and mathematics.”

objection to those who did. The commitment to science was of long standing. . . . Pluralism as empirical theory was hardly new—indeed, the “latent theory of the traditionalists as . . . [a] ‘parallelogram of forces’ . . .” sounds a lot like pluralism. (Dryzek 2006b, 489–90)

Not until that revolution had been won was it seriously challenged, and even then in a way the mainstream studiously ignored. Leo Strauss’s vituperative “Epilogue” to Storing’s 1962 *Essays on the Scientific Study of Politics* constituted the challenge, but “the challenge was never officially accepted by the profession of political science.” Other political theorists (Schaar and Wolin 1963) were left to “come to the defense of political science.” And “political theorists . . . were left to squabble among themselves in their isolation from the discipline at large,” which proceeded basically to ignore “both the accusations that had been made against them and the proffered defense” (Saxonhouse 2006, 847–8).

“Multi-perspectival approaches” are the embodiment of the refusal to succumb to the demands of “either–or.”²¹ The fruitfulness of such approaches, and the willingness of members of the profession not merely to tolerate but to embrace them, is evinced across the ten-volume series of *Oxford Handbooks of Political Science*. Constructivists co-edit and coauthor with rationalist-realists (Reus-Smit and Snidal, this volume), critical theorists with post-structuralists (Dryzek, Honig, and Phillips, this volume), qualitative methodologists with quantitative (Box-Steffensmeier, Brady, and Collier, this volume); and all of them celebrate the synergies. Nowadays very few sophisticated philosophers or social scientists believe in covering-law positivism anymore (Moon 1975; Kitcher 1981; Hay 2002). But instead of throwing their hands up in despair, they turn to whole other disciplines that systematically map the many other contextual factors upon which political outcomes depend (Tilly and Goodin, this volume; cf. Flyvbjerg 2001 and Laitin 2003).

For a brief worked example of how such a multi-perspectival approach might work, consider the “new institutionalism.” Distinction-mongers divide that into multiple distinct “new institutionalisms” which they insist are incompatible in their fundamental epistemological and ontological assumptions: rational-choice, historical, constructivist, network (Rhodes, Binder, and Rockman 2006, chs. 2–5), discursive (Schmidt 2008). But it is not really all that hard to see coherent ways of synthesizing them all.

Of course, any attempt at synthesis has to start somewhere and in so doing will inevitably privilege some of those building blocks more than others. My own inclination is to start with a basically rational-choice account of intentional agents pursuing

²¹ And they are commonplace among the wisest of the rational-choice modelers, so often accused of being narrow-mindedly hegemonic. Fiorina (1996, 88–9) observes that “when NASA put astronauts on the moon . . . its scientists and engineers did not rely on a single overarching model. They relied on literally hundreds of models and theories . . . No single model would have accounted for more than a few aspects of the total enterprise . . . [Likewise] I teach my students that [rational choice] models are most useful where stakes are high and numbers low, in recognition that it is not rational to go to the trouble to maximize if the consequences are trivial and/or your actions make no difference. . . . Thus, in work on mass behavior I utilize minimalist notions of rationality . . . whereas in work on elites I assume a higher order of rationality.”

their projects through games of a slightly richer sort than ordinary game theory captures.²² Out of the interplay of those interactions, institutionalized solutions to their common problems emerge and acquire normative force among those who want to rely on those institutionalized solutions for future dealings (the constructivist and network institutionalist insight). Some branches of the extensive form of the game end sooner and less satisfactorily than others, with all possibilities for further development having been played out; in those cases we must either resign ourselves to making do with nonideal arrangements or face the prospect of a sharp and costly renegotiation of our settled practices (the historical institutionalist point). Often however we can simply shift among a plurality of different institutions governed by different norms and involving different players to address different problems we encounter (constructivist, discursive, and network institutionalisms again). In short, thinking how intentional goal-seeking agents might operate on and through history, developing shared norms and institutions as an aid to doing so seems to me a tolerably good synthesis of the many ostensibly “very different” strands of the new institutionalism (Goodin 1996; 2000; see similarly Knight 1992; Hay and Richards 2000; Orren and Skowronek 2004; Offe 2006; Hertting 2007; and most especially Olsen 2009).

That seems a good example of the potential fruitfulness of judicious combinations of ostensibly either–or approaches within political science.²³ Such multi-perspectival accounts can come from collaboration via interdisciplinary or multidisciplinary research teams (Moran 2006). Or they might come from hybrid “border-crossing” scholars who themselves sit at the intersection of multiple different disciplines and move easily between them (Dogon and Pahre 1990; Dogon 1996; Rudolph 2002). Or they might come from collaborations across different sub-disciplines within political science, or from collaborations across some ostensible “great divide” within the same sub-discipline.

2 WHERE WE’RE AT

2.1 Revolutions We Have Known

Academics thrill at the thought of their disciplines having been racked by a series of “revolutions” (Kuhn 1962). It is the great aim of every aspiring academic to be at the forefront of the next revolution.

²² Hay (2004) in contrast incorporates soft rational-choice style analyses within a broadly constructivist model, to address “what if” questions broadly after the fashion of Tetlock, Lebow, and Parker (2006) and Levy (2008).

²³ For others, see: Bendor and Hammond’s (1992) masterly blending of Allison’s (1971) three models; and attempts at blending rational choice and interpretivist approaches to culture by Bates et al. (1998) and Laitin and Weingast (2006; cf. Johnson 2002).

But of course revolutions rarely are quite as consequential as their advocates hope, or their opponents fear. Most things go on pretty much the same, on the other side of the revolution. Bismarck's social insurance legislation remained on the books under Hitler and afterwards in remarkably similar form in East and West Germany alike (US DHEW 1978). A revolution installs a new regime to which one is obliged to pay polite obeisance, and it genuinely gets in the way of some things you want to do. But generally you can work around it, to carry on much as before.

It is important to bear in mind not only how small the revolutionary avant-garde generally is, but also what a small proportion of the discipline are signed-up members of the ruling cadre even after a revolution has seemingly succeeded. As our International Benchmarking Panel reminded the UK Economic and Social Research Council:

However monolithic the US discipline may seem from a distance, those working within it know fully well that it is internally highly diverse. From a distance, the US discipline may seem to be dominated by some hegemonic practice—"behaviouralism" in the previous generation or "rational choice" in the present one. But in fact, those supposedly "hegemonic" practices are actually practiced to any high degree by only perhaps 5% of the US discipline, even in many top departments. (Goodin et al.²⁴ 2007, 9)

How "big" a revolution has to be to qualify as a revolution—how much of the territory it has to occupy, and just how much control it has to exercise over it—is a particularly open question when it comes to scientific revolutions (Dryzek 2006b, 487). I thus prefer to couch the next section's prognostications in terms of what might be the next "big thing," rather than the next "revolution." Still, the discipline's self-conception of its past is firmly organized around epochs punctuated by successful revolutionary takeovers, so let me begin by introducing the discipline in those, its own preferred terms.

According to the standard periodization, political science in the USA has been marked by three successful revolutions.²⁵ The first was that which founded the discipline at the very beginning of the twentieth century: the turn away from the dilettantish do-gooderism of the American Social Science Association and toward systematic and professionalized study of political processes. The second successful revolution shaping the US discipline was the behavioral revolution of the 1950s, a self-styled break away from the previous preoccupation with what is formally supposed to happen and toward how people actually behave politically. The third successful revolution shaping the US discipline was the rational choice revolution of the 1970s, promising a break from "mindless empiricism" and offering instead a tight set of theoretical propositions deduced from a Spartan set of fundamental assumptions.

Those are the "storylines" of the discipline, at least in the USA. Elsewhere, even in the anglophone world and certainly outside it, those revolutionary waves either

²⁴ My fellow members on that panel were James Der Derian, Kris Deschower, Friedrich Kratochwil, Audie Klotz, Brigid Laffan, Pippa Norris, B. Guy Peters, Joel Rosenthal, and Virginia Sapiro.

²⁵ Gunnell (2005) dissents in a way sufficiently contrived as to strike me as confirmation of the truth of that conventional wisdom.

came much later or passed political science there by altogether (Barry 1999). And even as regards the US discipline, those highlighted storylines are only part of the larger story. Much going on in each period was at best only loosely connected to (and much more was wholly apart from) the ostensibly dominant storyline. There were important subthemes and counterpoints in each period, some of which went on to form the basis of the next “revolutionary challenge.” Some sub-disciplinary and sub-subdisciplinary and cross-disciplinary projects proceeded largely impervious to the imperial ambitions of the latest successful revolutionary cadre and were largely ignored by it: Public and Constitutional Law has always rather like that.²⁶ Others were definitely on the radar of people working across many different fields: The “new institutionalism” was certainly like that, having been widely embraced as the “next big thing” across the discipline as a whole over the past decade or more (Goodin and Klingemann 1996b, 11).

More will be said shortly about each of these various movements that have washed over the discipline. But that highly synoptic characterization will suffice to set up the principal point that I want to make at this stage. That point is simply that, by virtue of that self-conception of its past, the discipline has come to acquire an accumulated “core” body of knowledge that must be mastered by aspirants to the profession.

Professionals specialize as well, of course. Facilitating that, and marshaling specialization in collectively fruitful ways, is the whole point of a profession. So in much of their own work, disciplinary professionals will inevitably be engaged in ever more narrowly focused enquiries into arcane corners of some niche or another.

There have been complaints about political science saying “more and more about less and less” (Corwin 1929, 569) for the best part of a century. One measure of that, Sigelman (2006a, v–vi) notes half-jokingly, is the increasing frequency of colons in the titles of articles found in the premier journals.

But while the work of the profession becomes increasingly “fragmented,” the profession as a whole need not. If there is a shared sense of what constitutes the core of the discipline (substantively even more than methodologically), and if professional training in the discipline insists upon mastering that as a condition of entry, then specialists however specialized have some common ground with one another. They have some sense, necessarily rudimentary, of where one another’s specialty fits within the large scheme that they share.

Again, not all places in the world practice the profession of political science in this way. And not all that do necessarily would adopt exactly, or even roughly, the same canon as the “common core” to be communicated to aspiring members of the profession. Still, in the top US (and, increasingly, UK) graduate schools of political science, entry to the profession requires aspirants to have mastered at least after a fashion the “scope and methods” of the discipline, and that they have done so in broadly similar ways to those in pretty much any other top political science program.

²⁶ As Appendix 1.5 below shows.

As we put it in the introduction to the *New Handbook of Political Science*:

Few of those trained at any of the major [US] institutions from the 1970s [or UK ones from the 1990s on, I would now add] will be unduly intimidated (or unduly impressed either) by theories or techniques from behavioral psychology, empirical sociology or mathematical economics. Naturally, each will have his or her own predilections among them. But nowadays most will be perfectly conversant across all those methodological traditions, willing and able to borrow and steal, refute and repel, as the occasion requires.

(Goodin and Klingemann 1996b, 13)

In his introduction to its centenary issue, the editor of the *American Political Science Review* begged to differ. From where Sigelman (2006b, 473) sat, our “warm-and-fuzzy image of an increased empathetic capacity among adherents of different approaches to political science . . . seems oddly out of touch with the experience of a discipline wracked by periodic culture wars (as manifested in the Caucus for a New Political Science of the 1960s and 1970s and the Perestroika movement of the last few years).” He saw the discipline as being more “characterized by something closer to an armed truce—an agreement to disagree—among true believers of different disciplinary creeds than to an active, congenial engagement in a joint enterprise.”

It is true that Klingemann and I did not see Perestroika coming. And from where I now sit I have to concede that, realistically, the profession probably has to undergo that sort of insurgency once every couple of generations, to remind itself of “things lost” over the course of its latest “revolution.” I can well understand why those in the firing line—beseiged editors of the *American Political Science Review* preeminently among them—might see those as veritable “culture wars.” But in academic politics as in real-world politics, trumped-up culture wars greatly exaggerate the depth of the disagreements and the extent of irreconcilability (Fiorina 2006). There is, I say again, no reason to accept the “either–or” straitjacket that cultural warriors would impose on us.

While I am sure some irreconcilables remain in each of the armed camps that Sigelman describes, I cannot help thinking that they are unrepresentative. The best talents in the profession are much less tempted to wallow in endless “meta” debates, and much more inclined to “just do it” (Dryzek 2005). The *Oxford Handbook of Contextual Analysis* (Goodin and Tilly 2006) is offered in evidence of just how productive it can be for mainstream political science to take those sorts of insights systematically on board, and as evidence of just how disciplined and systematic you can be in doing so—hopefully thereby allaying some of the mainstream’s deepest concerns with the latest insurgency.

2.2 The Canon

What, then, is the core of the canon that practicing political scientists need to master in order to have mastered the discipline?

Of course, political science—like all the natural and many other social sciences²⁷—is increasingly becoming an article-based discipline.²⁸ Some classic journal articles never grow into a book. Some whole debates are conducted on the pages of journals alone. And some whole subfields seem dominated by articles rather than books. Still, most lasting contributions to political science as a whole typically come in, or eventually get consolidated into, book form.²⁹

The canon of political science can, therefore, be reasonably described in terms of a set of “core books” with which any competent professional must have at least a passing acquaintance. Any selection of “must read” classics is inevitably somewhat idiosyncratic, and inevitably there will be disagreement at the margins. But the list offered in Appendix 1.1 would, I think, command a reasonably broad consensus.³⁰

I hasten to add that the Appendix 1.1 list is more by way of a report than of an evaluation. Those books are ones that are professionally prominent. Each of us might privately harbor doubts about just how good some of them really are. Still, those books truly are touchstones of the discipline, with which any serious practitioner needs to be competently acquainted.

There are other books that should be better known by political scientists than they actually are. I nominate a few of my personal favorites in Appendix 1.1, as well. Doubtless others will have their own. My list is offered in the spirit of “starting a conversation.” Once we acknowledge that there is indeed a canon of the sort described in the first table in Appendix 1.1, the next logical thing we should do is have a conversation about what else ought to be included in it.

Where ought one to watch for new developments? Well, of course, much good work bubbles away below the surface for many years before breaking into high-profile places. But eventually important new trends will (and to become important discipline-wide, will have to) break into one or another of the truly major journals and book series that are the “outlets of record” for the profession as a whole. Among the journals serving that function are the *American Political Science Review*, the *American Journal of Political Science*, and the *British Journal of Political Science* and a raft of top-flight sub-disciplinary journals (such as *International Organization*, *Political Analysis*, *Studies in American Political Development*, *Politics & Society*, and so on). Among the book series serving that discipline-shaping function are CUP series on

²⁷ Note however that when mounting his spirited defense of postwar British empirical sociology, Marshall (1990) focuses on books rather than articles.

²⁸ A good guide to the “articles that have shaped the discipline” is found in the commentaries on the twenty most-cited articles in the *American Political Science Review* published in its Centennial Issue (Sigelman et al. 2006). For the British equivalent see Dunleavy, Kelly, and Moran (2000) and occasional online updates: <<http://www3.interscience.wiley.com/journal/118510540/home>>. Dewan, Dowding, and Shepsle (2008a) offer a similar list of classic articles in rational choice approaches to political science.

²⁹ Often the original article is a good substitute for (Axelrod 1981; 1984; Allison 1969; 1971), or occasionally much better than (March and Olsen 1984; 1989), the ensuing book. It also sometimes happens that the earlier editions of a book are much superior to subsequent ones (Allison 1971; Allison and Zeikow 1999; Neustadt 1960; 1990).

³⁰ Notice, for example, the strong overlap between the similar list produced for the *New Handbook of Political Science* (Goodin and Klingemann 1996a, 15–17) and the list of “40 classic books in political science” produced for rather different purposes by Hammond, Jen, and Maeda (2007, 437–4).

“Political Economy of Institutions & Decisions,” “Cambridge Studies in International Relations,” “Cambridge Studies in Comparative Politics,” and the OUP “Oxford Political Theory” series.³¹ All these developments are also ably surveyed in the *Annual Review of Political Science*, which serves, in effect, as the “annual supplement” to reference books such as the present one.

2.3 The Cast

The main players of the discipline can be identified through a bibliometric analysis of reference patterns across the ten volumes of *Oxford Handbooks of Political Science*. This exercise is akin to one conducted for the *New Handbook of Political Science* (Goodin and Klingemann 1996b, 27–43). It differs in counting index entries to authors, rather than the frequency of an author’s appearance in reference lists alone.³²

There is much information on citation rates already available (Masuoka, Grofman, and Feld 2007).³³ Standard citation counts based on citations in the journals indexed by the Thompson Scientific/ISI Web of Science are problematic in various ways. Coverage of non-anglophone journals is patchy (but decreasingly so as the database is expanded). Conventional citation rates traditionally count only journal-to-journal citations in journals, thus ignoring citations in and even to books—although unnecessarily so, since journal-to-book citations can (with effort) be extracted from information already in ISI databases (Butler 2006). Ordinary citation analyses tell us much of interest, and they would tell us even more if they were further enhanced in those ways.

There are, nonetheless, two compelling reasons to go on to conduct a citation analysis of the ten-volume *Oxford Handbooks of Political Science*. One reason relates to the nature of the works cited. Citations within reference books are much more likely to be laudatory citations, recommendations that these are publications that people in the profession really need to read. And because *Handbook* authors have to survey large literatures in a small space, their citations have furthermore to be highly selective.

The 1 percent of political scientists appearing most frequently in indices to the *Oxford Handbooks of Political Science* are listed in appendices to this chapter.³⁴ First that is done sub-discipline-by-sub-discipline, based on the index of each constituent sub-disciplinary volume. Appendix 1.2 lists “sub-disciplinary leaders” thus construed.

³¹ In their editor’s own immodest view, best relegated to a footnote, the *Journal of Political Philosophy* is almost certainly the most interesting political theory journal for a general political science audience, and the CUP series of books on “Theories of Institutional Design” is akin to those others listed.

³² The latter strategy had the unfortunate effect, in the *New Handbook*, of treating authors identically whether they were referenced only once or multiple times in a chapter (Barry 1999, 452). The current strategy risks undercounting only insofar as authors are referenced multiple times on the same page.

³³ This, like most such studies, is however confined to scholars based in US departments of political science.

³⁴ And remember, those cited in the *Oxford Handbooks of Political Science* represent only a small fraction of all political scientists worldwide.

Table 1.1. Integration of the discipline

	Number of authors
Cited in 10 out of 10	7
9 out of 10	16
8 out of 10	20
7 out of 10	49
6 out of 10	93
Total	185

Appendix 1.3 then merges the indexes of all ten volumes of *Oxford Handbooks of Political Science*, to identify leaders of the discipline as a whole.

There is a second discipline-related reason to conduct a citation analysis on the ten volumes of *Oxford Handbooks of Political Science*. Doing so enables us to map the structure of the discipline. It allows us to assess how fragmented or integrated the discipline is, across its various sub-disciplines. Just how many people who are cited in one sub-discipline are also cited in others? And so on.

One measure of the extent to which the discipline as a whole is indeed well-integrated is the number of people whose work is used by several sub-disciplines in common. Following the *New Handbook of Political Science* (Goodin and Klingemann 1996a, 33–4), scholars whose names appear in more than half of the sub-disciplinary volumes of the *Oxford Handbooks of Political Science* are dubbed “integrators of the discipline.” Appendix 1.4 names names. But what we most need to know to assess the state of the discipline are the frequency counts reported in Table 1.1.

There we see that only a handful of people—seven authors, to be precise—are influential across literally every sub-discipline. But from Table 1.1 we can also see that a fair few authors impact on more than half the sub-disciplines. Those 185 authors represent over 2 percent of all authors mentioned across the ten volumes of *Oxford Handbooks of Political Science*. Judged that way, it seems that the various sub-disciplines of political science do indeed have quite a few touchstones in common.

A second way of assessing the structure of the discipline is to map linkages between each of the sub-disciplines. We can do that by counting how often the scholars who appear in the index of one sub-disciplinary handbook also appear in the indexes of each of the other sub-disciplinary handbooks.³⁵ The larger the proportion of shared reference points, the “closer” one sub-discipline can be said to be to another. And the more other sub-disciplines that are “close” to it, the more “central” the sub-discipline can be said to be to the discipline as a whole.

³⁵ Admittedly, each sub-discipline is represented by a single *Oxford Handbook*; and a different set of editors would have chosen to emphasize different topics tapping different literatures. Still, each of those volumes contains chapters written by over fifty different people. Counting the overlaps between the references that two sets of fifty people employ is probably a fairly good basis upon which to assess linkages among the various sub-disciplines of political science.

Table 1.2. The structure of the discipline

	Law and pol.	Pol. econ.	Methodology	Pol. behavior	Comp. pol.	Pol. instns.	Context	Public policy	Internat. rela.	Theory
Law and pol.	—	*			**	**				
Pol. econ.		—	**	*	***	***	*	*	*	
Methodology			—	**	***	**			*	
Pol. behavior				—	****	**	**	*		
Comp. pol.					—	***	**	**	**	
Pol. instns.						—	**	***	**	
Context							—	*	**	*
Public policy								—	*	*
Internat. rela.									—	*
Theory										—

Notes: **** >25% authors shared; *** >20%; ** >15%; * >10%.

The percentage of authors whose names appear in one sub-disciplinary volume’s index also appear in the other’s is reported in Appendix 1.5.³⁶ Table 1.2 represents those data in more summary manner. The more stars, the more shared references there are between the two sub-disciplines represented by that cell in Table 1.2.³⁷

As we see from Table 1.2, the “core” of the discipline consists in Comparative Politics and Political Institutions. Political Behavior, Political Economy, and Political Methodology strands feed heavily into those. Law and Politics is connected to the Comparative Politics and Political Institutions core on the Political Economy side. Sub-disciplines of Public Policy and International Relations are connected to the Comparative Politics and Political Institutions core on the Contextual side. Political Theory dangles off the end of that latter cluster and is only very weakly connected to any of the rest of political science.

3 WHAT NEXT?

What is likely to be “the next big thing” to hit political science?³⁸ It is hard to say, of course. As Humphrey Lyttelton famously said of jazz, if we knew where political science was going we would be there already (Winch 1958, 94).

³⁶ Appendix 1.5 reports two slightly different numbers for each handbook pair. (The difference arises from the fact that the same number of overlapping authors is divided by the different total number of authors referenced in each of the handbooks concerned.) For purposes of Table 1.2, I simply average across those two numbers.

³⁷ I am grateful to Kieran Healy and Simon Niemeyer for advice on how this material might best be presented.

³⁸ I am indebted to Lee Sigelman for putting me in mind of this question.

What I can say, with a confidence born of reflection on the past bubbles that have punctuated the history of our discipline, is that the basic intellectual materials out of which the next big thing will be constructed already exist among us. The next big thing is something most of us will have bumped up against, something most of us will have mentioned in passing in writing or lectures. It is something we currently regard as an interesting curiosity, fitting awkwardly our current way of seeing things and doing business but not (as yet) occasioning any fundamental rethinking of them.

Having long been familiar with the next big thing as a minor curiosity, it will strike us as odd when a bubble suddenly arises around it, inflated by aspiring stars anxious to rise. Deeming it a perfectly worthy point or practice in its place, most of us will vaguely resent the exaggerated cure-all claims that will be made for the new snake oil. But that will be the modal tendency of a distribution whose two tails will inevitably go to war with one another, trying to force the sensible middle into taking sides.³⁹ Thus it was with the behavioral revolution, with systems theory and structural-functionalism, with rational choice, and so on.⁴⁰

3.1 The Nature of “Big Things”

Those are the sorts of things I mean by a “big thing.” They are not nearly so domineering as a *Weltanschauung* or a Kuhnian paradigm, necessarily, nor so overwhelming as a “revolution.” “Big things” certainly set a research agenda and focus attention on “critical points” (Morgenstern 1972) or “unresolved problems” (Elster 1979, ch. 3) within it.⁴¹

In social science, a “big thing” is a simple idea that promises to pack a big explanatory punch, explaining much on the basis of a little. That, of course, is just what philosophers of science have long told us we ought be looking for: an explanation that is powerful and parsimonious at one and the same time (Carnap 1950, 3–8; Quine 1961, 16–17; Kitcher 1981).

“Big things” claim wide application, offering ways of reconceiving the discipline as a whole, or anyway some large portion of it. They promise something like “A Common Language for the Social Sciences” (the motto of the Harvard Department of Social Relations (Geertz 2000, 8)) or “a long-term program of scholarly activity which aims at no less than the unification of theory in all fields of the behavioral sciences” (as Talcott Parsons pitched it to Harvard’s Faculty Committee on

³⁹ Merton (1973, ch. 3). The most vitriolic complaints come from those left behind: Straussians venting their spleen against then-orthodox behavioralism (Storing 1962; cf. Schaar and Wolin 1963); Green and Shapiro (1994; cf. Friedman 1996) venting theirs against the rational choice orthodoxy, after Mancur Olson’s review panel berated their Yale department for having missed that boat.

⁴⁰ I limit my attention to post-Second World War developments, although there are interesting stories well told by others of developments earlier. See, e.g.: Easton, Gunnell, and Stein (1995); Farr, Dryzek, and Leonard (1995); Farr and Seideman (1993); Heilbron, Magnusson, and Wittrock (1998); Wagner, Wittrock, and Whitley (1991); Collini, Winch, and Burrows (1983).

⁴¹ They can, and often do, do that more in the spirit of “tying up loose ends” in the research program than of building protective belts to insulate it from falsification, necessarily (Quine 1961, 43; Kuhn 1962; Lakatos and Musgrave 1970).

Behavioralism (quoted in Rudolph 2005, 8)). David Truman pitched the behavioral revolution to the Social Science Research Council in similar terms, as have done countless advocates of other would-be “big things” in countless other venues.⁴²

Alongside and within “big things” there are also many “medium-sized things”—“good gimmicks” (Mackenzie 1967, 111), tricks, tools, and mechanisms (Elster 1989; 2007; Hedström and Swedberg 1998), “theories of the middle range” (Merton 1968, ch. 2), and so on. These are loci of disciplinary feeding frenzies all their own. Recent examples include the new institutionalism (March and Olsen 1984; 1989) and ideas of social capital (Putnam 1993; 2000), path dependency (Pierson 2000; 2004), and deliberative democracy (Dryzek 2000). Those serve as ancillary theories, tricks or tools that can be mixed-and-matched with a variety of other things.⁴³

Medium-sized things sometimes mushroom into big things, staging a takeover bid for the discipline as a whole. That happens as several cognate medium-sized things consolidate into “one big thing.” Rational choice, for example, became a “big thing” in the 1970s by consolidating various previously unconnected strands: operations research in defense (Hitch and McKean 1960), game theory in international relations (Schelling 1960), and notions of “public goods” (Olson 1965) and “median voters” (Downs 1957). As late as 1967, so shrewd an observer as Mackenzie (1967, ch. 9) failed to foresee that consolidation coming, regarding all these instead as simply separate (albeit related) “partial theories” or “good gimmicks.” Or for another example, the behavioral revolution consolidated realist skepticism about actual adherence to formal rules and ideal standards across several domains—constitution-writing (Beard 1913), policy-making (Bentley 1908/1967), public administration (Simon 1947, ch. 2), local governance (Lynd and Lynd 1929), international relations (Morgenthau 1948)—together with new experimental and observational methodologies and statistical techniques for assessing their findings (Merriam 1921; Key 1954). So too with all the other “big things” that have come and gone over the years: All have been conglomerations of medium-sized things with which we have long been familiar.

Finally, there are always research programs with “big thing” ambitions that have not (yet) succeeded in achieving hegemonic status across the profession as a whole. Political culture was one such, psychoanalytics another, biopolitics yet another. Although they once aspired to be more (and may still do), at least for now they remain middle-sized things available for mixing-and-matching with others. Likewise, failed “big things” never completely disappear but merely revert to the status of

⁴² As Truman wrote in the 1951 SSRC newsletter, “Political behavior is not and should not be a specialty, for it . . . aims at stating all the phenomena of government in terms of the observed and observable behavior of men. To treat it as a ‘field’ coordinate with (and presumably isolated from) public law, state and local government, international relations, and so on, would be to defeat its major aim. That aim includes an eventual reworking and extension of most of the conventional ‘fields’ of political science” (quoted in Dahl 1961a, 767). Biopolitics similarly aspires to “effect a major transformation in political scientists’ views and . . . instead of becoming a larger subfield of the discipline . . . disappear by total incorporation into it, as Christianity (to use Somit’s metaphor) was enveloped by and incorporated into the Roman Empire” (Wahlke 1986, 872).

⁴³ Such medium-sized things predominate among the “advances in the social sciences” catalogued in Deutsch, Markovits, and Platt (1986; Deutsch 1979).

middle-sized things in the professional firmament, true so far as they go but definitely only part of the overall story.

3.2 The Next Big Thing: A Job Description

When scanning the horizon for the next big thing, it behooves us to recall the distinctive features of things that have formerly risen to become “big things.”

3.2.1 *Simplicity*

“Big things” are, first and foremost, fundamentally simple ideas. They are capable of being expressed succinctly—stated in just a few words—yet they have wide ramifications.⁴⁴ Think of the catchcry of the behavioral revolutionaries: “don’t just look at the formal rules, look at what people actually do.” Think of the catchcry of structural-functionalists: “form fits function.”⁴⁵ Think of the catchcry of systems theorists: “everything is connected; feedback matters.” Think of the catchcry of the rational choice revolutionaries: “always remember, people pursue power and interest.”⁴⁶ Think of the catchcry of new institutionalists: “institutions matter.” Simple ideas, all.

3.2.2 *Broad Application*

“Big things” must have wide application to politics, across the board. That is why “consolidation” of medium-sized projects into “one big thing” is so important. To travel far, however, “big things” need to travel light: They need to be easily adaptable to the wide variety of circumstances and settings to which they aspire to apply. The same basic idea can, and should, play out differently in different contexts, without sacrificing its claim to simplicity and parsimony at some more fundamental level.

3.2.3 *Formalizable*

“Big things” must also admit of formalization of some sort or another.⁴⁷ That need not necessarily take the form of propositional logic or higher mathematics, as with rational choice theory. In the case of structural-functionalism, formalization amounted to little more than an elaborate conceptual schema and a few diagrammatic techniques borrowed from structural anthropology (Merton 1968, ch. 3). Proper input–output analysis and linear programming came later (Forrester 1971; Meadows

⁴⁴ The same feature characterizes what come to be regarded as a “great book that everyone must read” in political science (Barry 1974, 80) and, indeed, as Nobel-worthy economics (Alt, Levi, and Ostrom 1999, xvi).

⁴⁵ With apologies to the Bauhaus movement.

⁴⁶ With apologies to Shapiro (1999).

⁴⁷ Arguably the behavioral and rational choice revolutions were both method driven. The “next big thing” in political science might be similarly methodological—agent-based modeling or experimental or quasi-experimental methods or Bayesian rather than frequentist statistics. All these are surveyed at length in excellent chapters in the *Oxford Handbook of Political Methodology* (Box-Steffensmeier, Brady, and Collier 2008).

et al. 1972; cf. Shubik 1971*b*; Cole et al. 1973), but the early application of systems theory to political science consisted just in boxes connected by arrows (Easton 1957; 1965). So too with the early behavioral revolutionaries: Fancier techniques came later, but their initial formalism amounted to little more than cross-tabulations and chi-squares (Miller 1996, 301). Still, a “big thing” must always admit of formalization, be it heavy or light.

3.2.4 *Familiarity*

Like stars of the cinema, things that have subsequently become “big things” have generally been around for quite a while before they hit it big.⁴⁸ The raw materials of any “big thing”—what will subsequently come to be seen as its “classic texts”—were published long before it became big. Rational choice theory became a “big thing” in the mid-1970s (Sigelman 2006*b*, 469), but by then its seminal texts were all a decade or two old (Arrow 1951; Black 1958; Downs 1957; Riker 1962; Olson 1965). Likewise, by the time the behavioral revolution became a “big thing” in the 1950s (Dahl 1961*a*, 766; Sigelman 2006*b*, 469), its seminal texts were similarly antiquated (Merriam and Gosnell 1924; Tingsten 1937; Lazarsfeld, Berelson, and Gaudet 1944), and the intellectual materials out of which behavioralists built their models of politics were drawn from psychology of several decades previously (Lipset et al. 1954).⁴⁹ Similarly, the materials out of which post-Second World War structural-functionalists built their models were drawn from interwar anthropologists (Radcliffe-Brown 1935; Malinowski 1936/1976). And the materials out of which the new institutionalists of the 1980s built their models were drawn from decades-old organization theory and behavioral theories of the firm (Simon 1947; March and Simon 1958; March 1962; 1996; Cyert and March 1963).

3.2.5 *Marginality*

Finally, the materials out of which political science constructs its own “next big thing” tend to be leftover fragments from other disciplines.⁵⁰ From the point of view of political scientists, that makes it look like a takeover bid by some other discipline. But from the point of view of that other discipline, the raiders more often look like renegade bands operating at the margins of their home discipline.⁵¹ The rational choice revolution in political science was spearheaded by scholars of social choice and of public finance, both far from the center of gravity of mainstream

⁴⁸ When leaders of the new revolution claim, with ostensive modesty, to be merely “standing on the shoulders of giants” (Merton 1965), what they are often really doing is claiming for themselves a lineage.

⁴⁹ As Hardin (2006, 4) remarks, “the [behavioral] movement had passed its peak in psychology when it became a dominant theoretical stance in political science.”

⁵⁰ One wave of the future might be “intradisciplinarity,” blending contributions from several of the discipline’s increasingly differentiated sub-disciplines. For example, work on the European Union (and on multiple overlapping sovereignties more generally) blends Comparative Politics and International Relations (Moravcsik 1998; H eritier 2007; Olsen 2007). “Comparative political theory” blends Comparative Politics and Political Theory (Dallmayr 1999; 2004; Euben 1999).

⁵¹ Perhaps because marginal scholars are, or are seen to be, more creative (Dogana and Pahre 1990).

economics.⁵² The behavioral revolution in political science drew on social psychology, which again is a somewhat marginalized subfield within psychology proper. And political science borrowed the structural-functionalist model from sociology and anthropology at just the time those disciplines were repudiating it (Radcliffe-Brown 1949; Davis 1959; cf. Merton 1968, ch. 3).

3.3 Candidates for the Next Big Thing

I offer the following shortlist of candidate “next big things” with hesitation, knowing it is inevitably incomplete and that it almost certainly omits what will eventually win out. Still, interviewing several candidates is often the best way find out what job we really want to have done for us.

3.3.1 *Framing Models*

The basic idea of framing models is that of “choice under description” (Davidson 1980; 1984). Objects of choice always display a literal infinity of attributes. In choosing one thing over another, we focus in on some of those attributes whilst ignoring all the rest. We choose one under some description, picking out some features we see as particularly salient. Different frames lead to different choices, so shifting or imposing frames is an exercise of power: of the choosing agent, if done consciously and autonomously; of psychology over rationality, if it happens less consciously; of one person over another, if it happens less autonomously.

Part of the appeal of framing models is that they will provide a way of taking account of at least some of the crucial contextual effects to which Perestroikans point (Rudolph 2005). There are many ways in which “context matters,” of course (Goodin and Tilly, this volume), and how we frame situations is only one; still, it looks like it might be a central one, connecting micro and macro. Another part of their appeal is that, in helping us see the choice situation from the actor’s own point of view, framing models answer to the hermeneutic, interpretivist impulse.⁵³

Within political science and political sociology, there is already a relatively rich array of forebears for this potential “next big thing” to build upon. Distinguished examples include: Allison’s classic discussion of conceptual maps in connection with the Cuban Missile Crisis (Allison 1971; Allison and Zeitkow 1999); classic discussions of ways in which rhetoric and media frame political perceptions and shape reactions

⁵² Judging from the “press release” under each of their entries in the archives of the Nobel Foundation (Royal Swedish Academy of Science 2005), Arrow’s Nobel was awarded much more for his work on general equilibrium theory than for his impossibility theorem, and Sen’s for his work on poverty measures and on famines as much as for his work on social choice. Only Buchanan’s Nobel was awarded principally for work in work on “public choice” of the sort that the rational choice revolution brought to political science.

⁵³ Summarizing one of the “lessons” of his dissertation, off which he says he has been living ever since, Geertz (2000, 6) writes: “To discover who people think they are, what they think they are doing, and to what end they think they are doing it, it is necessary to gain a working familiarity with the frames of meaning within which they enact their lives.” See further: Garfinkel 1967; Geertz 1973; Taylor 1985, volume 1, chs. 1, 3–4; Skinner 2002, volume 1.

(Gamson 1992; Riker 1986; Edelman 1988; Iyengar 1994; Chong and Druckman 2007); and more recent discussions of framing within social movements (Benford and Snow 2000), with echoes all the way back to Arendt's *Origins of Totalitarianism* (1951).

In adjacent disciplines, framing effects are central to the surprising findings of experimental economics and psychology. What these experiments systematically show is that people will differ dramatically in their reaction to objectively identical choice situations ("objectively identical," the sense that the same material effects will ensue from the same choices) depending just on the way the options are described and the choice thereby framed: whether as "saving lives" or "letting people die;" whether as a lost theater ticket or a lost bank note in the same value; whether as a gain or a loss, depending on how you describe the baseline; whether as being an issue of fairness or ordinary market behavior (Camerer 2002; Fehr and Fischbacher 2002; Gth, Schmittberger, and Schwarze 1982; Kahneman 2003; Kahneman, Knetsch, and Thaler 1986; Kahneman, Slovic, and Tversky 1982; Kahneman and Tversky 2000; Thaler 2000).

In trying to formalize notions of framing, one approach might be to extend Axelrod's (1973; 1976) work on the "structure of decision," mapping (literally: graphically) how people's beliefs hang together based on a close reading of texts. Another approach might be to work on the evidence in experimental psychology and experimental economics, trying to taxonomize the instances found there of framing and trying to tease out some generalizations about the various mechanisms that might lie behind them.

If people's decisions really are highly subject to framing and contextual effects, then we should obviously build that fact into our explanations of their actions and choices. We should do so with regret, however: normatively, because framing effects distort rationality and invite manipulation; empirically, because of the fundamental incompleteness of framing models. They must always be supplemented with some other account of what (or psychological forces of pattern recognition) causes people to see things one way rather than another. That crucial part of the explanation must come from outside the framing model itself.⁵⁴

3.3.2 *Evolutionary Models*

The basic idea of evolutionary models is that society takes the shape it does because over time its elements have been subject to a repeated process of "selection for fitness" (Elster 1989, ch. 8). The elements subject to the selection mechanism can be genotypes, productive practices, game strategies, or whatever. The selection mechanism might involve differential reproductive success of biological organisms, differential bankruptcy rates of firms, social practices persisting or fading away, and so on (Witt 1993).

Part of the appeal of evolutionary models is that they offer a particularly rich account of dynamic aspects of social life. Furthermore, they do so by reference to

⁵⁴ And notice: it cannot stop with a story about other people manipulating you so as to see it that way; the question then arises as to how they came to see *their* choices in the way they did, leading them into that manipulation.

structural- or systemic- rather than individual-level mechanisms. For those who are suspicious of models of individual choice as the fundamental driver of social stability and change, these are great attractions.

Within political science, there is a long tradition of thinking in broadly biopolitical terms. Some of those precedents, Social Darwinism and sociobiology most conspicuously, are unhappy ones (Dryzek and Schlosberg 1995); others make minimal reference to evolutionary dynamics as such.⁵⁵ But at least some of those models of biopolitics serve as important precursors to a systematic application of evolutionary modeling within political science (Somit and Peterson 2001; Alford and Hibbing 2008).

The greatest impetus for evolutionary models in political science, however, probably lies in analyses of the “evolution of cooperation” in both experimental games and social settings. It was a “first” for our profession when Axelrod and Hamilton’s (1981, 1396 n. 19) article in the prestigious journal *Science* referred readers for the proofs to the *American Political Science Review* (Axelrod 1981). Subsequent work by Axelrod (1986), Ostrom (1990), and myriad scholars following in their footsteps offer us important insights into “the evolution of institutions” for social cooperation and collective action.

Looking beyond our own discipline, economists have long noted the structural similarities between the equilibria reached as a great many selfish economic agents seek simultaneously to maximize their utility and the equilibria reached as a great many selfish genes seek simultaneously to maximize their inclusive fitness and hence survival (Hirschleifer 1977). In adapting evolutionary models to politics, we might therefore also turn to Nelson and Winter’s classic 1982 *Evolutionary Theory of Economic Change* or to Bowles’s work on evolutionary economics, combining experimental economics and anthropology with institutional factors (Bowles 2004; Bowles and Gintis, this volume).

Familiar models from population biology are an obvious, elaborate, and intricate source of off-the-shelf technologies for formalizing evolutionary models imported into other disciplines (Nowak 2006). Just how appropriate the borrowing is depends upon just how apt the analogy is between the biological and other borrowing discipline (more of which below). But there is a major industry working on the project.

Evolutionary models, like functional ones before them, must above all avoid reducing themselves to empty tautologies by saying “that something must be fit in some way simply because it exists. . . . Most evolutionary theorists in biology or social science would accept” that notions of “fitness” must be given independent specification and selection mechanisms must “be specified in some detail . . . if evolutionary theorizing is to explain anything” (Nelson 1994, 115).

Evolutionary modeling sometimes involves only a very loose analogy to processes modeled in evolutionary biology. Evolution of a Darwinian sort implies a very

⁵⁵ “Most writers on ‘biopolitical’ subjects,” Wahlke (1979, 25) reports, “draw more heavily on modern ethology than on evolutionary biology per se,” focusing on notions like “territoriality, dominance, submission and other concepts borrowed from biological disciplines” (Wahlke 1986, 871). Compare, e.g., Mackenzie (1967, ch. 11) and Masters (1989).

precise set of mechanisms: random mutation, natural selection, genetic transmission. Social science models purporting to be evolutionary often lack one or more of those features.

Sometimes there is nothing analogous to random mutation in the model. Thus for example models of “deterministic models employing complex nonlinear dynamic equations” are sometimes called “evolutionary” (Anderson, Arrow, and Pines 1988). Reflective evolutionary modelers, however, rightly insist that “the term ‘evolutionary’ is reserved for models that contain both systematic [selection] and random [stochastic] elements” (Nelson 1994, 114).

Social scientists typically call their models “evolutionary” primarily by virtue of the fact that some “selection process” is involved in producing a stable equilibrium, as in evolutionary game theory (Selten 1991; Skyrms 1996; Samuelson 1997; Binmore 1998). But the unit of selection and the mechanism of transmission are importantly different in those social applications compared to biological ones. The strategies played are sometimes treated as if they were themselves the players, and strategies in successive games as analogous to successive “generations” in biological selection. But of course what they really are, are successive choices of the same player who learns over time—which is more of a Lamarckian matter of transmission of acquired characteristics.

“The hallmark of standard biological evolutionary theory is,” of course, “that only the genes, not any acquired characteristics or behavior, get passed on across the generations” (Nelson 1994, 116). Models of “cultural evolution” thus conspicuously differ in that crucial respect (Masters 1989; McElreath and Boyd 2007). Stories about cultural evolution concern the transmission of “memes” intragenerationally and intergenerationally, with replicator dynamics leading to evolutionary stable strategies.⁵⁶ This process is “conditioned by human biology but with cumulative force of its own” (Nelson 1994, 118).⁵⁷

These models of “cultural evolution” are “tied to the genetical theory of natural selection no more than . . . to epidemiology” (Sober 1994, 479). They “do not attempt to specify the particular evolutionary mechanisms and ‘cultural fitness’ criteria operative. . . . Thus,” Nelson (1994, 119)—himself a distinguished evolutionary economist—concludes, “these extensions . . . do not really come to grips with the kinds of evolutionary processes with which economists or other social scientists . . . have been concerned.”

Before they can borrow with confidence the tools of evolutionary biology, social scientists thus have to assure themselves that the mechanisms at work are genuinely analogous in all respects that really matter. Until that has been done, “the biological concept of natural selection plays the role of a suggestive metaphor, and nothing more” (Sober 1994, 480).

⁵⁶ Maynard Smith’s (1982) notion of an “evolutionary stable strategy” assumes organisms are pre-programmed to play one strategy, and evolutionary dynamics weeds ones programmed to play “wrong” strategies out of successive generations of the population, whereas Dawkins’s (1976) model of “replicator dynamics” works through organisms each of which is choosing among various strategies.

⁵⁷ For example, “if characteristics were transmitted by parents teaching their children, a selection process could occur without the mediation of genes” (Sober 1994, 479).

3.3.3 *Network Models*

The basic idea behind network-based models is that of decentered governance. These are models with no strong central authority, where power is widely dispersed among many disparate agents who coordinate with one another via “partisan mutual adjustment” (Lindblom 1965). “Networks” are the communication channels—the “nerves” in Deutsch’s (1963) old phrase—along which that business is transacted (Ansell 2006; Rhodes 2006).

Much of the appeal of these models lies in the fact that so much of the real political world is precisely like that. That is obviously so in the case of international relations, where there is no central authority: governance is inevitably via networking among sovereign states (Keohane 1984; 2001; Slaughter 2003). It is obviously so in weak federations, like the European Union, where the crab-dance of multiple overlapping sovereignties gives rise to notions like “subsidiarity” (Føllesdal 1998; van Kersbergen and Verbeek 2004) and “the open method of coordination” (Vandenbroucke 2002; Offe 2003; Olsen 2007, chs. 1, 5)—just as they did in pre-state Europe (Berman 1983; Spruyt 1994). But savvy observers know that even in “proper” states with notionally strong central authorities, the process of government involves an endless series of negotiations and networking with stakeholders with the power to exact tribute (Hecló 1978; Lehmbuch 1984; Rhodes 1988). Prudent authorities would naturally want to avoid showdowns with them, echoing the sentiments of Shaw’s (1934, 1025) King Magnus: “Naturally I want to avert a conflict in which success would damage me and failure disable me.”

Many of the sources upon which network models borrow are of course rooted in anthropology and sociology (Boissevain 1974; Mitchell 1974). Mark Granovetter’s (1973; 1983) classic paper on “The strength of weak ties” would without doubt top anyone’s reading list on the subject. But there is also a rich tradition within political science of thinking about matters in this way, as already alluded to. All the works on “the governmental process” (Bentley 1908/1967; Truman 1971) and the “iron triangles” and private-interest networking that are involved within it (Hecló 1978; Laumann and Knoke 1987; Heinz et al. 1993; Useem 1979; 1984) obviously serve as crucial professional touchstones for any subsequent elaboration of network-based models of politics. These models might also draw on studies of, for example, how “comity” used to work to bind Congressmen together (Matthews 1959; Uslaner 1991), how “epistemic communities” shape public policy (Haas 1992), how global power is mobilized (Grewal 2008), and how nongovernmental organizations mobilize transnational support for their causes (Keck and Sikkink 1998; Risse, Ropp, and Sikkink 1999).

Several “how-to” manuals have already been prepared for network analysis (Boissevain and Mitchell 1973; Burt and Minor 1983; Scott 1991; Wasserman and Faust 1994). And given its anthropological roots, much network analysis is inevitably going to be essentially ethnographic. But if it is genuine “formalization” we are after, the place to look is mathematical “graph theory.” No doubt there remain more resources still to be tapped. But as an illustration of how graph-theoretic techniques can be put to social-scientific use, ponder Laumann and Pappi’s (1976) microdescription of

a small German town or Heinz and Laumann's (1982) mapping of networks among lawyers in Chicago.

Therein lies the rub, of course. Can those formalizations prove useful beyond small-scale networks, and if they cannot just how much insight into political life can they offer? The answer might still be "lots," if the governing elite is as small as some people suppose; but it might well be "not much," even if we live in only a moderately pluralist political world (of more than, say, a few hundred important actors).

3.3.4 *An End to Big Things?*

To ask "what will be the next big thing?" implies that there will be one. That begs a question that some hope and others believe is firmly closed in the negative. Perestroikans and postmodernists more generally, committed to a plurality of ways of understanding the world, suppose that the time of monolithic "grand narratives" and totalizing, hegemonic research programs is at an end (Monroe 2005; Rudolph 2005). Their view of "big things," like the Church of England's view of miracles, is that those are "something that had occurred in the past but could hardly be expected to happen nowadays" (Barry 1970, 1).

Sometimes that proposition is put as a sociological hypothesis, other times as a philosophical necessity. Sociologically, with the "fracturing of modernity" it becomes increasingly difficult to imagine ever finding one simple, overarching analysis capable of unifying all the fractured parts (Wagner 2008). Philosophically, world-making is seen as a process of social construction, with unlimited scope for human creativity and hence fundamentally unpredictable variability in social forms and practices.⁵⁸ Either alone, still more both together, make it difficult or impossible ever to come up with some single, unified "big thing" accounting for all social phenomena.

Those tempted by "big thing" ways of thinking, being of a more *a posteriori* cast of mind, will be undeterred by a priori demonstrations of the impossibility of their project. Their motto is, once again: "just do it" (Barry 1970, v; Dryzek 2005). If some "big thing" emerges that actually fills the bill, then the point is proven: The actual is *ipso facto* possible.

A raft of academic-political forces also push social science in "big thing" directions. "Big things" are hostile takeover bids, whereby outsiders seek to wrest control of the profession as a whole from its current management. The "next big thing" is the currency in which Young Turks bid to displace the Old Guard (Cornford 1908, 8; Moran 2006). It is the currency of department-builders and funding institutions trying to carve out a central rather than merely a niche role for themselves.⁵⁹

"Big things" are also the terms in which whole disciplines compete with one another for power and influence. Economics is not so much queen of the social

⁵⁸ A position anticipated in Popper's *Poverty of Historicism* (1964) and at the forefront of today's "social epistemology" (Antony 2006).

⁵⁹ As the SSRC did with the behavioral revolution and the Ford Foundation did with area studies (Dahl 1961a, 764–5).

sciences as its hedgehog: It knows “one big thing” (Berlin 1953).⁶⁰ And as Shaw has his Prime Minister Proteus saying to cabinet a propos their struggle with the sovereign, “One man that has a mind and knows it can always beat ten men who havnt and dont” (Shaw 1934, 1016). If that is why economics has so much more influence than the other social sciences, then the other social sciences will need to find some “one big thing” of their own in order to compete with the hegemony of economics in the public sphere.

The competitive logic of individual careerism also drives “big thingism.” Tempted though we may be to assimilate academic fads to mass frenzies like the South Sea Bubble and the Tulip Craze (Mackay 1841/1980), consider a couple of more rational sides to the story. In a world of imperfect and asymmetric information, price can serve as an indicator of quality. Other people know something you do not, and that is reflected in the higher price they are willing to pay or to demand for certain goods (Akerlof 1970; Stiglitz 1987). Further suppose the good in question is a “status good,” requiring you to invest more heavily than your competitors in order to win (Shubik 1971a; Hirsch 1976). Academic distinction and the rewards of educational attainment are both famously like that (Boudon 1974; Bourdieu 1984); investments in the “next big thing,” professionally, might be likewise, with the highest status rewards going to those who invest most. (Think of political methodologists investing ever more heavily in ever more rarified statistical techniques.) In that sort of scenario, everyone gets locked into a bidding war, with everyone investing ever more heavily in the good in question.

Alternatively (or perhaps equivalently), think of investing intellectual capital in the “next big thing” as akin to investing political capital in rising political stars. Bandwagons arise, arguably, from payoff structures, which derive in turn from the marginal increase in the probability of the candidate’s winning caused by that supporter’s joining the bandwagon. The marginal returns to the first comers are low, rising rapidly as support grows, and then drop off precipitously for late-comers (Brams 1978, ch. 2). Assume that payoffs (cabinet posts, good ambassadorships, in the case of a nominating convention; credit for having been central to the “revolution,” in the case of academic disciplines) are proportional to contributions. It then follows that prudent people will want to get on board quick-smart once a bandwagon is really rolling, within the profession just as at a nominating convention.

“Big things” will always be with us, therefore—for better or worse. But what does it matter? Just how big a deal are those ostensibly “big things” in the workaday world of practicing political scientists? Are they ever more than merely loose “moods” (Dahl 1961a, 766) or “persuasions” (Eulau 1963)?

Clearly, for the committed cadre in the vanguard of the new revolution, they are a very big deal indeed. Their entire careers are oriented toward elaborating, extending, and advocating their particular “big thing.” But the vast majority of political scientists

⁶⁰ And sociology knows three: Marx, Weber, and Durkheim.

whose main concerns lie elsewhere are generally nonplussed. They do obeisance to the reigning “big thing” in their opening paragraphs, but then they get down to business in pretty much the same way they would have done under any alternate regime.

Easy examples are found in structural-functionalism, which served (among political scientists anyway) largely just as a set of coding categories to facilitate comparison of “political systems differing radically in scale, structure and culture” (Almond 1960, 4; Mackenzie 1967, 317–21).⁶¹ In *The Politics of the Developing Areas*, for example, Almond’s introduction set out some functional categories and Coleman made light use of them in framing his conclusion; but those categories hardly impinged at all on the rest of the contributors, for whom they provided mostly just section headings and a framework for organizing material that, substantively, would have been very much the same if written to any other brief. Or for another example, notice how little “systems theory” actually impinges on Eisenstadt’s fascinating account of *The Political Systems of Empires* (1963).

The same is true of all the other “big things” that have come along from time to time. Mid-century studies of Congress and the Presidency wore their behavioralism lightly: They were typically told in the manner of “contemporary history” without recourse to any of the technical apparatus of the behavioral sciences. Come the rational choice revolution, studies of Congress, the executive, voting, and such like began gesturing toward rational choice frameworks in their opening pages, but often without any very deep impact on the way the subsequent studies proceeded. Or again, after a cursory nod toward new institutionalism, contemporary studies typically just get down to the business of describing the workings of the institution at hand, without further reference to that or any other overarching theory.

So do “big things” not really matter? Certainly not as much as some hope, and others fear. Still, just as hypocrisy is the tribute that vice pays to virtue, so too might paying obeisance to some shared “big things” be the tribute that parochialism pays to professionalism.

“Big things” serve as a lingua franca—or for those more dismissively inclined, a pidgin—facilitating conversation across the disparate sub-disciplinary communities within the profession.⁶² They help us see why something that matters to others maybe ought also matter to us, by showing us ways in which the two might be connected. They are what “bind us together into an intellectual community” (Stinchcombe 1982, 10).

⁶¹ The same was true among sociologists (Parsons 1951: 3; cf. Barry 1970, 166). Structural-functionalists justified their functional specifications by reference to “system maintenance” at a pretty high level of abstraction, and analysis of any very particular practice or piece of behavior in those terms became terribly contrived and question-begging (Barry 1970, chs. 4, 8). See e.g. Birnbaum’s (1955) critique of Shils and Young (1953).

⁶² Milner (1998) for example comments on how rational choice institutionalism brought the American Politics and International Relations subfields into conversation with one another, in a way that had previously been rare.

4 DISCIPLINES WE HAVE KNOWN

Readers can judge for themselves the state of contemporary political science, after having perused the following chapters. Those constitute less a comprehensive survey than a select sample of what is currently under way within the discipline.

Still, even that small sample suffices—to my mind, at least—to illustrate both the unity and the diversity of contemporary political science. Much good work—interestingly different, yet powerfully complementary—is under way. Synergies abound. There is a genuine sense of excitement at forging research collaborations across long-standing divides. Different sub-disciplines are borrowing from, and contributing to, one another in ways and to extents not seen for years. Rejecting the illogic of “either–or,” scholars are crafting judicious blends of different methodologies to give richer and more nuanced analyses of important political phenomena that are easily distorted or obscured when viewed through a single methodological lens alone.

What has made all this progress possible, I submit, is not any loosening of the discipline of political science. Rather, that progress is attributable to the strength of the discipline’s discipline. If we were all freed from the discipline of the discipline merely to “go off and do our own thing,” then very few of these extraordinarily fruitful collaborations would have come about. It is the obligation to talk together, the sense of shared purpose, of shared histories and shared touchstones in the professional literature, that brings political scientists—however different their particular concerns and approaches—together to read, critique, and profit from one another’s work. The discipline is a pluralist one, but the plurality is contained within and disciplined by a discipline.⁶³ Diversity is a healthy attribute of a gene pool—but only if the carriers of those diverse genes actually interbreed. So too in the talent pools constituting an academic discipline (Aldrich, Alt, and Lupia 2008).

APPENDICES

Appendix 1.1 The Canon of Political Science

Table A1.1.1 lists classics of political science, divided by periods. The first column contains items first published prior to the watershed *American Voter* (Campbell et al. 1960). The second column items published between that and the seminal Greenstein–Polsby *Handbook of Political*

⁶³ Schram (2003, 837), describing the Perestroika vision of a “new political science,” pauses at the work “discipline” to query “if that word is still appropriate.” Perestroikans, I submit, were wildly wrong and wickedly pernicious ever to cast doubt on that.

Table A1.1.1. Classic texts in political science

Pre-1960	1960–75	1976–96	Post-1996
Arendt, <i>Origins of Totalitarianism</i> (1951)	Allison, <i>Essence of Decision</i> (1971)	Alt and Shepsle (eds.), <i>Perspectives on Positive Political Economy</i> (1990)	Acemoglu and Robinson, <i>Economic Origins of Dictatorship and Democracy</i> (2006)
Arrow, <i>Social Choice and Individual Values</i> (1951)	Almond and Verba, <i>Civic Culture</i> (1963)	Axelrod, <i>Evolution of Cooperation</i> (1984)	Boix, <i>Democracy and Redistribution</i> (2004)
Black, <i>Theory of Committees and Elections</i> (1958)	Barry, <i>Political Argument</i> (1965)	Barnes and Kaase, <i>Political Action</i> (1979)	Bueno de Mesquita et al., <i>Logic of Political Survival</i> (2005)
Dahrendorf, <i>Class and Class Conflict in Industrial Society</i> (1959)	Burnham, <i>Critical Elections and the Mainsprings of American Politics</i> (1970)	Bueno de Mesquita and Lahman, <i>War and Reason</i> (1992)	Cox, <i>Making Votes Count</i> (1997)
Downs, <i>Economic Theory of Democracy</i> (1957)	Campbell et al., <i>American Voter</i> (1960)	Cox and McCubbins, <i>Legislative Leviathan</i> (1993)	Dryzek, <i>Deliberative Democracy and Beyond</i> (2000)
Duverger, <i>Political Parties</i> (1951)	Campbell and Stanley, <i>Experimental and Quasi-Experimental Designs for Research</i> (1963)	Esping-Andersen, <i>Three Worlds of Welfare Capitalism</i> (1990)	Keck and Sikkink, <i>Activists beyond Borders</i> (1998)
Easton, <i>The Political System</i> (1953)	Dahl, <i>Who Governs?</i> (1961)	Fenno, <i>Home Style</i> (1978)	Krehbiel, <i>Pivotal Politics</i> (1998)
Hartz, <i>The Liberal Tradition in America</i> (1955)	Deutsch, <i>Nerves of Government</i> (1963)	Fiorina, <i>Retrospective Voting in American National Elections</i> (1981)	Lijphart, <i>Patterns of Democracy</i> (1999)
Key, <i>Politics, Parties and Pressure Groups</i> (1942)	Edelman, <i>The Symbolic Uses of Politics</i> (1964)	Hood, <i>Tools of Government</i> (1983)	McAdam, Tarrow, and Tilly, <i>Dynamics of Contention</i> (2001)
Key, <i>Southern Politics in State and Nation</i> (1950)	Ferejohn, <i>Park Barrel Politics</i> (1974)	Inglehart, <i>The Silent Revolution</i> (1977)	Moravcsik, <i>Choice for Europe</i> (1998)
Luce and Raiffa, <i>Games and Decisions</i> (1957)	Hirschman, <i>Exit, Voice and Loyalty</i> (1970)	Jennings and Niemi, <i>Generations and Politics</i> (1981)	Pogge, <i>World Poverty and Human Rights</i> (2002)
March and Simon, <i>Organizations</i> (1958)	Huntington, <i>Political Order in Changing Societies</i> (1968)	Jervis, <i>Perception and Misperception in International Politics</i> (1976)	Russett and Oneal, <i>Triangulating Peace</i> (2001)
Mills, <i>Power Elite</i> (1956)	Kaufman, <i>The Forest Ranger</i> (1960)	Kaase, Newton, and Scarbrough, <i>Beliefs in Government</i> (1995)	Scott, <i>Seeing Like a State</i> (1997)
Morgenthau, <i>Politics among Nations</i> (1948)	Lane, <i>Political Ideology</i> (1962)	Katznelson, <i>City Trenches</i> (1981)	Tsebelis, <i>Veto Players</i> (2002)
Schumpeter, <i>Capitalism, Socialism and Democracy</i> (1943)	Lijphart, <i>Politics of Accommodation</i> (1968)	Keohane, <i>After Hegemony</i> (1984)	Wendt, <i>Social Theory of International Politics</i> (1999)
Seiznick, <i>TVA and the Grassroots</i> (1949)	Lipset, <i>Intelligence of Democracy</i> (1965)	King, Keohane, and Verba, <i>Designing Social Inquiry</i> (1994)	
Simon, <i>Administrative Behavior</i> (1947)	Lipset and Rokkan, <i>Party Systems and Voter Alignments</i> (1967)	Kymlicka, <i>Multicultural Citizenship</i> (1995)	
Waltz, <i>Man, the State and War</i> (1959)	Mayhew, <i>Congress: The Electoral Connection</i> (1974)	Lindblom, <i>Politics and Markets</i> (1977)	
	Moore, <i>Social Origins of Dictatorship and Democracy</i> (1966)	Lipsky, <i>Street Level Bureaucracy</i> (1980)	
	Neustadt, <i>Presidential Power</i> (1960)	March and Olsen, <i>Rediscovering Institutions</i> (1989)	
	Nozick, <i>Anarchy, State and Utopia</i> (1974)		

(cont.)

Table A1.1.1. (Continued)

Pre-1960	1960-75	1976-96	Post-1996
	<p>Olson, <i>Logic of Collective Action</i> (1965)</p> <p>Pateman, <i>Participation and Democratic Theory</i> (1970)</p> <p>Piven and Cloward, <i>Regulating the Poor</i> (1971)</p> <p>Rawls, <i>Theory of Justice</i> (1971)</p> <p>Riker, <i>Theory of Political Coalitions</i> (1962)</p> <p>Schattschneider, <i>Semi-sovereign People</i> (1960)</p> <p>Schelling, <i>The Strategy of Conflict</i> (1960)</p> <p>Wildavsky, <i>The Politics of the Budgetary Process</i> (1964)</p> <p>Wolin, <i>Politics and Vision</i> (1960)</p>	<p>Olson, <i>Rise and Decline of Nations</i> (1982)</p> <p>Orren, <i>Belated Feudalism</i> (1991)</p> <p>Ostrom, <i>Governing the Commons</i> (1990)</p> <p>Pateman, <i>Sexual Contract</i> (1988)</p> <p>Peterson, <i>City Limits</i> (1981)</p> <p>Popkin, <i>The Reasoning Voter</i> (1991)</p> <p>Przeworski and Sprague, <i>Paper Stones</i> (1986)</p> <p>Putnam, <i>Making Democracy Work</i> (1993)</p> <p>Riker, <i>Liberalism against Populism</i> (1982)</p> <p>Rogowski, <i>Commerce and Coalitions</i> (1989)</p> <p>Schelling, <i>Micromotives and Macrobehavior</i> (1978)</p> <p>Segal and Spaeth, <i>Supreme Court and the Attitudinal Model</i> (1993)</p> <p>Skocpol, <i>States and Social Revolution</i> (1979)</p> <p>Skocpol, <i>Protecting Soldiers and Mothers</i> (1992)</p> <p>Skowronek, <i>Politics Presidents Make</i> (1993)</p> <p>Sniderman, Brody, and Tetlock, <i>Reasoning and Choice</i> (1991)</p> <p>Wildavsky, <i>Speaking the Truth to Power</i> (1979)</p> <p>Wilson, <i>Politics of Regulation</i> (1980)</p> <p>Young, <i>Justice and the Politics of Difference</i> (1990)</p> <p>Zaller, <i>Nature and Origins of Mass Public Opinion</i> (1992)</p>	

Science (1975). The third column items published between that and the *New Handbook of Political Science* (Goodin and Klingemann 1996a). The final column items published since then.

The lists in the four columns differ in length, not so much because some periods were more fruitful than others, but merely because they represent different lengths of time. The fact that the list in the last column is particularly short does not necessarily suggest that political science is running out of steam. That column covers the shortest period of time; and the list there is further truncated by fact that it takes time to see whether books, however good, actually start getting picked up by the profession at large.

There are other books, often already famous within their home discipline, that political scientists should know better than they do. Among those I would nominate are those listed in Table A1.1.2.

Table A1.1.2. Books political scientists should know better

Akerlof, <i>An Economic Theorist's Book of Tales</i> (1984)
Bates, de Figueiredo, and Weingast, <i>Analytical Narratives</i> (1998)
Berman, <i>Law and Revolution</i> (1983)
Braithwaite and Drahos, <i>Global Business Regulation</i> (2000)
Camerer, <i>Behavioral Game Theory</i> (2002)
Coleman, <i>Foundations of Social Theory</i> (1990)
Geertz, <i>The Interpretation of Cultures</i> (1973)
Hall and Soskice, <i>Varieties of Capitalism</i> (2001)
Sen, <i>Poverty and Famines</i> (1981)

Appendix 1.2 Sub-disciplinary Leaders

“Sub-disciplinary leaders” are defined as the 1 percent of people (ignoring pre-twentieth century authors) whose names appear most frequently in the index of the respective sub-disciplinary volume in the *Oxford Handbooks of Political Science* series.⁶⁴

Conventional citation counts typically disregard self-citations. But it would be wrong to do so here. Authors were chosen to write for the *Oxford Handbooks of Political Science* because they are leaders in their field. Anyone writing a chapter on that topic would have referenced their work frequently, albeit perhaps not quite so frequently as they sometimes reference themselves. (In none of the cases listed below does any substantial proportion of the author’s references come from self-referencing.)

I have therefore opted to include self-references in the counts reported in Table A1.2.1, and simply to flag the fact that the count might be inflated somewhat by setting an “A” against the count for people who authored a chapter in one of the volumes. Similar issues might arise with volume editors and with me as General Editor of the series; I flag that fact by setting an “E” against the count for any editor of the volume in question, and a “GE” against my name.

⁶⁴ References to the collective McNollgast are attributed to the individual authors, McCubbins, Noll, and Weingast.

Table A1.2.1. Sub-disciplinary leaders

Top 1%	Number of entries	Top 1%	Number of entries
<i>Political theory</i> (1,403 individuals cited)			
J. Rawls	86	H. Arendt	23
J. Habermas	50	B. Barry	23
M. Foucault	48	L. Strauss	23
I. M. Young	36	J. Waldron	22
R. Dworkin	35	S. Wolin	22
W. Kymlicka	34	<i>The next few</i>	
C. Taylor	30	J. Derrida	21
S. Benhabib	29	J. Dryzek	21 (E, A)
W. E. Connolly	28 (A)	J. Raz	21
D. Miller	24 (A)		
<i>Political institutions</i> (1,730 individuals cited)			
A. Lijphart	26	M. McCubbins	15
G. Cox	22	M. Duverger	14
R. A. W. Rhodes	22 (E, A)	P. Pierson	14
R. E. Goodin	20 (GE)	W. Riker	14
P. Hall	20	K. Strøm	14
J. P. Olsen	20 (A)	<i>The next few</i>	
G. Tsebelis	20	S. A. Binder	13 (E)
R. Keohane	19	G. Stoker	13 (A)
L. Martin	18 (A)	J. Braithwaite	12 (A)
B. G. Peters	16	C. Hay	11 (A)
K. Shepsle	16 (A)	G. B. Powell	11
J. M. Colomer	15 (A)		
<i>Law and politics</i> (1,693 individuals cited)			
M. Shapiro	40 (A)	M. A. Graber	17 (A)
J. A. Segal	34 (A)	L. A. Kornhauser	16 (A)
B. Weingast	34	C. R. Epp	15 (A)
L. Epstein	28 (A)	G. N. Rosenberg	15
H. Gillman	25 (A)	S. Silbey	15 (A)
J. Ferejohn	26	A. Stone Sweet	15
H. Spaeth	26 (A)	<i>The next few</i>	
K. E. Whittington	24 (E, A)	F. B. Cross	14 (A)
G. Caldiera	23 (E, A)	W. N. Eskridge, Jr.	14
M. McCann	21 (A)	J. Knight	14
R. Posner	20	M. McCubbins	14
A. Sarat	20	R. H. Pildes	14 (A)
S. Scheingold	20 (A)	C. R. Sunstein	14
R. Hirschl	18 (A)		
<i>Political behavior</i> (1,956 individuals cited)			
R. Inglehart	78 (A)	H.-D. Klingemann	44 (E, A)
R. Dalton	60 (E, A)	P. Converse	38 (A)
S. Verba	54	R. Rose	36 (A)
R. Putnam	53	W. Miller	28
P. Norris	50 (A)	P. M. Sniderman	27 (A)

(cont.)

Table A1.2.1. (Continued)

Top 1%	Number of entries	Top 1%	Number of entries
<i>Political behavior (cont.)</i>			
J. L. Gibson	26 (A)	M. Franklin	20
R. Huckfeldt	26 (A)	S. Huntington	20
W. Mishler	23	M. Kaase	20 (A)
D. Fuchs	22 (A)	<i>The next few</i>	
C. Welzel	22 (A)	S. Barnes	19
S. M. Lipset	21	A. Blais	19 (A)
J. Sprague	21	O. Knutsen	19 (A)
R. A. Dahl	20	J. Stimson	19 (A)
<i>Contextual political analysis (2,056 individuals cited)</i>			
C. Tilly	39 (E, A)	F. A. Polletta	11 (A)
C. Geertz	21	P. Bourdieu	10
C. Ginzburg	21	J. Goldstone	10
J. C. Scott	18	S. Jasanoff	10 (A)
T. Parsons	16	R. Keohane	10
W. Sewell	16	W. E. Bijker	9 (A)
P. Pettit	14 (A)	W. Gamson	9
S. Tarrow	14	A. Giddens	9
S. Verba	13	J. Habermas	9
G. A. Almond	12	T. Kuhn	9
P. Pierson	12	J. Mahoney	9 (A)
A. Wendt	12	T. Skocpol	9
D. McAdam	11	J. Wajcman	9 (A)
<i>Comparative politics (1,778 individuals cited)</i>			
A. Przeworski	57 (A)	R. M. Duch	23
C. Boix	39 (E, A)	J. A. Robinson	23
C. Tilly	32 (A)	T. Skocpol	23
G. Cox	30	A. Lijphart	22
H. Kitschelt	29 (A)	R. Stevenson	22
S. Verba	29	M. Laver	21
W. Riker	28	S. Tarrow	21 (A)
S. Stokes	27 (E, A)	<i>The next few</i>	
S. M. Lipset	25	D. Laitin	20
M. Shugart	25	D. C. North	19
B. R. Weingast	24	J. Ferejohn	18 (A)
<i>International relations (1,219 individuals cited)</i>			
R. O. Keohane	60 (A)	R. G. Gilpin	23
K. N. Waltz	48	P. Katzenstein	23 (A)
A. Wendt	40	D. S. Snidal	23 (E, A)
H. Bull	39	<i>The next few</i>	
A. Linklater	33	J. S. Nye, Jr.	22 (A)
C. Reus-Smit	33 (E, A)	D. Campbell	21
J. G. Ruggie	33	E. H. Carr	20
H. J. Morgenthau	28	A. Moravcsik	20 (A)
J. J. Mearsheimer	26	K. Sikkink	20 (A)

(cont.)

Table A1.2.1. (Continued)

Top 1%	Number of entries	Top 1%	Number of entries
<i>Political economy</i> (1,775 individuals cited)			
B. R. Weingast	57 (E, A)	K. Krehbiel	24 (A)
A. Alesina	49	J. Snyder	24
G. W. Cox	45 (A)	A. Downs	23
M. D. McCubbins	45 (A)	P. Ordeshook	23
K. A. Shepsle	39 (A)	T. R. Palfrey	23 (A)
J. Ferejohn	29	J. D. Fearon	22 (A)
R. McKelvey	29	<i>The next few</i>	
T. Perrson	28 (A)	J. Roemer	21 (A)
G. Tabellini	28 (A)	H. Rosenthal	21
D. Wittman	27 (E, A)	D. Austen-Smith	20 (A)
W. H. Riker	25	M. Laver	20 (A)
J. Buchanan	24 (A)	A. Przeworski	20 (A)
<i>Public policy</i> (1,986 individuals cited)			
A. Wildavsky	43	M. A. Hajer	16 (A)
J. G. March	33 (A)	E. Bardach	15 (A)
R. E. Goodin	31 (E, A, GE)	G. Esping-Andersen	15
J. P. Olsen	29 (A)	J. Pressman	15
M. Rein	29 (E, A)	R. A. Dahl	14
D. Schön	28	C. E. Lindblom	14
J. Forester	27 (A)	P. A. Sabatier	14
F. Fischer	24	<i>The next few</i>	
G. Majone	24 (A)	R. Neustadt	13
H. Heclo	22	J. W. Kingdon	12
C. Hood	22 (A)	M. Lipsky	12
H. D. Lasswell	22	C. Pollitt	12
B. G. Peters	19	J. C. Scott	12
R. A. W. Rhodes	18 (A)	H. Wagenaar	12
P. de Leon	16 (A)	R. J. Zeckhauser	12 (A)
<i>Political methodology</i> (1,642 individuals cited)			
G. King	36	J. Mahoney	17 (A)
N. Beck	33 (A)	A. George	16
D. Collier	29 (E, A)	S. Jackman	16 (A)
C. C. Ragin	29 (A)	D. P. Green	15 (A)
H. E. Brady	28 (E, A)	R. O. Keohane	15
C. H. Achen	22	T. Skocpol	15
J. Box-Steffensmeier	22 (E, A)	S. Verba	15
A. Bennett	21 (A)	<i>The next few</i>	
D. B. Rubin	21	J. N. Katz	14
L. M. Bartels	18	D. Campbell	13
B. Jones	18 (A)	D. A. Freedman	13
G. Goertz	17 (A)	J. M. Snyder	13

Appendix 1.3 Leaders of the Discipline

“Leaders of the discipline” are defined as the 1 percent of people (ignoring pre-twentieth century authors) whose names appear most frequently in the indices of the ten volumes of *Oxford Handbooks of Political Science* taken as a whole.

The same convention as in Appendix 1.2 is employed here, with an “A” indicating someone who authored a chapter in the volume in question; an “E” indicates someone who served as editor of the volume; and “GE” indicates the General Editor of the series as a whole.

The extent to which these results might be skewed by differences in citation practices across different sub-disciplines can be ascertained from Table A1.2. There is some difference across sub-disciplines in the frequency with which the very most frequently mentioned author is mentioned. But focusing on the last person among the 1 percent most referenced, that person gets around twenty mentions in all the established sub-disciplines. (The only exception is the less-established field of “Contextual Political Analysis.”)

Table A1.3. Leaders of the discipline

Top 80 (~1%) Number of entries	Top 80 (~1%) cont.	Top 80 (~1%) cont.	The next few
B. Weingast 138 (E, A)	J. G. March 65 (A)	B. M. Barry 47	N. Beck 40
R. Keohane 134 (A)	A. Alesina 64	R. Dworkin 47	C. Reus-Smit 40 (E, A)
J. Rawls 132	D. North 62 (A)	M. Fiorina 47	C. Taylor 40
S. Verba 124	A. Wendt 62	M. Duverger 46	I. Budge 39 (A)
G. W. Cox 109 (A)	H. E. Brady 61 (E, A)	H. Kitschelt 46 (A)	P. Katzenstein 39 (A)
R. Ingelhart 109 (A)	S. P. Huntington 59	S. Krasner 46	P. Sinderman 39 (A)
A. Przeworski 107 (A)	J. P. Olsen 59 (A)	M. Shapiro 46 (A)	I. M. Young 39
R. Dahl 98	K. N. Waltz 59	K. Sikkink 46 (A)	L. M. Bartels 38
J. Habermas 95	S. Tarrow 58 (A)	A. George 45	J. S. Dryzek 38 (E, A)
W. H. Riker 93	P. A. Hall 57	J. G. Ruggie 45	C. Geertz 38
R. D. Putnam 91	M. Olson 57	J. L. Gibson 44 (A)	R. Hardin 38 (A)
M. McCubbins 89 (A)	A. Wildavsky 57	W. E. Miller 44	W. Kymlicka 38
A. Lijphart 88	D. Collier 55 (E, A)	L. L. Martin 43 (A)	H. Rosenthal 38
C. Tilly 88 (E, A)	P. E. Converse 55 (A)	J. Elster 43	G. Sartori 38
T. Skocpol 85	C. Boix 54 (E, A)	R. Franzese 37 (A)	B. Simmons 38 (A)
M. Foucault 84	H.-D. Klingemann 54 (E, A)	J. J. Linz 43	J. M. Snyder 38
G. A. Almond 82	R. Rose 54 (A)	G. Tsebelis 43	J. Aldrich 37 (A)
J. Ferejohn 82 (A)	K. Strøm 54 (A)	J. A. Robinson 42 (A)	R. Axelrod 37
K. A. Shepsle 81 (A)	M. Shugart 53 (A)	G. Esping-Andersen 42	S. Benhabib 37
G. King 75	J. A. Segal 51 (A)	H. D. Lasswell 42	J. M. Buchanan 37 (A)
R. Dalton 74 (E, A)	J. C. Scott 50	P. Ordeshook 42	B. Bueno de Mesquita 37 (A)
P. Norris 71 (A)	T. Persson 50 (A)	D. Snidal 42 (E, A)	D. Diermeier 37 (A)
P. Pierson 71	G. Tabellini 50 (A)	C. Achen 41	J. S. Nye, Jr. 37 (A)
J. Fearon 69	D. Laitin 49 (A)	H. Bull 41	N. Schofield 37
S. M. Lipset 68	G. B. Powell 49 (A)	R. McKelvey 41	
A. Downs 68	J. Mahoney 48 (A)	J. A. Stimson 41 (A)	
R. E. Goodin 66 (GE, E, A)	C. C. Ragin 48 (A)		
M. Laver 65 (A)	A. Bennett 47 (A)		

Appendix 1.4 Integrators of the Discipline

“Integrators of the discipline” are defined as people whose work is influential across multiple branches of the discipline (Goodin and Klingemann 1996*b*, 33–4). Table A1.4 lists people whose name appears in the indexes of multiple volumes of the *Oxford Handbooks of Political Science*, according to the number of those ten volumes in which it appears.

Again, in deference to fears that self-referencing might have affected the results, an “E” indicates someone who edited one of the volumes and “GE” the General Editor of the series as a whole.

Appendix 1.5 Structure of the Discipline

This appendix maps relations among the sub-disciplines of political science, as represented by each of the ten component volumes of the *Oxford Handbooks of Political Science* series. The “closeness” of one sub-discipline to another will be adjudged by the proportion of authors who appear in the name indexes of both sub-disciplinary volumes.

Pre-twentieth-century authors are once again ignored. So too are the names of public figures not cited as authors (US Supreme Court justices whose written opinions are cited are treated as authors, however). It is sometimes unclear whether people with the same name are the same author or not; for those cases I have adopted the rule “when in doubt, count.”

In Table A1.5, the total number of entries in the name index of each sub-disciplinary volume is reported below the name of the sub-discipline in the row and column headings. In each cell in the body of that table, two numbers appear: First, the total number of individuals whose name appears in the indexes of both volumes; and second, that expressed as a percentage of all names that appear in the handbook represented by that row. Since the total number of names indexed in different sub-disciplinary handbooks differs (the Context volume has 70 percent more names in its index than the one on International Relations), the two such percentages reported for the intersection of each pair of handbooks differs somewhat in the top-right and lower-left halves of Table A1.5.

The final column in Table A1.5 averages the interrelationships across the row representing the sub-discipline. From those averages, it would seem that—except for Political Theory and Law and Politics—the discipline as a whole is pretty uniformly integrated. Except for those two sub-disciplines, the average interrelationship between sub-disciplines ranges between 12 and 18 percent, with Comparative Politics and Political Institutions appearing as most central on average.

Table A1.4. Integrators of the discipline
 Authors appearing in multiple subdisciplinary volumes (omitting pre-twentieth-century authors)

<i>Influence 10 out of 10 sub-disciplines</i>	<i>Influence 7 out of 10 sub-disciplines</i>	<i>Influence 6 out of 10 sub-disciplines</i>	<i>Influence 6 out of 10 (cont.)</i>
G. A. Almond	K. J. Arrow	B. Ackerman	J. J. Mansbridge
R. A. Dahl	P. Bachrach	T. Adorno	R. McKelvey
D. Laitin	R. H. Bates	J. E. Alt	R. K. Merton
A. Lijphart	P. Bourdieu	R. M. Alvarez	J. W. Meyer
J. Rawls	J. M. Buchanan	C. Anderson	R. Niemi
W. H. Riker	I. Budge	L. Anderson	J. S. Nye, Jr.
T. Skocpol	T. Carothers	H. Arendt	C. Offe
<i>Influence 9 out of 10 sub-disciplines</i>	J. S. Coleman	U. Beck	P. C. Ordeshook
B. Barry	J. Dewey	P. L. Berger	K. Orren
H. E. Brady (E)	L. Diamond	I. Berlin	E. Ostrom
J. Elster	D. Easton	B. Bueno de Mesquita	T. Persson
D. P. Green	H. Eckstein	A. Campbell	K. T. Poole
P. A. Hall	G. Esping-Andersen	F. G. Castles	D. Rae
R. Hardin	J. Fearon	D. Collier (E)	C. Ragin
S. P. Huntington	E. Fehr	R. J. Dalton (E)	J. E. Roemer
R. O. Keohane	M. Fiorina	D. Diermeier	S. Rokkan
S. D. Krasner	F. Fukuyama	A. Downs	R. Rose
D. C. North	W. A. Gamson	G. W. Downs	J. G. Ruggie
M. Olson	A. Giddens	J. N. Druckman	F. Scharpf
P. Pierson	J. H. Goldthorp	P. B. Evans	E. E. Schattschneider
A. Przeworski	J. R. Hibbing	R. Fenno	T. C. Schelling
I. Shapiro	A. O. Hirschman	M. Finnemore	T. Schwartz
S. Verba	R. Inglehart	M. Foucault	J. A. Segal
B. R. Weingast	D. Kahneman	M. Friedman	C. R. Shipan
<i>Influence 8 of 10 sub-disciplines</i>	T. S. Kuhn	G. Garrett	B. Simmons
R. Axelrod	H. D. Lasswell	B. Geddes	N. J. Smelser
J. Ferejohn	M. Levi	C. Geertz	J. McC. Smith
C. J. Friedrich	A. Lupia	A. Gramsci	J. Sprague
R. E. Goodin (E, GE)	C. W. Mills	B. Grofman	J. E. Stiglitz
J. Habermas	B. Moore	T. R. Gurr	A. L. Stinchcombe
J. D. Huber	A. Moravcsik	D. Held	D. E. Stokes
P. Katzenstein	R. A. Posner	S. Holmes	G. Tabellini
G. King	G. B. Powell	E. Huber	C. Taylor
S. M. Lipset	W. W. Powell	G. Jacobson	K. Thelen
M. D. McCubbins	R. Rogowski	R. Jervis	D. B. Truman
T. Parsons	G. Sartori	R. A. Kagan	A. Tversky
R. Putnam	N. Schofield	T. L. Karl	E. M. Uslaner
C. Reus-Smit (E)	J. A. Schumpeter	R. S. Katz	J. L. Walker
P. C. Schmitter	J. C. Scott	M. Keck	I. Wallerstein
A. K. Sen	K. Sikkink	V. O. Key	M. Walzer
K. A. Shepsle	D. Snidal (E)	M. Laver	M. P. Wattenberg
H. A. Simon	J. Snyder	S. Levitsky	B. D. Wood
J. D. Stephens	D. Soskice	C. E. Lindblom	O. R. Young
G. Tsebelis	A. Stepan	J. J. Linz	J. Zaller
A. Wildavsky	C. R. Sunstein	T. Lowi	
	S. Tarrow	N. Luhmann	
	E. Theiss-Morse	S. Lukes	
	C. Tilly (E)	M. B. MacKuen	
	G. Tullock	G. Majone	

Table A1.5. The structure of the discipline (number and % of shared authors)

	Comp. pol. 1778	Pol. instns. 1730	Inter. rels. 1219	Pol. econ. 1775	Meth. 1642	Pol. beh. 1956	Context 2056	Public pol. 1986	Law & pol. 1693	Pol. th. 1403	Ave.
Comp. pol. 1778	—	404 = 22.72	211 = 11.87	516 = 29.02	390 = 21.93	506 = 28.46	348 = 19.57	255 = 14.34	204 = 11.47	134 = 7.54	18.55
Pol. instns. 1730	404 = 23.35	—	225 = 13.01	397 = 22.95	253 = 14.62	331 = 19.13	268 = 15.49	414 = 23.93	324 = 18.73	142 = 8.21	17.60
Inter. rels. 1219	211 = 17.31	225 = 18.46	—	150 = 12.31	190 = 15.59	127 = 10.42	233 = 19.11	192 = 15.75	139 = 11.40	163 = 13.37	14.86
Pol. econ. 1775	516 = 29.07	397 = 22.37	150 = 8.45	—	310 = 17.46	268 = 15.10	228 = 12.85	215 = 12.11	181 = 10.20	92 = 5.18	14.76
Meth. 1642	390 = 23.75	253 = 15.41	190 = 11.57	310 = 18.88	—	312 = 19.00	232 = 14.13	187 = 11.39	122 = 7.43	77 = 4.69	14.03
Pol. beh. 1956	506 = 25.87	331 = 16.92	127 = 6.49	268 = 13.70	312 = 15.95	—	326 = 16.67	236 = 12.07	137 = 7.00	130 = 6.65	13.48
Context 2056	348 = 16.93	268 = 13.04	233 = 11.33	228 = 11.09	232 = 11.28	326 = 15.86	—	294 = 14.30	162 = 7.88	202 = 9.82	12.39
Public pol. 1986	255 = 12.84	414 = 20.85	192 = 9.67	215 = 10.83	187 = 9.42	236 = 11.88	294 = 14.80	—	201 = 10.12	176 = 8.86	12.14
Law & pol. 1693	204 = 12.05	324 = 19.14	139 = 8.21	181 = 10.69	122 = 7.21	137 = 8.09	162 = 9.57	201 = 11.87	—	128 = 7.56	10.49
Pol. th. 1403	134 = 9.55	142 = 10.12	163 = 11.62	92 = 6.56	77 = 5.49	130 = 9.27	202 = 14.40	176 = 12.54	128 = 9.12	—	9.85

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