



THE
OXFORD
HANDBOOKS
OF
POLITICAL
SCIENCE

GENERAL EDITOR
ROBERT E. GOODIN


EDITED BY

ROBERT E.
GOODIN

≡ The Oxford Handbook of
**POLITICAL
SCIENCE**

THE OXFORD HANDBOOK OF

POLITICAL SCIENCE



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GENERAL EDITOR: ROBERT E. GOODIN

The *Oxford Handbooks of Political Science* is a ten-volume set of reference books offering authoritative and engaging critical overviews of all the main branches of political science.

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ROBERT E. GOODIN

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PREFACE

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Every book has a history. This one's begins with the XVIth World Congress of the International Political Science Association in Berlin, way back in 1994. I was scheduled to be its Program Chair; that was going to be a lot of work; I decided I'd be damned if the program booklet itself was all I'd have to show for my efforts. So Hans-Dieter Klingemann and I carved out a stream of "State of the Discipline" panels designed (with a few nips and tucks here and there) to feed into *A New Handbook of Political Science*, eventually published by Oxford University Press in 1996. That book did well for OUP. Indecently well, apparently. OUP editors ever since have been under orders to commission several such handbooks each year—doubtless cursing us as they do, for launching the handbook industry.

Publication of the *New Handbook* was overseen by Tim Barton, then OUP Politics Editor, and his then assistant, Dominic Byatt, whom I first met at the party for IPSA "State of the Discipline" panelists thrown by Hans-Dieter in the courtyard of James Stirling's wonderful Wissenschaftszentrum-Berlin. Passing through Oxford five years after the *New Handbook's* publication, I joined Tim (by then Academic Director of OUP) and Dominic (risen to Politics Editor) for a drink in the Eagle and Child to celebrate its success. Tim was full of praise for the *New Handbook*, recounting how it had spawned a whole clutch of Oxford Handbooks across all academic disciplines. "Perhaps I ought get half a percent royalties on each of them, then," I replied. "I have an idea about that!" Tim shot back. And over the next pint or two, the scheme for the multi-volume series of "Oxford Handbooks of Political Science" was hatched.

There are of course all too many of handbooks of this and that, these days. (Apologies for whatever part our initial *New Handbook* might have played in that.) The ten-volume series of "Oxford Handbooks of Political Science" was supposed to be something different. It was not to be just another clutch of handbooks on random topics. Instead, the animating idea was to map political science systematically, sub-discipline by sub-discipline. The aim was nothing less than mapping of the genome of the discipline.

This was clearly going to be a massive undertaking: ten volumes, fifty chapters each. And while it would overload the production team to try to publish them all at the same time, OUP were rightly anxious that all ten volumes should be published within a very few years of one another (in the end, we managed to get all ten out in just three years). Clearly, I needed help. So I inveigled two dozen of the best political scientists in the world to edit the component volumes. My greatest debt is to them, whose names appear opposite the title page, for their gargantuan efforts in pulling this all off: conceptualizing their volume, talking demigods of the profession into writing for

them (and chivvying them to deliver), working with authors to make strong chapters even stronger, and doing it all within a very tight timeframe. I co-edited the first two of the ten volumes myself and know just how much work was involved. So I thank them again, publicly and profusely, for their grace, their commitment, and above all for the excellent products of all their labors.

The present volume has been constructed by “mining” their ten volumes. When Tim, Dominic, and I conceived this plan over drinks seven years ago, it sounded like this step would be the easy one: a good way to produce, in effect, a replacement for the ageing (but still useful) *New Handbook*. That turned out to be an illusion. Editors of each of the ten sub-disciplinary handbooks had fifty chapters to play with; in the one-volume consolidation text, I had to represent all those fifty chapters with merely five per sub-discipline. Editors of the sub-disciplinary handbooks could orchestrate synergies among their chapters that I could not with so few chapters per sub-discipline. In the sub-disciplinary handbooks, many of the most outstanding chapters are detailed discussions of special topics, wonderful but ill-suited for the more general overview purposes of this consolidation text. So I apologize, firstly, to the many authors of truly excellent chapters that, for one reason or another, did not find their way into this volume. The tables of contents of all ten sub-disciplinary handbooks are printed at the back of the present book: I strongly encourage readers to check there to see what they are missing.

I apologize, secondly, to the editors of the ten other handbooks for giving them no hand in making the editorial selection for this volume. I suspect many of them might be relieved not to have had to make invidious comparisons among all the excellent chapters in their own volumes. But the real reason I did not ask them was that all they could tell me was what they thought the “best” chapters in their own handbooks were. This consolidation text is supposed to be more than the sum of its parts, however. While I hope to have chosen chapters that my fellow editors would agree are among the best in their own handbooks, even more than that I hope to have put together a set of chapters that makes organic sense as a collection in its own right, from the point of view of a general political science readership with only a passing acquaintance with many of the sub-disciplines represented. In doing that I have occasionally chosen chapters from handbooks other than that of the sub-discipline concerned: but let there be no implication that there were not plenty of great chapters in that sub-disciplinary handbook to choose from; it was just that some chapter from another of the handbooks better fitted the particular hole I needed to fill in *this* book.

In my opening chapter—which also is very much a personal statement from which many of my fellow handbook editors might well dissent in many places—I report the results of a rudimentary citation analysis operating on the ten-volume series as a whole, paralleling the one Hans-Dieter and I performed on the contents of the *New Handbook*. Take that with as many grains of salt as you deem appropriate: Bibliometrics are always wonky at the margins. Just know, however, that no one was told ahead of time that I was going to analyze other handbooks’ indices in this way. Even if from the *New Handbook* precedent someone guessed that I might, there were

so many different people writing chapters for the ten volumes overall that no one could, by strategic citation choices, do much to alter the overall outcome.

There are two overarching debts that remain for me to record. One is to my home institution: the Research School of Social Sciences at Australian National University. It is a truly remarkable hotbed of intellectual activity, across the whole range of social sciences. Looking at the map it may not seem so, but Canberra truly is the crossroads of the academic universe. Anyone who's anyone eventually visits, and when they come this far they come for a goodly period of time, so it is a genuinely useful interaction. I am proud to have had the chance to get to know so many talented people so well, thanks to the RSSS; and the fruits of all that networking have fed powerfully into the "Oxford Handbooks of Political Science" series and, through that, into this book. In addition to being a magnet for academic talent, I am also especially grateful to RSSS for relieving me from the need to teach students twenty-at-a-time, thus affording me the space to put together volumes like this that teach thousands-at-a-time.

My second overarching debt is to Oxford University Press and many fine people there. This volume is the culmination of a project of conceptualizing, commissioning, and cajoling that has been going on for some seven years; many people at Oxford University Press have helped at various points in the process, for which I'm grateful. But there are three people who have been there throughout, and who deserve far greater tribute than any I can possibly pay them here. Tim Barton as Academic Director for almost the entire period and Des King as the ever-present Politics Delegate were super-supportive from start to finish, always ready to wade in from on high when needed, always smoothing the way. But on a day-to-day (often many-times-a-day) basis it has been Dominic Byatt who has kept this show on the road. I have known many good editors, but I've never known a better one than Dominic: sensible, efficient, firm, unflappable, smart, judicious, funny. He has gone way beyond the call of duty to rescue us from more looming disasters, large and small, than either of us would care to count. He has been the source of as much good substantive advice as virtually any of my academic colleagues. Working with him has been a treat. So thanks, thanks, and thanks again, Dominic, for everything.

When proposing this consolidation volume as the de facto replacement for the *New Handbook of Political Science*, I jokingly suggested we entitle it *A Newer Handbook of Political Science*, so as to preserve my option of doing one more—to be entitled, of course, *The Newest Handbook of Political Science*. Now, at the end of this eleven-volume slog, I'm not sure . . . but give it five or ten years and another drink or two with the good people from OUP at the Eagle and Child, and who knows?

What I can say with some confidence is that the ten-volume mapping of the genome of the discipline truly feels to me like a once-in-a-generation undertaking, unlikely to be replicated anytime soon. I thank Oxford University Press for entrusting it to my General Editorship, and I thank my fellow editors and all the contributors to those volumes for pulling it off so magnificently.

Canberra

September 2008

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PART I

INTRODUCTION

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CHAPTER 1

THE STATE OF THE DISCIPLINE, THE DISCIPLINE OF THE STATE

ROBERT E. GOODIN

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; Schultz 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” (Seidelman 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 2006b, 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 1996b; 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 2006b, 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 (Dogan and Pahre 1990; Dogan 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	Internatl. rela.	Theory
Law and pol.	—	*			**	**				
Pol. econ.		—	**	*	***	***	*	*	*	
Methodology			—	**	***	**	*	*	*	
Pol. behavior				—	****	**	**	*		
Comp. pol.					—	***	**	**	**	
Pol. instns.						—	*	***	**	
Context							—	*	**	*
Public policy								—	*	*
Internatl. 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 Seidelman (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 “intradisciplinary,” 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; Héritier 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 (Dogan 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 Zeitzkow 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; Güth, 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 basis 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 (Heclo 1978; Lehmbruch 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 (Heclo 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; Uslander 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)	Banfield and Wilson, <i>City Politics</i> (1963)	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)	Barry, <i>Political Argument</i> (1965)	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)	Burnham, <i>Critical Elections and the Mainsprings of American Politics</i> (1970)	Cox and McCubbins, <i>Legislative Leviathan</i> (1993)	Dryzek, <i>Deliberative Democracy and Beyond</i> (2000)
Duverger, <i>Political Parties</i> (1951)	Campbell et al., <i>American Voter</i> (1960)	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)	Campbell and Stanley, <i>Experimental and Quasi Experimental Designs for Research</i> (1963)	Fenno, <i>Home Style</i> (1978)	Krehbiel, <i>Pivotal Politics</i> (1998)
Hartz, <i>The Liberal Tradition in America</i> (1955)	Dahl, <i>Who Governs?</i> (1961)	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)	Deutsch, <i>Nerves of Government</i> (1963)	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)	Edelman, <i>The Symbolic Uses of Politics</i> (1964)	Inglehart, <i>The Silent Revolution</i> (1977)	Moravcsik, <i>Choice for Europe</i> (1998)
Luce and Raiffa, <i>Games and Decisions</i> (1957)	Ferejohn, <i>Pork Barrel Politics</i> (1974)	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)	Hirschman, <i>Exit, Voice and Loyalty</i> (1970)	Jervis, <i>Perception and Misperception in International Politics</i> (1976)	Russett and Oneal, <i>Triangulating Peace</i> (2001)
Mills, <i>Power Elite</i> (1956)	Huntington, <i>Political Order in Changing Societies</i> (1968)	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)	Kaufman, <i>The Forest Ranger</i> (1960)	Katznelson, <i>City Trenches</i> (1981)	Tsebelis, <i>Veto Players</i> (2002)
Schumpeter, <i>Capitalism, Socialism and Democracy</i> (1943)	Key, <i>Responsible Electorate</i> (1966)	Keohane, <i>After Hegemony</i> (1984)	Wendt, <i>Social Theory of International Politics</i> (1999)
Selznick, <i>TVA and the Grassroots</i> (1949)	Lane, <i>Political Ideology</i> (1962)	King, Keohane, and Verba, <i>Designing Social Inquiry</i> (1994)	
Simon, <i>Administrative Behavior</i> (1947)	Lijphart, <i>Politics of Accommodation</i> (1968)	Kymlicka, <i>Multicultural Citizenship</i> (1995)	
Waltz, <i>Man, the State and War</i> (1959)	Lindblom, <i>Intelligence of Democracy</i> (1965)	Lindblom, <i>Politics and Markets</i> (1977)	
	Lipset, <i>Political Man</i> (1960)	Lipsky, <i>Street Level Bureaucracy</i> (1980)	
	Lipset and Rokkan, <i>Party Systems and Voter Alignments</i> (1967)	March and Olsen, <i>Rediscovering Institutions</i> (1989)	
	Mayhew, <i>Congress: The Electoral Connection</i> (1974)		
	Moore, <i>Social Origins of Dictatorship and Democracy</i> (1966)		
	Neustadt, <i>Presidential Power</i> (1960)		
	Nozick, <i>Anarchy, State and Utopia</i> (1974)		

(cont.)

Table A1.1.1. (Continued)

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

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. Hecla	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 1996b, 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. Bordieu	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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PART II

POLITICAL
THEORY

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CHAPTER 2

OVERVIEW OF POLITICAL THEORY

JOHN S. DRYZEK
BONNIE HONIG
ANNE PHILLIPS

“What’s your line of business, then?”

“I’m a scholar of the Enlightenment,” said Nicholas.

“Oh Lord!” the young man said. “Another producer of useless graduates!”

Nicholas felt despondent.

(Lukes 1995, 199)

IN *The Curious Enlightenment of Professor Caritat*—Steven Lukes’s fictionalized round-up of contemporary political theory—the hapless professor has been kidnapped by the resistance movement and sent off to search for grounds for optimism. In Utilitaria, he is asked to give a lecture on “Breaking Free from the Past;” in Communitaria, on “Why the Enlightenment Project Had to Fail.” Neither topic is much to his taste, but it is only when he reaches Libertaria (not, as one of its gloomy inhabitants tells him, a good place to be unlucky, unemployed, or employed by the state) that he is made to recognize the limited purchase of his academic expertise. At the end of the book, the professor still has not found the mythical land of Egalitaria. But he has derived one important lesson from his adventures: In the pursuit of any one ideal, it is disastrous to lose sight of all the others.

The moral of that story is the need to accept both normative and methodological pluralism. As Lukes suggests, political theory has often been a battleground where competing theorists pursue their mutually exclusive positions, either ignoring or denying the insights they might derive from considering alternative approaches. Much of that mutual indifference and intolerance remains—political theorists are no more ideal citizens than anyone else—but there is also considerable evidence of pluralism and a marked capacity for borrowing from other traditions. We argue that this pluralism is a key feature and major strength of the field.

1 WHAT IS POLITICAL THEORY?

Political theory is an interdisciplinary endeavor whose center of gravity lies at the humanities end of the happily still undisciplined discipline of political science. Its traditions, approaches, and styles vary, but the field is united by a commitment to theorize, critique, and diagnose the norms, practices, and organization of political action in the past and present, in our own places and elsewhere. Across what sometimes seem chasms of difference, political theorists share a concern with the demands of justice and how to fulfill them, the presuppositions and promise of democracy, the divide between secular and religious ways of life, and the nature and identity of public goods, among many other topics.

Political theorists also share a commitment to the humanistic study of politics (although with considerable disagreement over what that means), and a skepticism towards the hegemony sometimes sought by our more self-consciously “scientific” colleagues. In recent years, and especially in the USA, the study of politics has become increasingly formal and quantitative. Indeed, there are those for whom political theory, properly understood, would be formal theory geared solely towards the *explanation* of political phenomena, where explanation is modeled on the natural sciences and takes the form of seeking patterns and offering causal explanations for events in the human world. Such approaches have been challenged—most recently by the Perestroika movement (Monroe 2005)—on behalf of more qualitative and interpretative approaches. Political theory is located at one remove from this quantitative vs. qualitative debate, sitting somewhere between the distanced universals of normative philosophy and the empirical world of politics.

For a long time, the challenge for the identity of political theory has been how to position itself productively in three sorts of location: in relation to the academic disciplines of political science, history, and philosophy; between the world of politics and the more abstract, ruminative register of theory; between canonical political theory and the newer resources (such as feminist and critical theory, discourse analysis, film and film theory, popular and political culture, mass media studies, neuroscience, environmental studies, behavioral science, and economics) on which political

theorists increasingly draw. Political theorists engage with empirical work in politics, economics, sociology, and law to inform their reflections, and there have been plenty of productive associations between those who call themselves political scientists and those who call themselves political theorists. The connection to law is strongest when it comes to constitutional law and its normative foundations (for example, Sunstein 1993; Tully 1995; 2002).

Most of political theory has an irreducibly normative component—regardless of whether the theory is systematic or diagnostic in its approach, textual or cultural in its focus, analytic, critical, genealogical, or deconstructive in its method, ideal or piecemeal in its procedures, socialist, liberal, or conservative in its politics. The field welcomes all these approaches. It has a core canon, often referred to as Plato to NATO, although the canon is itself unstable, with the rediscovery of figures such as Sophocles, Thucydides, Baruch Spinoza, and Mary Wollstonecraft, previously treated as marginal, and the addition of new icons such as Hannah Arendt, John Rawls, Michel Foucault, and Jürgen Habermas. Moreover, the subject matter of political theory has always extended beyond this canon and its interpretations, as theorists bring their analytic tools to bear on novels, film, and other cultural artefacts, and on developments in other social sciences and even in natural science.

Political theory is an unapologetically mongrel sub-discipline, with no dominant methodology or approach. When asked to describe themselves, theorists will sometimes employ the shorthand of a key formative influence—as in “I’m a Deleuzian,” or Rawlsian, or Habermasian, or Arendtian—although it is probably more common to be labeled in this way by others than to claim the description oneself. In contrast, however, to some neighboring producers of knowledge, political theorists do not readily position themselves by reference to three or four dominant schools that define their field. There is, for example, no parallel to the division between realists, liberals, and constructivists, recently joined by neoconservatives, that defines international relations theory. And there is certainly nothing like the old Marx–Weber–Durkheim triad that was the staple of courses in sociological theory up to the 1970s.

Because of this, political theory can sometimes seem to lack a core identity. Some practitioners seek to rectify the perceived lack, either by putting political theory back into what is said to be its proper role as arbiter of universal questions and explorer of timeless texts, or by returning the focus of political theory to history. The majority, however, have a strong sense of their vocation. Many see the internally riven and uncertain character of the field as reflective of the internally riven and uncertain character of the political world in which we live, bringing with it all the challenges and promises of that condition. In the last two decades of the twentieth century, liberal, critical, and post-structuralist theorists have (in their very different ways) responded to the breakdown of old assumptions about the unitary nature of nation-state identities. They have rethought the presuppositions and meanings of identity, often rejecting unitary conceptions and moving towards more pluralistic, diverse, or agonistic conceptions in their place. These reflections have had an impact on the field’s own self-perception and understanding. Happily for political theory, the process has coincided with a movement within the academy to reconceive knowledge

as more fundamentally *interdisciplinary*. This reconsideration of the function and role of the boundaries of the academic disciplines may help others, as well as political theorists, to see the field's pluralism as a virtue and a strength, rather than a weakness in need of rectification.

1.1 Relationship with Political Science

Political theory's relationship to the discipline of political science has not always been a happy one. Since the founding of the discipline in the late nineteenth century, there have been periodic proclamations of its newly scientific character. The "soft" other for the new science has sometimes been journalism, sometimes historical narrative, sometimes case-study methods. It has also, very often, been political theory. Beginning in the 1950s, behavioral revolutionaries tried to purge the ranks of theorists—and had some success at this in one or two large Midwestern departments of political science in the USA. The later impact of rational choice theory encouraged others, like William Riker (1982a, 753), to reject "belles lettres, criticism, and philosophic speculation" along with "phenomenology and hermeneutics." For those driven by their scientific aspirations, it has always been important to distinguish the "true" scientific study of politics from more humanistic approaches—and political theory has sometimes borne the brunt of this.

Political theorists have noted, in response, that science and objectivity are steeped in a normativity that the self-proclaimed scientists wrongly disavow; and theorists have not been inclined to take the description of political "science" at face value. They have challenged the idea that their own work in normative theory lacks rigor, pointing to criteria *within* political theory that differentiate more from less rigorous work. While resisting the epistemic assumptions of empiricism, many also point out that much of what passes for political theory is profoundly engaged with empirical politics: What, after all, could be more "real", vital, and important than the symbols and categories that organize our lives and the frameworks of our understanding? The French have a word to describe what results when those elected as president and prime minister are representatives of two different political parties: *cohabitation*. The word connotes, variously, cooperation, toleration, sufferance, antagonism, and a sense of common enterprise. Cohabitation, in this sense, is a good way to cast the relationship between political theory and political science.

1.2 Relationship with History

History as a point of reference has also proven contentious, with recurrent debates about the extent to which theory is contained by its historical context and whether one can legitimately employ political principles from one era as a basis for criticizing political practice in another. When Quentin Skinner, famous for his commitment to historical contextualism, suggested that early principles of republican freedom

might offer a telling alternative to the conceptions of liberty around today, he took care to distance himself from any suggestion that “intellectual historians should turn themselves into moralists” (Skinner 1998, 118). He still drew criticism for abandoning the historian’s traditional caution.

In an essay published in 1989, Richard Ashcraft called upon political theorists to acknowledge the fundamentally historical character of their enterprise. While contemporary theorists recognize the “basic social/historical conditions which structure” their practice, “this recognition does not serve as a conscious guideline for their teaching and writing of political theory.” Ashcraft continued: “On the contrary, political theory is taught and written about *as if* it were great philosophy rather than ideology” (Ashcraft 1989, 700). For Ashcraft, acknowledging the ideological character of political theory meant embracing its political character. The main objects of his critique were Leo Strauss and his followers, whom Ashcraft saw as seeking evidence of universally valid standards in canonical political theorists and calling on those standards to judge their works. For Straussians, the wisdom of the ancients and greats is outside history.

Ashcraft also criticized Sheldon Wolin, who shared Ashcraft’s displeasure with Straussians, on the grounds of their inadequate attention to politics. Although Wolin acknowledged the historicity of the texts he had examined in his seminal *Politics and Vision* (1960), Ashcraft claimed that Wolin resisted the “wholesale transformation” that would result, in both his view and Ashcraft’s, from putting that historicity at the center of his interpretative practice. Wolin is famous for championing what, in the style of Hannah Arendt, he termed “the political:” politics understood, not in its instrumental capacity (Harold Lasswell’s (1961) “‘Who gets what, when, and how’”), but rather in its orientation toward the public good coupled with a commitment to the “public happiness” of political participation. Contra Ashcraft, one might see Wolin’s move to the political as a way of splitting the difference between a Straussian universalism and the thick contextualism of Ashcraft’s preferred historicist approach.

“The political” is a conceptual category, itself outside of history, that rejects the idea that politics is about universal truths, while also rejecting the reduction of politics to interests. “The political” tends to connote, minimally, some form of individual or collective action that disrupts ordinary states of affairs, normal life, or routine patterns of behavior or governance. There are diverse conceptions of this notion. To take three as exemplary: The political takes its meaning from its figuration in Wolin’s work by contrast primarily with statism, constitutionalism, and political apathy; in Arendt’s work by contrast with private or natural spheres of human behavior; and in Rancière’s (1999) work by contrast with the “police.”

1.3 Relationship with Philosophy

The most unhistorical influence on political theory in recent decades has been John Rawls, whose work represents a close alliance with analytic philosophy. On one popular account, Rawls arrived from outside as political theory’s foreign savior and

rescued political theory from the doldrums with the publication in 1971 of *A Theory of Justice* (see Arneson, this volume). Rawls's book was an ambitious, normative, and systematic investigation of what political, economic, and social justice should look like in contemporary democracies. With the distancing mechanisms of a veil of ignorance and hypothetical social contract, Rawls followed Kant in looking to reason to adjudicate what he saw as the fundamental question of politics: the conflict between liberty and equality. Writing from within the discipline of philosophy, he returned political theory to one of its grand styles (Tocqueville's two-volume *Democracy in America*, also written by an outsider, would represent another). Much subsequent work on questions of justice and equality has continued in this vein, and while those who have followed Rawls have not necessarily shared his conclusions, they have often employed similar mind experiments to arrive at the appropriate relationship between equality and choice. The clamshell auction imagined by Ronald Dworkin (1981), where all the society's resources are up for sale and the participants employ their clamshells to bid for what best suits their own projects in life, is another classic illustration. Starting with what seems the remotest of scenarios, Dworkin claims to arrive at very specific recommendations for the contemporary welfare state.

One strand of current debates in political theory revolves around the relationship between the more abstracted or hypothetical register of analytic philosophy and approaches that stress the specificities of historical or contemporary contexts. Those working in close association with the traditions of analytic philosophy—and often preferring to call themselves political philosophers—have generated some of the most interesting and innovative work in recent decades. But they have also been repeatedly challenged. Communitarians and post-structuralists claim that the unencumbered individual of Rawlsian liberalism is not neutral but an ideological premiss with significant, unacknowledged political effects on its theoretical conclusions (Sandel 1982; Honig 1993). Feminists criticize the analytic abstraction from bodily difference as a move that reinforces heteronormative assumptions and gender inequalities (Okin 1989; Pateman 1988; Zerilli 2006). As we indicate later, analytic liberalism has made some considerable concessions in this regard. In *Political Liberalism*, for example, Rawls no longer represents his theory of justice as addressing what is right for all societies at all times, but is careful to present his arguments as reflecting the intuitions of contemporary liberal and pluralistic societies.

1.4 Relationship with “Real World” Politics

The way political theory positions itself in relation to political science, history, and philosophy can be read in part as reflections on the meaning of the political. It can also be read as reflections on the nature of theory, and what can—or cannot—be brought into existence through theoretical work. The possibilities are bounded on one side

by utopianism. Political theorists have seemed at their most vulnerable to criticism by political scientists or economists when their normative explorations generate conclusions that cannot plausibly be implemented: principles of living, perhaps, that invoke the practices of small-scale face-to-face societies; or principles of distribution that ignore the implosion of Communism or the seemingly irresistible global spread of consumerist ideas (see Dunn 2000, for one such warning). There is an important strand in political theory that relishes the utopian label, regarding this as evidence of the capacity to think beyond current confines, the political theorist's version of blue-sky science. Ever since Aristotle, however, this has been challenged by an insistence on working within the parameters of the possible, an insistence often called "sober" by those who favor it. At issue here is not the status of political theory in relation to political science, but how theory engages with developments in the political world.

Some see it as failing to do so. John Gunnell (1986) has represented political theory as alienated from politics, while Jeffrey Isaac (1995) argues that a reader of political theory journals in the mid-1990s would have had no idea that the Berlin Wall had fallen. Against this, one could cite a flurry of studies employing empirical results to shed light on the real-world prospects for the kind of deliberative democracy currently advocated by democratic theorists (see for example the 2005 double issue of *Acta Politica*); or testing out theories of justice by reference to empirical studies of social mobility (Marshall, Swift, and Roberts 1997). Or one might take note of the rather large number of political theorists whose interest in contemporary political events such as the formation of a European identity, the new international human rights regime and the politics of immigration, the eschewal of the Geneva Conventions at the turn of the twentieth century, or the appropriate political response to natural disasters leads them to think about how to theorize these events. Concepts or figures of thought invoked here include Giorgio Agamben's (1998) "bare life" of the human being to whom anything can be done by the state, Michel Foucault's (1979) "disciplinary power" that conditions what people can think, Carl Schmitt's (1985) "state of exception" wherein the sovereign suspends the rule of law, Ronald Dworkin's (1977) superhuman judge "Hercules," Jacques Derrida's (2000) "unconditional hospitality" to the other, or Étienne Balibar's (2004) "marks of sovereignty" which signal the arrogation to themselves by political actors in civil society of rights and privileges of action historically assumed by states.

Political theorists take their cue from events around them, turning their attention to the challenges presented by ecological crisis; emergency or security politics; the impact of new technologies on the ways we think about privacy, justice, or the category of the human; the impact of new migrations on ideas of race, tolerance, and multiculturalism; the implications of growing global inequalities on the way we theorize liberty, equality, democracy, sovereignty, or hegemony. Indeed, in writing this overview of the current state of political theory, we have been struck by the strong sense of political engagement and the way this shapes the field.

1.5 Institutional Landscape

Institutionally, political theory is located in several disciplines, starting of course with political science, but continuing through philosophy and law, and including some representation in departments of history, sociology, and economics. This means that the professional associations and journals of these disciplines are hospitable (if to varying degrees) to work in political theory. Among the general political science journals, it is quite common to find political theory published in *Polity* and *Political Studies*, somewhat less so in the *American Journal of Political Science*, *British Journal of Political Science*, and *Journal of Politics*. On the face of it, the *American Political Science Review* publishes a substantial number of political theory articles, but the majority of these have been in the history of political thought, with Straussian authors especially well represented. In philosophy, *Ethics* and *Philosophy and Public Affairs* are the two high-profile journals most likely to publish political theory. Some of the more theoretically inclined law journals publish political theory, and so do some of the more politically inclined sociology journals.

Political theory's best-established journal of its own is *Political Theory*, founded in 1972. Prior to its establishment, the closest we had to a general political-theory academic periodical were two book series. The first was the sporadic *Philosophy, Politics and Society* series published by Basil Blackwell and always co-edited by Peter Laslett, beginning in 1956 and reaching its seventh volume in 2003. Far more regularly published have been the NOMOS yearbooks of the American Society for Political and Legal Philosophy, which began in 1958 and continue to this day. Recent years have seen an explosion in political theory journal titles: *History of Political Thought*; *Journal of Political Philosophy*; *The Good Society*; *Politics, Philosophy and Economics*; *Critical Review of International Social and Political Philosophy*; *European Journal of Political Theory*; *Contemporary Political Theory*; *Constellations*; and *Theory and Event* (an online journal). The *Review of Politics* has been publishing since 1939, although its coverage has been selective, with a Straussian emphasis for much of its history. Political theorists can often be found publishing in related areas such as feminism, law, international relations, or cultural studies. Journals that feature their work from these various interdisciplinary locations include *differences*; *Politics, Culture, and Society*; *Daedalus*; *Social Text*; *Logos*; *Strategies*; *Signs*; and *Millennium*. However, political theory is a field very much oriented to book publication (a fact which artificially depresses the standing of political theory journals when computed from citation indexes, for even journal articles in the field tend to cite books rather than other articles). All the major English-language academic presses publish political theory. Oxford University Press's *Oxford Political Theory* series is especially noteworthy.

Political theory is much in evidence at meetings of disciplinary associations. The Foundations of Political Theory section of the American Political Science Association is especially important, not just in organizing panels and lectures and sponsoring awards, but also in hosting what is for a couple of hours every year probably the largest number of political theorists in one room talking at once (the Foundations reception). The field also has associations of its own that sponsor conferences: the

Conference for the Study of Political Thought International, and the Association for Political Theory (both based in North America). In the UK, there is an annual Political Theory conference in Oxford; and though the European Consortium for Political Research has tended to focus more on comparative studies, it also provides an important context for workshops on political theory.

2 CONTEMPORARY THEMES AND DEVELOPMENTS

As befits a relentlessly critical field, political theory is prone to self-examination. We have already noted controversies over its relationship to various disciplinary and interdisciplinary landscapes. Occasionally the self-examination takes a morbid turn, with demise or death at issue; the most notorious example being when Laslett (1956) claimed in his introduction to the 1956 *Philosophy, Politics and Society* book series that the tradition of political theory was broken, and the practice dead. Even the field's defenders have at times detected only a faint pulse.

Concerns about the fate of theory peaked in the 1950s and 1960s with the ascendancy of behavioralism in US political science. Such worries were circumvented, but not finally ended, by the flurry of political and philosophical activity in the USA around the Berkeley Free Speech movement (with which Sheldon Wolin 1969, and John Schaar 1970, were associated), the Civil Rights movement (Arendt 1959), and protests against the Vietnam war and the US military draft (Walzer 1967; 1970). At that moment, the legitimacy of the state, the limits of obligation, the nature of justice, and the claims of conscience in politics were more than theoretical concerns. Civil disobedience was high on political theory's agenda.¹ Members of activist networks read and quoted Hannah Arendt, Herbert Marcuse, and others in support of their actions and visions of politics.

Throughout the 1960s, the struggle over the fate of theory was entwined with questions about what counted as politics and how to find a political-theoretical space between or outside liberalism and Marxism. It was against this political and theoretical background that John Rawls was developing the ideas gathered together in systematic form in *A Theory of Justice* (1971), a book devoted to the examination of themes that the turbulent 1960s had made so prominent: redistributive policies, conscientious objection, and the legitimacy of state power. Later in that decade Quentin Skinner and a new school of contextualist history of political thought (known as the Cambridge school) rose to prominence in the English-speaking world. Still other works of political theory from this period give the lie to the idea that political theory

¹ See notably Marcuse's "Repressive Tolerance" contribution in Wolff, Moore, and Marcuse (1965), Pitkin (1966), Dworkin (1968), the essay on "Civil Disobedience" in Arendt (1969), and Rawls (1969).

was in need of rescue or revivification. The following stand out, and in some cases remain influential: Leo Strauss's *Natural Right and History* (1953), Louis Hartz's *The Liberal Tradition in America* (1955), Karl Popper's *The Poverty of Historicism* (1957), Hannah Arendt's *The Human Condition* (1958) and *On Revolution* (1963), Sheldon Wolin's *Politics and Vision* (1960), Friedrich A. von Hayek's *The Constitution of Liberty* (1960), Michael Oakeshott's *Rationalism in Politics* (1962), James Buchanan and Gordon Tullock's *The Calculus of Consent* (1962), Judith Shklar's *Legalism* (1964), Herbert Marcuse's *One-Dimensional Man* (1964), Brian Barry's *Political Argument* (1965), and Isaiah Berlin's *Four Essays on Liberty* (1969).

2.1 Liberalism and its Critics

Looking at the field from the vantage point of the first years of the twenty-first century, there is certainly no indication of political theory failing in its vitality: This is a time of energetic and expansive debate, with new topics crowding into an already busy field. For many in political theory, including many critics of liberal theory, this pluralistic activity obscures a more important point: the dominance that has been achieved by liberalism, at least in the Anglo-American world. In its classic guise, liberalism assumes that individuals are for the most part motivated by self-interest, and regards them as the best judges of what this interest requires. In its most confident variants, it sees the material aspects of interest as best realized through exchange in a market economy, to the benefit of all. Politics enters when interests cannot be so met to mutual benefit. Politics is therefore largely about how to reconcile and aggregate individual interests, and takes place under a supposedly neutral set of constitutional rules. Given that powerful individuals organized politically into minorities or majorities can turn public power to their private benefit, checks across different centers of power are necessary, and constitutional rights are required to protect individuals against government and against one another. These rights are accompanied by obligations on the part of their holders to respect rights held by others, and duties to the government that establishes and protects rights. Liberalism so defined leaves plenty of scope for dispute concerning the boundaries of politics, political intervention in markets, political preference aggregation and conflict resolution mechanisms, and the content of rights, constitutions, obligations, and duties. There is, for example, substantial distance between the egalitarian disposition of Rawls and the ultra-individualistic libertarianism of Robert Nozick (1974).² Liberalism's conception of politics clearly differs, however, from the various conceptions of the political deployed by Arendt, Wolin, Rancière, and others, as well as from republican conceptions of freedom explored by Quentin Skinner (1998) or Philip Pettit (1997).

In earlier decades, liberalism had a clear comprehensive competitor in the form of Marxism, not just in the form of real-world governments claiming to be Marxist,

² Other important works in the vast liberal justice literature include Gauthier (1986), Barry (1995), and Scanlon (1998).

but also in political theory. Marxism scorned liberalism's individualist ontology, pointing instead to the centrality of social classes in political conflict. The market was seen not as a mechanism for meeting individual interests, but as a generator of oppression and inequality (as well as undeniable material progress). Marxism also rejected liberalism's static and ahistorical account of politics in favor of an analysis of history driven by material forces that determined what individuals were and could be in different historical epochs. Different versions of this were hotly debated in the 1970s, as theorists positioned themselves behind the "humanist" Marx, revealed in his earlier writings on alienation (McLellan 1970),³ or the "Althusserian" Marx, dealing in social relations and forces of production (Althusser 1969; Althusser and Balibar 1970). Disagreements between these schools were intense, although both proclaimed the superiority of Marxist over liberal thought. In the period that followed, however, the influence of academic Marxism in the English-speaking world waned. The fortunes of Marxist theory were not helped by the demise of the Soviet bloc in 1989–91, and the determined pursuit of capitalism in China under the leadership of a nominally Marxist regime.

Questions remain about liberalism's success in defeating or replacing this rival. One way to think of subsequent developments is to see a strand from both liberalism and Marxism as being successfully appropriated by practitioners of analytic philosophy, such as Rawls and G. A. Cohen (1978). Focusing strictly on Marxism vs. liberalism, however, threatens to obscure the presence of other vigorous alternatives, from alternative liberalisms critical (sometimes implicitly) of Rawlsianism, such as those developed by Richard Flathman (1992), George Kateb (1992), Jeremy Waldron (1993), and William Galston (1991), to alternative Marxisms such as those explored by Jacques Rancière (1989) and Étienne Balibar and Immanuel Wallerstein (1991), and Nancy Hartsock (1983). Michael Rogin combined the insights of Marxism and Freudian psychoanalysis to generate work now considered canonical to American studies and cultural studies (though he himself was critical of that set of approaches; see Dean 2006). Rogin (1987) pressed for the centrality of race, class, property, and the unconscious to the study of American politics (on race, see also Mills 1997).

Liberal theory's assumptions about power and individualism were criticized or bypassed from still other perspectives through the 1970s, 1980s, and 1990s, a fecund period during which political theorists had a wide range of approaches and languages from which to choose in pursuit of their work. In France, social theorists writing in the 1970s (in the aftermath of May 1968) included, most famously, Michel Foucault, whose retheorization of power had a powerful influence on generations of American theorists. In Germany, a discursive account of politics developed by Jürgen Habermas (for example Habermas 1989, first published in German in 1962) captured the imaginations of a generation of critical theorists committed to developing normative standards through which to assess the claims of liberal democratic states to legitimacy. The 1970s Italian *Autonomia* movement inspired new Gramscian and

³ See also the work of the US–Yugoslav Praxis group, and their now-defunct journal *Praxis International*.

Foucaultian reflections on equality, politics, violence, and state power (Virno 2004). For much of this period, feminism defined itself almost as an opposite of liberalism, drawing inspiration initially from Marxism, later from psychoanalytic theories of difference, and developing its own critique of the abstract individual. In Canada and at Oxford, Charles Taylor (1975) was thinking about politics through a rereading of Hegel that stressed the importance of community to political autonomy, influencing Michael Sandel (1982) and many subsequent theorists of multiculturalism. Deleuze and Guattari combined post-structuralism and psychoanalysis into a series of difficult ruminations on the spatial metaphors that organize our thinking at the ontological level about politics, nature, and life (1977; see also Patton 2006). Ranging from Freudian to Lacanian approaches, psychoanalysis has provided political theorists with a perspective from which to examine the politics of mass society, race and gender inequalities, and personal and political identity (Butler 1993; Laclau 2006; Žizek 2001; Irigaray 1985; Zerilli 1994; Glass 2006).

2.2 Liberal Egalitarianism

As the above suggests, alternatives to liberalism continue to proliferate, and yet, in many areas of political theory, liberalism has become the dominant position. Marxism has continued to inform debates on exploitation and equality, but in a shift that has been widely replayed through the last twenty-five years, reinvented itself to give more normative and analytic weight to the individual (Roemer 1982; 1986; Cohen 1995; 2000). There has been a particularly significant convergence, therefore, in the debates around equality, with socialists unexpectedly preoccupied with questions of individual responsibility and desert, liberals representing equality rather than liberty as the “sovereign virtue” (Dworkin 2000), and the two combining to make liberal egalitarianism almost the only remaining tradition of egalitarianism. One intriguing outcome is the literature on basic income or basic endowment, which all individuals would receive from government to facilitate their participation in an otherwise liberal society (van Parijs 1995; Ackerman and Alstott 1999).

For generations, liberalism had been taken to task for what was said to be its “formal” understanding of equality: its tendency to think that there were no particular resource implications attached to human equality. In the wake of Rawls’s “difference principle” (see Arneson, this volume) or Dworkin’s “equality of resources,” this now seems a singularly inappropriate complaint. At the beginning of the 1980s, Amartya Sen posed a question that was to frame much of the literature on distributive justice through the next decade: equality of what? This generated a multiplicity of answers, ranging through welfare, resources, capabilities (Sen’s preferred candidate), to the more cumbersome “equality of ‘opportunity’ for welfare,” and “equality of access to advantage.”⁴ None of the answers could be dismissed as representing a merely formal

⁴ Key contributions to this debate include Sen (1980; 1992); Dworkin (1981; 2000); Arneson (1989); and G. A. Cohen (1989).

understanding of equality, but all engaged with key liberal themes of individuality and responsibility. The subsequent explosion of liberal egalitarianism can be read as a radicalization of the liberal tradition. But the convergence between what were once distinctively liberal and socialist takes on equality can also be seen as demonstrating the new dominance of liberal theory. Much of the literature on equality is now resolutely individualist in form, running its arguments through thought experiments designed to tease out our intuitions of equality, and illustrating with stories of differently endowed individuals, exhibiting different degrees of aspiration and effort, whose entitlements we are then asked to assess. It is not always clear what purchase this discourse of individual variation (with a cast of characters including opera singers, wine buffs, surfers, and fishermen) has on the larger inequalities of the contemporary world. “What,” as Elizabeth Anderson has asked, “has happened to the concerns of the politically oppressed? What about inequalities of race, gender, class, and caste?” (Anderson 1999, 288).

In the course of the 1990s, a number of theorists voiced concern about the way issues of redistribution were being displaced by issues of recognition, casting matters of economic inequality into the shade (Fraser 1997). There is considerable truth to this observation, but it would be misleading to say that no one now writes about economic inequality. There is, on the contrary, a large literature (and a useful website, The Equality Exchange⁵) dealing with these issues. The more telling point is that the egalitarian literature has become increasingly focused around questions of individual responsibility, opportunity, and endowment, thus less engaged with social structures of inequality, and less easily distinguishable from liberalism.

2.3 Communitarianism

One central axis of contention in the 1980s was what came to be known as the liberal–communitarian debate (for an overview, see Mulhall and Swift 1996). Communitarians like Michael Sandel (1982), influenced by both Arendt and Taylor, argued that in stressing abstract individuals and their rights as the building blocks for political theory, liberalism missed the importance of the community that creates individuals as they actually exist. For communitarians, individuals are always embedded in a network of social relationships, never the social isolates that liberalism assumes, and they have obligations to the community, not just to the political arrangements that facilitate their own interests. This opposition between the liberal’s stripped-down, rights-bearing individual and the communitarian’s socially embedded bearer of obligations seemed, for a period, *the* debate in political philosophy. But voices soon made themselves heard arguing that this was a storm in a teacup, a debate within liberalism rather than between liberalism and its critics, the main question being the degree to which holistic notions of community are instrumental to the rights and freedoms that both sides in the debate prized (Taylor 1989; Walzer 1990; Galston 1991).

⁵ <http://aran.univ-pau.fr/ee/index.html>.

Liberalism, it is said, was misrepresented. Its conception of the individual was never as atomistic, abstracted, or self-interested as its critics tried to suggest.

2.4 Feminism

In the 1980s, feminists had mostly positioned themselves as critics of both schools. They shared much of the communitarian skepticism about disembodied individuals, and brought to this an even more compelling point about the abstract individual being disembodied, as if it made no difference whether “he” were female or male (Pateman 1988). But they also warned against the authoritarian potential in holistic notions of community, and the way these could be wielded against women (e.g. Frazer and Lacey 1993). Growing numbers challenged impartialist conceptions of justice, arguing for a contextual ethics that recognizes the responsibilities individuals have for one another and/or the differences in our social location (Gilligan 1982; Young 1990). Still others warned against treating the language of justice and rights as irredeemably masculine, and failing, as a result, to defend the rights of women (Okin 1989).

As the above suggests, feminism remained a highly diverse body of thought through the 1980s and 1990s; but to the extent that there was a consensus, it was largely critical of the liberal tradition, which was represented as overly individualistic, wedded to a strong public/private divide, and insufficiently alert to gender issues. There has since been a discernible softening in this critique, and this seems to reflect a growing conviction that liberalism is not as dependent on the socially isolated self as had been suggested. Nussbaum (1999, 62) argues that liberal individualism “does not entail either egoism or normative self-sufficiency;” and while feminists writing on autonomy have developed their own distinctive understanding of “relational autonomy,” many now explicitly repudiate the picture of mainstream liberal theory as ignoring the social nature of the self (see essays in MacKenzie and Stoljar 2000). Some of the earlier feminist critiques overstated the points of difference from liberalism, misrepresenting the individual at the heart of the tradition as more self-contained, self-interested, and self-centered than was necessarily the case. But it also seems that liberalism made some important adjustments and in the process met at least part of the feminist critique. It would be churlish to complain of this (when you criticize a tradition, you presumably hope it will mend its ways), but one is left, once again, with a sense of a tradition mopping up its erstwhile opponents. Some forms of feminism are committed to a radical politics of sexual difference that it is hard to imagine liberalism ever wanting or claiming (see Zerilli 2006). But many brands of feminism that were once critical of liberalism have made peace with the liberal tradition.

2.5 Democracy and Critical Theory

In the literature on citizenship and democracy, liberalism has faced a number of critical challenges, but here, too, some of the vigor of that challenge seems to have

dispersed. Republicanism pre-dates liberalism by two thousand years and emphasizes active citizenship, civic virtue, and the pursuit of public values, not the private interests associated more with the liberal tradition. Republicanism enjoyed a significant revival through the 1980s and 1990s as one of the main alternatives to liberal democracy (Sunstein 1990; Pettit 1997); indeed, it looked, for a time, as if it might substitute for socialism as *the* alternative to the liberal tradition. Nowadays, even the republican Richard Dagger (2004, 175) allows that “a republican polity must be able to count on a commitment to principles generally associated with liberalism, such as tolerance, fair play, and respect for the rights of others;” this is not, in other words, a total alternative. Deliberative democracy also emerged in the early 1990s as a challenge to established liberal models that regarded politics as the aggregation of preferences defined mostly in a private realm (J. Cohen 1989). For deliberative democrats, reflection upon preferences in a public forum was central; and again, it looked as though this would require innovative thinking about alternative institutional arrangements that would take democracies beyond the standard liberal repertoire (Dryzek 1990). By the late 1990s, however, the very institutions that deliberative democrats had once criticized became widely seen as the natural home for deliberation, with an emphasis on courts and legislatures. Prominent liberals such as Rawls (1997, 771–2) proclaimed themselves deliberative democrats, and while Bohman (1998) celebrates this transformation as “the coming of age of deliberative democracy,” it also seems like another swallowing up of critical alternatives.

The recent history of critical theory—and more specifically, the work of Jürgen Habermas—is exemplary in this respect. Critical theory’s ancestry extends back via the Frankfurt School to Marx. In the hands of Max Horkheimer and Theodor Adorno (1972; first published 1947) in particular, critique was directed at dominant forms of instrumental rationality that defined modern society. Habermas rescued this critique from a potential dead end by showing that a communicative conception of rationality could underwrite a more congenial political order and associated emancipatory projects. Habermas’s theory of the state was originally that of a monolith under sway of instrumental reason in the service of capitalism, which had to be resisted. Yet come the 1990s, Habermas (1996) had redefined himself as a constitutionalist stressing the role of rights in establishing the conditions for open discourse in the public sphere, whose democratic task was to influence political institutions that could come straight from a liberal democratic textbook.

2.6 Green Political Theory

Green political theory began in the 1970s, generating creative proposals for ecologically defensible alternatives to liberal capitalism. The center of gravity was left-libertarianism verging on eco-anarchism (Bookchin 1982), although (at least in the 1970s) some more Hobbesian and authoritarian voices were raised (Ophuls 1977). All could agree that liberal individualism and capitalist economic growth were antithetical to any sustainable political ecology. More recently, we have seen the progress

of “post-exuberant” ecological political theory, characterized by engagement with liberalism. Not all green theory has moved in this direction. For example, Bennett and Chaloupka (1993) work more in the traditions of Thoreau and Foucault, while Plumwood (2002) draws on radical ecology and feminism to criticize the dualisms and anthropocentric rationalism of liberalism.

2.7 Post-structuralism

Post-structuralism is often seen as merely critical rather than constructive. This mistaken impression comes from a focus on the intersections between post-structuralist theory and liberal theory. Some post-structuralist theorists seek to supplement rather than supplant liberalism, to correct its excesses, or even to give it a conscience that, in the opinion of many, it too often seems to lack. Hence Patton’s suggestion (2006) that the distance between post-structuralist and liberal political theory may not be as unbridgeable as is commonly conceived. And some versions of liberal theory are more likely to be embraced or explored by post-structuralists than others: Isaiah Berlin, Richard Flathman, Jeremy Waldron, and Stuart Hampshire are all liberals whose work has been attended to in some detail by post-structuralist thinkers.

But post-structuralists have also developed alternative models of politics and ethics not directly addressed to liberal theory. One way to canvass those is with reference to the varying grand narratives on offer from this side of the field. Post-structuralism is often defined as intrinsically hostile to any sort of grand narrative, a claim attributed to Jean-Francois Lyotard (1984). This claim is belied by a great deal of work in the field that does not so much reject grand narrative as reimagine and reiterate it (Bennett 2002). Post-structuralists do reject foundational meta-narratives: those that present themselves as transcendentally true, for which nature or history has an intrinsic purpose, or that entail a two-world metaphysic. Those post-structuralists who do use meta-narratives tend to see themselves as writing in the tradition of social contract theorists like Hobbes, whose political arguments are animated by imaginary or speculative claims about the origins and trajectories of social life. Post-structuralists, however, are careful to represent their post-metaphysical views as an “onto-story whose persuasiveness is always at issue and can never be fully disentangled from an interpretation of present historical circumstances” (White 2000, 10–11; see also Deleuze and Guattari 1977).

What post-structuralists try to do without is not the origin story by means of which political theory has always motivated its readers, nor the wagers by way of which it offers hope. Rather, post-structuralists seek to do without the ends or guarantees (such as faith, or progress, or virtue) which have enabled some enviable achievements (such as the broadening of human rights), but in the name of which cruelties have also been committed (in the so-called “developing” world, or in the West against nonbelievers and nonconformists).⁶ These ends or guarantees have

⁶ On the role of progress in India, see Mehta (1999). On the fate of nonconformists in Rawls, for example, see Honig (1993).

sometimes enabled political theorists to evade full responsibility for the conclusions they seek, by claiming the goals or values in question are called for by some extra-human source, like god or nature.

3 POLITICAL THEORY AND THE GLOBAL TURN

Liberalism has demonstrated an almost unprecedented capacity for absorbing its competitors, aided by the collapse of its rival, Marxism, but also by its own virtuosity in reinventing itself and incorporating key elements from opposing traditions. Yet this is not a triumphalist liberalism, of the kind proclaimed in Fukuyama's (1989) "end of history," which celebrated the victory of liberal capitalism in the real-world competition of political-economic models. The paradox is that liberalism's absorption of some of its competitors has been accompanied by increasing anxiety about the way Western liberalism illegitimately centers itself. The much discussed shift in the work of Rawls is one classic illustration of this, for while the Rawls of *A Theory of Justice* (1971) seemed to be setting out "the" principles of justice that would be acceptable to any rational individual in any social context, the Rawls of *Political Liberalism* (1993) stressed the reasonableness of a variety of "comprehensive doctrines," including those that could be nonliberal, and the Rawls of *The Law of Peoples* (1999) encouraged us to recognize the "decency" of hierarchical, nonliberal societies that are nonetheless well ordered and respect a certain minimum of human rights.

Having won over many erstwhile critics in the metropolitan centers, liberals now more readily acknowledge that there are significant traditions of thought beyond those that helped form Western liberalism. They acknowledge, moreover, that the grounds for rejecting these other traditions are more slippery than previously conceived. The critique of "foundationalism" (for example, Rorty 1989) used to arouse heated debate among political theorists. Many were incensed at the suggestion that their claims about universal justice, equality, or human rights had no independent grounding, and accused the skeptics of abandoning normative political theory (see, for example, Benhabib et al. 1995). In the course of the 1990s, however, anti-foundationalism moved from being a contested minority position to something more like the consensus. Post-structuralist critiques of foundationalism led to liberalism's late twentieth-century announcement that it is "post-foundational" (Rawls 1993; Habermas 1996)—although with no fundamental rethinking of the key commitments of liberal theory. In the wake, however, of Rawls and Habermas disavowing metaphysical support for their (clearly normative) projects, Western political theorists have increasingly acknowledged the historical contingency of their own schools of thought; and this is generating some small increase in interest in alternative

traditions. The awareness of these traditions does not, of itself, signal a crisis of confidence in liberal principles (arch antifoundationalist Richard Rorty certainly has no trouble declaring himself a liberal), but it does mean that political theory now grapples more extensively with questions of moral universalism and cultural or religious difference (e.g. Euben 1999; Parekh 2000; Honig 2001).

The explosion of writing on multiculturalism—largely from the 1990s—is particularly telling here. Multiculturalism is, by definition, concerned with the multiplicity of cultures: It deals with what may be radical differences in values, belief-systems, and practices, and has been especially preoccupied with the rights, if any, of nonliberal groups in liberal societies. The “problem” arises because liberalism is not the only doctrine on offer, and yet the way the problem is framed—as a question of toleration, or the rights of minorities, or whether groups as well as individuals can hold rights—remains quintessentially liberal. Will Kymlicka (1995) famously defended group rights for threatened cultural communities on the grounds that a secure cultural context is necessary to individual autonomy, such that the very importance liberals attach to individual autonomy requires them to support multicultural policies. His version of liberal multiculturalism has been widely criticized and many continue to see liberalism as at odds with multiculturalism (for example, Okin 1998; 2002; Barry 2001). But in analyzing the “problem” of multiculturalism through the paradigm of liberalism, Kymlicka very much exemplifies the field of debate. Liberalism simultaneously makes itself the defining tradition and notices the awkwardness in this. Its very dominance then seems to spawn an increasing awareness of traditions other than itself.

It is not entirely clear why this has happened now (liberalism, after all, has been around for many years) but that useful shorthand, globalization, must provide at least part of the explanation. It is difficult to sustain a belief in liberalism as the only tradition, or in secularism as the norm, when the majority of the world’s population is patently unconvinced by either (Gray 1995; 1998). And although political theorists have drawn heavily on the liberal tradition in their explorations of human rights or global justice, the very topics they address require them to think about the specificity of Western political thought. Political theory now roams more widely than in the past, pondering accusations of ethno-centricity, questioning the significance of national borders, engaging in what one might almost term a denationalization of political theory. That description is an overstatement, for even in addressing explicitly global issues, political theory draws on concepts that are national in origin, and the assumptions written into them often linger into their more global phase. Terms like nation or state are not going to disappear from the vocabulary of political theory—but the kinds of shift Chris Brown (2006) discerns from international to global conceptions of justice are being played out in many corners of contemporary political thought.

It is hard to predict how this will develop, although the combination of a dominant liberalism with a concern that Western liberalism may have illegitimately centered itself looks unstable, and it seems probable that pockets of resistance and new alternatives to liberalism will therefore gain strength in future years. It seems certain that moves to reframe political theory in a more self-consciously global context will

gather pace. This is already evident in the literature on equality, democracy, and social justice, where there is increasing attention to both international and global dimensions. It is also becoming evident in new ways of theorizing religion. Religion has been discussed so far in political theory mainly in the context of the “problem” of religious toleration, with little attention to the internal structure of religious beliefs. But other dimensions are now emerging, including new ways of understanding the politics of secularism, and closer examination of the normative arguments developed within different religions. It seems likely that new developments in science (particularly those associated with bio-genetics) will provide political theorists with difficult challenges in the coming decade, especially as regards our understanding of the boundaries between public and private, and the prospects for equality. And while the prospect of a more participatory or deliberative democracy remains elusive, we can perhaps anticipate an increasing focus on the role of pleasure and passion in political activism.

It is harder to predict what will happen in the continuing battle to incorporate issues of gender and “race” into mainstream political theory. Many of those who played significant roles in the development of feminist political theory no longer make feminism and/or gender so central to their work. The optimistic take on this is that gender is no longer a distinct and separate topic, but now a central component in political thought. The more pessimistic take is suggested in Zerilli (2006): that the attempt to think politics outside an exclusively gender-centered frame may end up reproducing the blind spots associated with the earlier canon of political thought. The likely developments as regards “race” are also unclear. We can anticipate that racial inequality will continue to figure in important ways in discussions of affirmative action or political representation, but the explosion of work on multiculturalism has focused more on “culture” or ethnicity, and political theory has not engaged in a thoroughgoing way with the legacies of colonialism or slavery.

4 POLITICAL THEORY AND POLITICAL SCIENCE: CURRENT TRAJECTORIES

We noted earlier the sometimes difficult relationship between political theory and the rest of political science. We return to this here, but more with a view to areas of cooperation. In addition to its interdisciplinary locations, political theory has a place in the standard contemporary line-up of subfields in political science, alongside comparative politics, international relations, public policy, and the politics of one’s own country. Here and there, methodology, public administration, political psychology, and public law might be added; and truly adventurous departments may stretch to political economy and environmental politics. All these subfields have a

theoretical edge that potentially connects with the preoccupations of political theory. These connections confirm the importance of political theory to the rest of political science.

International relations has a well-defined sub-subfield of international relations (IR) theory, and we have noted that this is defined largely in terms of the three grand positions of realism, constructivism, and liberalism. Confusingly, liberalism in IR is not quite the same as liberalism in political theory. In IR theory, liberalism refers to the idea that actors can cooperate and build international institutions for the sake of mutual gains; it is therefore linked to a relatively hopeful view of the international system. Realism, in contrast, assumes that states maximize security in an anarchy where violent conflict is an ever-present possibility. Constructivism points to the degree to which actors, interests, norms, and systems are social constructions that can change over time and place. Each of these provides plenty of scope for engagement with political theory—even if these possibilities are not always realized. Despite its differences, IR liberalism connects with the liberalism of political theory in their shared Lockean view of how governing arrangements can be established, and when it comes to specifying principles for the construction of just and legitimate international institutions. Realism is explicitly grounded in the political theory of Thomas Hobbes, interpreting the international system in Hobbesian “state of nature” terms. Thucydides has also been an important if contestable resource for realism (Monoson and Loriaux 1998). Constructivism has been represented (for example, by Price and Reus-Smit 1998) as consistent with Habermasian critical theory. As Scheuerman (2006) points out, critical theory has reciprocated, in that it now sees the international system as the crucial testing ground for its democratic prescriptions. Normative theory is currently flourishing in international relations, and many of the resources for this are provided by political theory (Cochran 1999), with post-modernists, Rawlsian liberals, feminists, and critical theorists making particularly important contributions.⁷

The connections between comparative politics and political theory are harder to summarize because many of the practitioners of the former are area specialists with only a limited interest in theory. Those comparatists who use either large-*n* quantitative studies or small-*n* comparative case studies are often more interested in simple explanatory theory, one source of which is rational choice theory. But there are also points of engagement with political theory as we understand it. The comparative study of social movements and their relationships with the state has drawn upon the idea of the public sphere in democratic political theory, and vice versa. Accounts of the role of the state in political development have drawn upon liberal constitutionalist political theory. More critical accounts of the state in developing societies have drawn upon Marxist theory. In the last two decades democratization has been an important theme in comparative politics, and this work ought to have benefited from a dialogue with democratic theory. Unfortunately this has not happened. Studies of

⁷ See, for example, Pogge (2002), Lynch (1999), Connolly (1991), der Derian (2001), Elshtain (2003), Walker (1993), Rawls (1999), and Habermas (2001a; 2001b).

democratization generally work with a minimalist account of democracy in terms of competitive elections, developed in the 1940s by Joseph Schumpeter (1942), ignoring the subsequent sixty years of democratic theory. Recent work on race and diaspora studies in a comparative context is perhaps a more promising site of connection, invoking Tocqueville (see also Bourdieu and Wacquant 1999; Hanchard 2003). And theorists working on multiculturalism and race have been especially attentive to comparative politics questions about the variety of governmental forms and their interaction with cultural difference (Carens 2000; Kymlicka 2001; Taylor 1994; Gilroy 2000).

Methodology might seem the subfield least likely to engage with political theory, and if methodology is thought of in terms of quantitative techniques alone, that might well be true. However, methodology is also home to reflection on what particular sorts of methods can do. Here, political theorists are in an especially good position to mediate between the philosophy of social science on the one hand, and particular methods on the other. Taylor (1979) and Ball (1987) point to the inevitable moment of interpretation in the application of all social science methods, questioning the positivist self-image of many of those who deploy quantitative methods. The inter-disciplinarity that characterizes so much political theory provides especially fruitful material for methodological reflection.

Public policy is at the “applied” end of political science, but its focus on the relationship between disciplinary knowledge and political practice invites contribution from political theory; and many political theorists see themselves as clarifying the normative principles that underpin policy proposals. From Rawls and Dworkin onwards, work on principles of justice and equality has carried definite policy implications regarding taxation, public expenditure on health, the treatment of those with disabilities, and so on. While it has rarely been possible to translate the theories into specific recommendations (Dworkin’s hypothetical insurance market and Amartya Sen’s theory of capabilities are often said to be especially disappointing in this respect), they are undoubtedly directed at public policy. Normative reasoning applied to public policy largely defines the content of *Philosophy and Public Affairs*, though this reasoning involves moral philosophy as much as or more than political theory.⁸ Political theorists working on questions of democracy and representation have also drawn direct policy conclusions regarding the nature of electoral systems or the use of gender quotas to modify patterns of representation (Phillips 1995).

Policy evaluation and design are important parts of the public policy subfield, and both require normative criteria to provide standards by which to evaluate actual or potential policies. Again, political theory is well placed to illuminate such criteria and how one might think about handling conflicts between them (for example, when efficiency and justice appear to point in different directions). It is also well placed to explore the discourse aspects of public policy, an aspect that has been an especial interest of the Theory, Policy, and Society group of the American Political Science Association. Among the linkages this group develops are those between

⁸ See the compilations of Cohen, Nagel, and Scanlon (1974a; 1974b; 1977); also Goodin (1982).

deliberative democratic theory and policy analysis, between the logic of political argument and interventions by analysts and advocates in policy processes, and between interpretative philosophy of social science and policy evaluation (Hajer and Wagenaar 2003).

Cutting across all the subfields of political science in recent decades has been rational choice theory, grounded in microeconomic assumptions about the wellsprings of individual behavior. Indeed, to some of its practitioners, rational choice is what should truly be described as political theory. For these practitioners, rational choice theory is “positive” political theory, value free and geared toward explanation, not prescription. This claim does not hold up: As explanatory theory, rational choice theory is increasingly regarded as a failure (Green and Shapiro 1994). But many believe that it is very useful nevertheless. Game theory, for example, can clarify what rationality *is* in particular situations (Johnson 1991), thereby illuminating one of the perennial questions in political theory. And despite the frequent description of rational choice theory as value free, it has provided for plenty of normative theorizing among its practitioners. Arch-positivist Riker (1982*b*) deploys Arrow’s social choice theory to argue that democracy is inherently unstable and meaningless in the outcomes it produces, and uses this to back a normative argument on behalf of a minimal liberal democracy that allows corrupt or incompetent rulers to be voted out—but nothing more. The conclusions of rational choice theory are often bad news for democracy (Barry and Hardin 1982); but it is possible to reinterpret this edifice in terms of critical theory, as showing what *would* happen if everyone behaved according to microeconomic assumptions. The political challenge then becomes one of how to curb this destructive behavioral proclivity (Dryzek 1992).

Leading comparativist Bo Rothstein (2005) has expressed the worry that the empirical arm of the discipline has lost its moral compass. To use his running example, its “technically competent barbarians” would have no defense against lining up in support of a political force like Nazism, should that be expedient. Rothstein himself sees the remedy in political theory: “The good news is that, unlike other disciplines, I think we have the solution within our own field of research. This, I believe, lies in reconnecting the normative side of the discipline—that is, political philosophy—with the positive/empirical side” (2005, 10). Despite the likelihood of some resistance to this from both sides of the divide, the examples discussed above suggest that such connection (or reconnection) is indeed possible.

We have argued that political theory is something of a mongrel sub-discipline, made up of many traditions, approaches, and styles of thought, and increasingly characterized by its borrowing from feminist and critical theory, film theory, popular culture, mass media, behavioral science, and economics. The current academy confronts two opposing trends. One draws the boundaries of each discipline ever more tightly, sometimes as part of a bid for higher status, sometimes in the (not totally implausible) belief that this is the route to deeper and more systematic knowledge. Another looks to the serendipitous inspirations that can come through cross-disciplinary and interdisciplinary work; or, more simply and modestly, realizes that there may be much to learn from other areas of study. It is hard to predict which of

these will win out—and most likely, both will continue in uneasy combination for many years to come. We hope and believe that the second trend will turn out to be the dominant one.

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CHAPTER 3

NORMATIVE METHODOLOGY

RUSSELL HARDIN

MODERN political philosophy begins with Thomas Hobbes, David Hume, and others who train their focus on the individual and on interactions between individuals. The purpose of politics in their view is to regulate the behavior of individuals to enable them to be peaceful and productive. They treat of behavior and virtually ignore beliefs. They are interested in social order and its maintenance, not in the salvation of the soul, the creation of a heavenly city, or the ideal society. Hobbes's (1642; 1651) great works of political theory, *De Cive* and *Leviathan*, were published in the first and last years, respectively, of the English Civil Wars, one of the most devastating periods of English history. Against this background, his view of the role of political theory is the explanation and therefore the enablement of social order, a focus that continued through Locke and Hume, although they are increasingly concerned with the working of government and the nature of politics. If any of these three theorists were concerned with "the good society," they would have meant a society that is good for individuals. In an important sense, they are normatively behaviorist. That is to say, they attempt to explain rather than to justify political institutions and behavior. They are also forerunners of the modern self-interest and rational-choice schools of social thought. They are normative theorists only in the very limited sense of *explaining* what would get us to better states of affairs, in the sense of those states' being de facto in our interest or better for us by our own lights. From this vision, the main contemporary approaches to explanation derive. In contemporary normative social theory, there are three main schools—conflict, shared-value, and exchange theories—based, respectively, on interests, shared values, and agreement (as in contractarian theories of both explanation and justification).

The first move in much of normative social science, especially in normative political theory, is to establish a background of self-interested motivation and behavior. Indeed, the transformation of political theory by Hume in his *Treatise of Human Nature* is based on an account of normative issues that is not specifically a theory of those issues and how we should deal with them but is rather an account of how we see them and why we see them that way (Hume 2000 [1739–40], book 3; Hardin 2007, ch. 5). His account is essentially psychological. The way we see normative issues is to fit them to our interests. In keeping with their program to explain, not to justify, Hobbes and Hume are naturalists. Their explanations are grounded in the assumption that people are essentially self-interested and that their actions can be explained from this fact. From their time forward, the development of normative social science has depended heavily on the assumption that individuals are relatively self-interested.

1 SELF-INTEREST

One need not suppose that people are wholly self-interested, but a preponderance or a strong element of self-interest makes behavior explicable in fairly consistent terms. Consistency of individual motivations is central to the task of general explanation of behavior. Many normative or moral theories might yield explanations of behavior but only idiosyncratically, so that we can explain much of your behavior and commitments but not those of your neighbor. No standard moral theory comes close to the general applicability of self-interest as a motivation for large numbers of people.

Hobbes and Hume are not alone in this view. Bernard Mandeville,¹ Adam Smith, and Alexis de Tocqueville, among many others, conclude that self-seeking behavior in certain very important and pervasive contexts promotes the good of society in the—to them—only meaningful sense, which is promoting the good of individuals. Consider Tocqueville (1966 [1835 and 1840], ii, ch. 8) who, with his characteristic clarity, justifies the interest-based normative program in a forceful chapter on “Individualism and the doctrine of self-interest properly understood.” He says that the doctrine of self-interest properly understood is *the best moral theory for our time*. He comes from a background in which French Catholic virtue theory was the dominant strain of moral judgment. He notes that in the United States, where he famously toured as de facto an ethnographer, there was no talk of virtue. Clearly he approves of this fact. In virtue theory, he says, one does good without self-interest. The American trick combines interest and charity because it is in the interest of each to work for the

¹ Mandeville’s subtitle is “Private Vices, Publick Virtues.”

good of all, although they need not know or intend this. This is Smith's argument from the invisible hand and it leads us to a resolution of the logic of collective action in the provision of large-scale public benefits. I seek my own good, you seek yours, and all together we promote the good of all. The happiness of all comes from the selfishness of each (ii. 376). Recall one of Smith's most quoted aphorisms, that it is "not from the benevolence of the butcher, the brewer, or the baker, that we expect our dinner, but from their regard to their own interest" (Smith 1976 [1776], 1.2.2, 26–7). Arguably, Tocqueville's central thesis is that, if you give democratic peoples education and freedom and leave them alone, they will extract from the world all the good things it has to offer (Tocqueville 1966 [1835 and 1840], ii. 543). This is, of course, a collective achievement based on individually motivated actions.

This view is not strictly only a modern vision. Aristotle states a partial version of it, in passing, in his praise of farmers as especially good citizens for democracy: "For the many strive for profit more than honor" (*Politics*, 1318b16–17). Aristotle says this with approval. If his claim were not true, he supposes that society would not cohere, because it is founded on the generality and stability of the motivations of farmers, whose productivity is fundamentally important for the good of all in the society. The scale of the contributions of farmers to the good of society remained relatively constant from the time of Aristotle until roughly two or three centuries ago in Europe when industrial production began to displace it as the main locus of employment. Today 2 or 3 percent of the workforce in the advanced economies suffices for agricultural production. It is an extraordinary fact that all of our main strands of political theory originate in the earlier era, when social structure was radically different.

A slight variant of the Aristotle–Hobbes–Hume view of the role of interest in the ordering of society is an assumption at the foundation of John Rawls's theory of justice. Rawls (1999, 112 [1971, 128]; see also Hardin 2003, 3–8) supposes that citizens are *mutually disinterested*. By this he means that my assessment of *my own benefits* from the social order established under his theory of justice does not depend on *your benefits* from that order. For example, I do not envy you and you do not envy me. Our social order has been established as just and there is no alternative that is similarly just and that would better serve my interests.² If we are mutually disinterested, then we have no direct concern with the aggregate outcome, but only with our own part or share in that outcome. This is a fundamentally important assumption in Rawls's theory, without which the theory would not go, but it is not often addressed in the massive literature on that theory. But even that theory, put forward in a nonagricultural world, builds on earlier visions of society.

² There could be two equally qualified just orderings, in one of which I am better off than I am in the other. It does not follow that a society of people who are committed to justice would rank the one of these equally qualified orderings in which I am better off above the other, because someone else will be worse off in that ordering. Hence, there would be no mutual advantage move that would make both of us better off.

2 THREE SCHOOLS OF SOCIAL THEORY

One can do normative political analysis without starting from rational choice principles and, indeed, such analysis is often done as an alternative to rational choice theories. But one cannot do very systematic, coherent political analysis without a clear delineation of basic principles on which the analyses are to be built. So for example, there are three grand theories—or schools of theory—on social order, each of which is based on a systematic set of theoretical assumptions. First are *conflict*, as represented by Thrasymachus (in Plato's *Republic*), Karl Marx, and Ralf Dahrendorf (1968; also see Wrong 1994). Hobbes is also commonly considered strictly a conflict theorist, but I think that this is wrong; that, as noted below, he is largely a coordination theorist. Conflict theories commonly turn to coercion or the threat of coercion to resolve issues. Hence, they almost inherently lead us into normative discussions of the justification of coercion in varied political contexts (Hardin 1990). They can also lead to debates about the nature of power and compliance as in Machiavelli, Marx, Gramsci, Nietzsche, or Foucault.

Second are *shared-value theories*, as represented by John Locke, Ibn Khaldun, and Talcott Parsons (1968 [1937], 89–94). Religious visions of social order are usually shared-value theories and, as Tocqueville notes, interest is the chief means used by religions to guide people. Religious and theological theories and justifications once held sway but are now of little import in Western social science. Now religious commitments and beliefs are merely social facts to be explained. Many contemporary shared-value theorists in the social sciences in the West are followers of Parsons. These followers are mostly sociologists and anthropologists—there are virtually no economists and there are now few political scientists in the Parsons camp. There was a grand Parsonian movement in political science from the 1950s through some time in the 1970s. The most notable and creative example of this movement is the civic culture of Gabriel Almond and Sidney Verba (1963) and others. Although there is not much of a grand-synthesis view of norms that remains in political science or even in much of sociology, there are still ad hoc theories of norms. For example, political scientists often explain the voting that occurs as public spirited, altruistic, or duty driven. And there is today a rising chorus of political scientists who take a more or less ad hoc stand on the importance of a value consensus, as represented by those concerned with the supposed declines in trust, family values, and community (e.g. Putnam 2000).

Contractarians in social theory are typically shared-value theorists. This may sound odd, because legal contracts typically govern exchanges. But social contract theory requires a motivation for fulfilling one's side of a contractual arrangement and a social contract is not analogous to a legal contract in this respect. Because there is no enforcer of it, a social contract is commonly therefore seen to require a normative commitment—essentially the same normative commitment from everyone (see Hardin 1999, ch. 3). For example, in the view of Thomas Scanlon (1982, 115 n.; 1999; see further Barry 1995 and Hardin 1998) the motivation to keep to a social

contract is the desire to achieve reasonable agreement on cooperative arrangements. This appears to entail a straightforward factual issue about the existence of this desire. Is this desire prevalent? Because of the difficulty of defining reasonable agreement, it seems unlikely. The methodological task of demonstrating the prevalence of such a desire seems simple enough, but the reasonable agreement theorists have not bothered to test their assumption. It seems very unlikely that there is such a desire, so that Scanlon's contractualism cannot undergird social cooperation or, therefore, social theory. Contracts for ordinary exchanges are backed by various incentives to perform, especially by the threat of legal enforcement, by the interest the parties have in maintaining the relationship for future exchanges, or in maintaining their reputations. Social contracts have none of these to back them.

And, third, there are *exchange*, which are relatively more recent than the other two schools, with Bernard Mandeville and Adam Smith among the first major figures, and, in our time, George Homans and many social choice theorists and economists.³ At the core of an exchange theory is individualism. Tocqueville (1966 [1835 and 1840], vol. ii. 506–08), writing in the 1830s, says “individualism” is a new term. It turns on the calm feeling that disposes each to isolate himself from the mass and to live among family and friends. It tends to isolate us from our past and our contemporaries. The rigorous, uncompromising focus on individuals is a distinctive contribution of Hobbes, the contribution that puts us on the track to modern political philosophy and that makes Hobbes at least partially an exchange theorist. For him, the assumption of individualism is *de facto* a method for focusing on what is central to social order. It is also, of course, a descriptive fact of the social world that he analyzes. It becomes Tocqueville's assumption in analyzing American society two centuries later, when it is also the basis for criticizing his own French society. He says that, at the head of any undertaking, where in France we would find government and in England some territorial magnate, in the United States we are sure to find an association (513). These associations are made up of individuals who voluntarily take on their roles; they are not appointed to these roles, which are not part of any official hierarchy. Tocqueville has a forceful method: go to the core of any activity to explain the form of its successes and failures. And when we do that for America in the 1830s, we find individuals motivated by their own interests. When we do it for France, we find government agents and regulations. Anyone who has lived in both France and the United States might reasonably conclude that the two societies have moved toward one another in this respect, but that they still differ in the way Tocqueville finds nearly two centuries ago.

Note that these three sets of assumptions—individualism, self-interest, and the collective benefits of self-seeking behavior—are the assumptions of both positive and normative theories. This should not be a surprise because the world we wish to judge normatively is the same world we wish to explain positively. Moreover, all of the normative theories we might address are likely to have positive elements that

³ There are also many theories and assumptions, such as structural theories as represented by Marx and articulated by many structuralist sociologists in our time, that are much less broadly applicable, both positively and normatively.

we could analyze from the perspective of relevant positive theories. For example, to argue persuasively for shared-value theories we must be able to show that there are shared values. This is often not done very well or even at all, but is merely assumed as though it were obvious. A fully adequate normative theory must therefore fit both positive and normative assumptions and must depend on both positive and normative methodologies. Often this must mean that the methodological demands of normative claims are more stringent than the methodological demands of any parallel positive claims. Normative claims must pass muster on both positive and normative methodological standards.

Given the pervasiveness of shared-value theories in contemporary social and political theory, we should consider whether there are shared values of the relevant kind and force. This is, again, a positive issue and it should not be hard to handle. Once we establish that there are or are not relevant shared values, we can go on to discuss how they are constructed and what implications they might have for social theory, actual institutions, and political behavior.

3 SHARED VALUES

Suppose it is established that we do share some important set of political values, X, Y, and Z. What follows? Our shared values do not directly entail any particular actions because acting on those values might conflict with our interests in other things, and acting on our shared values might cost you heavily enough to block you from acting in our common interest. Superficially it might seem that interest, for example in the form of resources, is merely another value, or rather a proxy for values that could compare to X, Y, and Z. But this will commonly be wrong. For an important political example, suppose we are all or almost all patriotic. Your patriotism benefits me if it motivates you to act in certain ways, but acting in those ways likely has costs for you, so that although we share the value of patriotism we may not have incentives to act in ways that benefit each other. Given that we share the value of patriotism to a particular nation, we might want to ask on what that value commitment is founded. It could be founded on interests, identity, or bald commitment to our nation, right or wrong. It might not be easy to establish which, if any, of these plays a role. Tocqueville supposes that patriotism founded on interests must be fragile, because interests can change (Tocqueville 1966 [1835 and 1840], i. 373). We might also suppose that our interests in patriotism here could be compromised in favor of other interests.

Perhaps our commitment turns on our ethnic identity, as is commonly claimed for nationalist commitments. There can typically be no compromise on ethnicity and the costs of defending one's ethnicity may be discounted heavily for that reason. You cannot trade half of your ethnic commitments for half of mine. Of course,

the next generation might do exactly that. They might marry across our ethnic divide, engage in joint corporate activities, and have friendship groups that straddle ethnic lines. Sadly, such actions and even merely their possibility might be sources of deep conflict between our groups. On economic issues, there commonly is some possibility of compromise that lets the parties split differences to allow all to gain from staying involved with each other, even cooperating together and coordinating on many fundamentally important activities. This is, for Smith and many other political economists, a major unintended benefit of the market for exchange.

Contract or agreement theories suggest a need or at least an urge to explain why we agree, and the answer often must be that it is in our interest to agree on some particular social arrangement or that we share the values on which we are to contract. Hence, agreement theories threaten to reduce to simple interest or to shared-value theories or explanations. But even then they have a strength that shared-value theories often lack. Once your interests, pro and con, are established, there is likely no further need to explain why you act in relevant ways. Motivations and interests tend to collapse into each other if they are fully defined. Unless someone's commitment to some value translates in standard terms into their interests (hence, the odd locution "can be cashed out" as), we still face the task of determining how that value commitment will motivate action, if at all. In sum, interest is both a value and a motivation. Shared-value theories must first establish what values are shared and then give an account of how commitment to them motivates action. Both steps here may be very difficult. Indeed, each of these steps might challenge some of our standard methodologies for establishing social and psychological facts.

An important subcategory of shared-value theory is the body of norms that regulate our behavior in social interaction. The category of norms is much broader than that for social order, but it is these that matter for political theory. We may parse the category of norms in many ways. The most common move is simply to list many norms and to apply them to particular problems, as with the putative norms on voting. In a far more systematic approach, Edna Ullmann-Margalit (1977) lays out several categories as based on the game-theoretic structure of the underlying problems that the norms help to resolve or at least address. Her deep insight is that norms must handle the strategic structure of the incentives people face if the norms are to get them to behave cooperatively. Her modal strategic categories are prisoner's dilemma, coordination and unequal coordination, and conflict. Some of Ullmann-Margalit's norms help us, respectively, to coordinate, to cooperate, or to manage conflict in these contexts.

It is striking that Ullmann-Margalit's book from only four decades ago is among the first serious efforts to bring strategic analysis systematically to bear on normative theory and problems. Indeed, we might well speak of the strategic turn in social theory, a turn that has been heavily influenced and even guided by game theory, which was invented roughly during the Second World War (Neumann and Morgenstern 1953 [1944]). That turn has influenced both positive and normative theory. There are standard norms that address all of Ullmann-Margalit's strategic categories and those norms have vernacular standing in ordinary life contexts. But Ullmann-Margalit

shows that many norms are strategically related and thereby shows how they are grounded in incentives. In political theory, the norms that most interest us are those that regulate social order (Hardin 1995, chs. 4 and 5).

4 A FOURTH THEORY: COORDINATION

Because there generally is conflict in any moderately large society, coercion is a *sine qua non* for social order. But it is only one *sine qua non*. Two others are exchange and coordination. All are needed because the strategic structures of our potential interactions are quite varied, and we need devices for handling all of these reasonably well if we are to have desirable order and prosperity. In a subsistence agricultural society, coercion might be very nearly the only point of government. But in a complex society, coercion seems to be a minor element in the actual lives of most people, although the threat of it might stand behind more of our actions than we suppose. In such a society, exchange and coordination loom very large, radically larger than in the subsistence economy.

The three grand, broadly established schools of political thought—conflict, shared values, and agreement or exchange—are right about particular aspects of social order. But they miss the central mode of social order in a complex modern society, which is coordination (Lindblom 1977; Schelling 1960). We do not necessarily share values but we can coordinate to allow each of us to pursue our own values without destructive interaction or exchange. To grossly simplify much of the problem of social order in a complex society, consider the relatively trivial problem of maintaining order in traffic on roads. There are two main coordinations at stake. The first is the obvious one of merely getting all drivers to drive on the same side of the road—either all on their left or all on their right—in order to prevent constant accidents and difficult problems of negotiating who gets to go first. The second is the problem of controlling the flow of traffic at intersections, for which traffic signals and signs are used when the traffic is heavy enough. Two striking things about the collection of drivers are that *they are not genuinely in conflict* and that *they do not typically have to share any general social values* in order for these coordinations to work well. Furthermore, *there is no exchange* that they can make to solve the problems arising from their interactions. I have my purposes, you have yours, and we want merely to avoid getting in each other's way while going about our own affairs. The seeming miracle is that often we can do all of this spontaneously. For example, some coordinations can be managed by relying on focal points (Schelling 1960) that make a particular solution obvious or on institutions, which can define a resolution. Getting everyone to drive right is an instance of the first of these devices; managing traffic flow at intersections is an instance of the second.

As are conflict theories, coordination is an interest theory. Hobbes is perhaps the first major coordination theorist.⁴ But David Hume (2000 [1739–40], book 3), Adam Smith (1976 [1776]), and C. E. Lindblom (1977) see much of social order as a matter of coordinating the disparate interests of many people. A shared-value theory could be essentially a coordination theory if the values motivate coordinated actions, but *coordination does not require broadly shared values*. This is the chief reason why coordination is fundamentally important in modern social and political theory. Shared-value theories typically make adherence to relevant values a matter of overriding one's interests and, when put into political power, overriding the interests of many citizens. For example, I help to defend my community despite the risks that such effort entails, I submerge my identity in the collective identity (whatever that might mean), or I vote despite the burden to me of doing so and despite the virtual irrelevance of the effect of my vote on my interests. But against the strenuous and implausible view of Parsons, a collection of quite diverse pluralists can coordinate on an order for the society in which they seek their diverse values. In sum, coordination interactions are especially important for politics and political theory and probably for sociology, although exchange relations might be most of economics, or at least of classical economics. In a sense, the residual Parsonians are right to claim that conflict relations are not the whole of political order, although not for reasons that they might recognize. They are right, again, because the core or modal character of social order is coordination.

While at a commonsense level the problem of coordination is typically not difficult to grasp, its general significance and its compelling nature have not been central understandings in the social sciences or in political philosophy. Hobbes had a nascent coordination theory in his vision of our coordinating on a single sovereign (Hardin 1991). Had he been more supple in his views, he might have recognized that the dreadful problem of civil war in his England was a matter of *multiple coordinations* of various groups in mortal conflict with each other. There was no war of all against all but only war between alternative factions for rule, each of which was well enough coordinated to wreak havoc on the others and on nonparticipant bystanders. Hume made the outstanding philosophical contribution to understanding coordination problems, but his insights were largely ignored for two centuries or more after he wrote and they are still commonly misread.⁵ Thomas Schelling (1960, 54–8) gave the first insightful game-theoretic account of coordination problems and their strategic and incentive structures. But their pervasive importance in social life is still not a standard part of social scientific and philosophical understanding.

In social life, coordination occurs in two very different forms: spontaneously and institutionally. We can coordinate and we can be coordinated as in the two-part coordination of traffic. In Philadelphia in 1787 a small number of people coordinated spontaneously to create the framework to organize the new US nation institutionally.

⁴ Not all Hobbes scholars would agree with this assessment. For an argument for understanding him as a coordination theorist, see Hardin (1991).

⁵ Hume's arguments may have been overlooked because they are chiefly in a series of long footnotes in Hume (2000 [1739–40], 3.2.3.4 n–11 n).

Once they had drafted their constitution, its adoption was beneficial to enough of the politically significant groups in the thirteen states that, for them, it was mutually advantageous (Hardin 1999). Therefore, they were able to *coordinate spontaneously on that constitution* to subject themselves to being *coordinated institutionally by it* thereafter. This is the story of very many institutional structures that govern our social lives, and the more often this story plays out in varied realms, the more pervasively we can expect to see it carried over to other realms and to organize our institutions, practices, and even, finally, our preferences, tastes, and values. As it does so, it might be expected then to drive out or to dominate alternative ways to create and justify our social organization.

5 CONCLUDING REMARKS

In the era of Hobbes, writing during the English Civil War, the first focus of political theory was social order in which individuals might survive and prosper. Success in managing order has pushed worry about social order out of its formerly central place, even virtually out of concern altogether for many political theorists. The meaning of justice has changed to match this development. Through Hume's writings, justice is commonly conceived as "justice as order," as in Henry Sidgwick's (1907, 440) somewhat derisive term. This is more or less the justice that legal authorities and courts achieve in the management of criminal law and of the civil law of contracts and property relations. By Sidgwick's time, it begins to be conceived as, or at least to include, distributive justice, as in the theory of John Rawls (1999 [1971]). Hume and John Stuart Mill (1977 [1861]) also shift the focus toward the institutions of government, which in large modern societies entails representative government. This move brings back classical and Renaissance political thought. It also makes great demands on causal understandings and therefore on positive theory and methodology, again tying the normative and the positive tightly together in a single account. Rawls's theory also requires massive positive understandings when he says that now the task is to design institutions capable of delivering distributive justice, a task that he leaves to others, who have so far generally failed to take it up. Rawls's and Hobbes's theories are relatively holistic and general; Hume's and Mill's are relatively piecemeal and specific. Perhaps no methodology gives us serious entrée to handling holistic social and political theory at the level and scale required by Hobbes and Rawls. Eventually, therefore, we must want to break down the institutional moves entailed by Rawls's theory to make them piecemeal and manageable.

It is an interesting fact that normative methodologies have changed substantially over the past several decades. Methodologies in many fields of social theory and explanation have been refined extensively during that period, especially under the influence of rational choice and game theory but few if any of them have been dropped or

developed *de novo*. Today's three leading normative methods have come into their own during that period, so much so that it is hard to imagine what normative theories would be like today without those methods driving their articulation and refinement. Developments have not been equally dramatic in all three methods. Two of the methods, shared-value and contractarian arguments, threaten to be narrowed down to use by academic moral theorists with little resonance beyond that narrow community. Any method that becomes as esoteric as much of contemporary moral theory has become is apt to be ignored and even dismissed by the overwhelming majority of social theorists as irrelevant. That would be a profoundly sad separation of normative from positive theory, the worst such separation in the history of social theory, worse than the separation of economic from utilitarian value theory wrought by G. E. Moore (1903, 84) a century ago, when he literally took utility into the vacuousness of outer space.

The theorists who work in the normative vineyard often seem to strive more for novelty than for comprehensiveness or even comprehension. Great novelty cannot generally be a worthy goal for us in social theory. The occasional major novel invention, such as Hobbes's all-powerful sovereign as a form of institutionally enforced coordination, Hume's convention as a form of spontaneously enforced coordination, Smith's classical economics, Vilfredo Pareto's (1971 [1927]) value theory, John von Neumann and Oskar Morgenstern's (1953 [1944]) game theory, or Schelling's (1960) coordination theory takes a long time to be incorporated into the main stream of theory and explanation of social institutions and practices. A flood of supposedly novel contributions is apt to be ignored or openly dismissed. Creativity in social theory is not likely to depend on such major innovations except on relatively rare occasions. *Most of the creativity we see is in the application of well-established innovations across many realms.*

Over the past four or five decades, rational-choice normative theory, the third major branch of contemporary normative methodology, has become a vast program that increasingly leaves the other two branches behind in its scope and sheer quantity of work. This development is made more readily possible by the clarity and systematic structure of game theory and game-theoretic rational choice. Game theory and rational choice methodology are very well laid out and easily put to use. Perhaps at least partially because of that fact, rational choice methods are taking over normative theorizing and theories. Early steps along the way in this seeming conquest include Richard Braithwaite's (1955) use of game theory in moral reasoning, David Lewis's (1969) analysis of convention in the spirit of Hume, Ullmann-Margalit's (1977) theory of norms, and a flood of other works from the 1980s on.

In this program, method and theory tend to merge. One might wonder whether this is a typical tendency for relatively developed theories and the methods successfully associated with them. Shared-value theory is perhaps becoming the most commonly asserted alternative to rational choice in our time as contractarian reasoning recedes from center stage in the face of challenges to the story of contracting that lies behind it and the difficulty of believing people actually think they have consciously agreed to their political order, as long ago noted by Hume (1985 [1748]). But it faces a

harder task than rational-choice normative theory because it has barely begun at the basic level of establishing a set of demonstrably shared values other than own welfare. Own welfare is, of course, the shared value that shared-value theorists most want to reject, although one wonders how many of the most ardent opponents of that value as a general guiding principle in social theory would actually reject that value in their own lives.

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CHAPTER 4

THEORY IN HISTORY

PROBLEMS OF CONTEXT AND NARRATIVE

J. G. A. Pocock

1 THE PROBLEMS OF TERMINOLOGY

To construct a study of the relations between “political theory” and “history”—as conceptualized phenomena or as disciplines we practice—it is necessary to study these terms and, if possible, to reduce them to manageable forms. The term “political theory” is imprecise; it has been used in a diversity of ways, and the contributors to this *Handbook* are probably not agreed on any single usage. From the standpoint from which this chapter is written, it is observable that “political theory” is often used as if it were interchangeable with “political thought,” a term equally inexact. In the first half of the twentieth century, there were written a number of “histories of political thought,” or of “political theory,” of which the subject-matter and the method were practically indistinguishable. By “political thought” (and therefore “theory”) were meant a number of intellectual disciplines—or alternatively, modes of rhetoric—which had from time to time been applied to a subject or subjects which it was agreed formed that of “politics.” The “history” of these modes of discourse was agreed to form the “history of political thought” or “theory.” They contained much that amounted to a “theoretical” treatment of an abstract concept of “politics,” and each of them—at least in principle—had generated a second-order discourse which critically examined its conduct, and so amounted to “theory” in a further sense of that term.

These “histories” of political thought/theory were canonically constructed; that is, they arranged modes of discourse—and above all, the major texts that had acquired classical status and authority in each—in an order which it had come to be agreed formed the “history” being presented. Classically—and, it should be emphasized, for historical reasons, many of which were good—they began with the invention in fourth-century Athens of what was termed “political philosophy,” so that “political philosophy” became a term of equal status (and imprecision) with “political thought” and “theory.” A historical grand narrative emerged, in which “the history of political thought,” “theory,” or “philosophy” moved from Platonic or Aristotelian beginnings through a medieval period in which “philosophy” encountered Christian theology, into one in which this encounter was liquidated and replaced by modes of thought, theory, and philosophy it was agreed to term “modern.”

It was a further characteristic of these “histories” that they were not written by historians so much as by “political theorists” and “philosophers” who held that the study of this “history” was in some way conducive to the enterprise or enquiry in which they were themselves engaged. To study “the history of political theory” was helpful to the practice of “political theory.” This assumption came, at and after the middle of the twentieth century, to be attacked in two ways. There arose ways of conducting both the empirical and the normative study of politics which claimed to have no need of historical knowledge—still described in its canonical form—because they possessed means of validating, criticizing, verifying or falsifying, the statements that they made, which depended upon the method that they practiced and not upon historical circumstance or character. This may be considered one of the moments at which the term “political science” made its appearance. Concurrently—and in some ways in response to this development—historians appeared who proposed (often aggressively) to reduce “the history of political thought” to a rigorously autonomous mode of historical enquiry. The writing of texts, the slower formation of belief systems or “philosophies,” were to be reduced to historical performances or “speech acts,” the actions of historical actors in circumstances and with intentions that could be ascertained. They were not part of a “theory of politics,” or if they were, the processes by which they had come to be so, and the very existence of “political theories” themselves, were historical processes in the performance of acts and the formation of languages, to be studied as such.

Important claims can be made about the increase and intensification of historical knowledge which this revolution in method brings about. The theorist or philosopher is faced with the question of whether “political theory” is or is not to be reduced to the knowledge of its own history. A typical response has been to treat this question as itself a problem in theory or philosophy, and it can be observed that more has been written about Quentin Skinner—a leader in the historical revolution—as political theorist or philosopher than as historian. The author of this article, however, treats Skinner’s work, and his own, as the construction of historical narratives, in which things happen (in this case the utterance of theoretical statements about politics), the conditions or “contexts” in which they happen exist and change, and processes occur in the history of these performances that can be narrated. In what follows, it will be presupposed that a “historian,” interested in the question “what was it that

was happening?”, and a “political theorist,” engaged in an enquiry possessing its own ways of self-validation, confront each other over the reading of a given text. I will bias my own enquiry by pointing out that the text will be a historical artifact, but that the theorist desires to make use of it for purposes other than establishing it as a historical phenomenon.

2 HISTORY AND THEORY: THE ENCOUNTER

The activity of the mind called “political theory” will have been defined—probably, and properly, in more ways than one—by the contributors to this volume. For purposes of abbreviation, I will suppose that they have defined it as the construction of heuristic and normative statements, or systems of such statements, about an area of human experience and activity called “politics” or “the political.” I will also suppose that the activity called “political theory” is a discipline possessing its own rules: that is to say, the statements it aims to construct acknowledge certain procedures according to which they are constructed and may be validated and criticized. There will instantly arise, however, a further activity of questioning how such procedures have been and are being constructed, to what capacities of the mind they make appeal, whether their claims to validity are or have been justifiable, and in short whether, and how, it is possible to construct a discipline called “political theory” at all. This activity of the second order may be called “political philosophy”—although this term has borne other meanings—and distinguished from “political theory” as carried on at levels confident enough of its procedures to dispense, at least provisionally, with the questioning of them at the levels called “philosophy.” Having made this distinction, of course, we observe that the two activities continually intersect, although the distinction does not disappear.

It is valuable to imagine the “political theorist”—given that this term may have more than one meaning—confronted by a “historian of political thought,” who regards “political theory,” in any of its meanings, as one of many ways in which “thought,” or rather “discourse,” about “politics” has been going on. Even if we suppose our agonists to agree on a definition of the activity to be called “political theory,” and to agree that this activity has had a continuous history of some duration, there will remain many senses in which they do not and perhaps should not have much to say to one another. The “theorist” is interested in the making of statements (hypotheses?) obedient to certain modes of validation; the “philosopher” in the question of how (and whether) it is possible to construct these (or any) modes of validation (or evaluation). The historian is not interested primarily, although perhaps secondarily, in any of these questions, but in the question “what happened?” (or was happening)—more broadly still, “what was it that was happening?”—when events or processes occurred in the past under study. One aims to characterize, to evaluate, to explicate (rather than explain), and therefore in the last analysis to narrate, actions

performed in the recorded past; and if they were performed according to, or even in search of, certain modes of validation, one is interested in their performance rather than their validity, and in the validations to which they appealed as the context that renders them the happenings they were. The questions “is this statement valid?” and “what has happened when it is made?” are not identical, unless—and this is the issue—the theorist who asks the former can oblige the historian who asks the latter to admit that nothing has been going on except the practice of a certain mode of validation; and this the questions asked by the “philosopher” have already rendered somewhat uncertain.

The historian, then, may be thought of as scrutinizing the actions and activity of political theory, and asking questions about what it has been and done, answers to which will necessarily take the form of narratives of actions performed and their consequences. The historian’s activity is clearly not identical with that of the political theorist. Before we go on to set these two activities in confrontation and interaction, it is desirable to ask whether “histories of political theory” have been or may be constructed, and what character they may possess. Here the focus of our enquiry shifts. A “history of political theory” would clearly move beyond the scrutiny of particular acts in the construction of such theory, and would suppose “political theory” to be and have been an ongoing activity, about which generalizations may be made and which can be said to have undergone changes in its general character over the course of time; changes which could be recounted in the form of a narrated history. There are, however, few such histories; few, that is, which are or may be called histories of political “theory” in any sense in which that term may be distinguished from, or isolated within, the “history of political thought” as the academic genre it has become. Histories of this kind are themselves indeterminate, in the sense that options exist and have been exercised as to what kinds of literature may or should be included in them, and it is a consequence that the terms “political thought” and “political theory” have often been used interchangeably, or with no precise attention to differences between them. The political theorist whose attention turns to history, therefore, is often confronted with historical narratives whose content bears little relation to the activity of “political theory” as it may have been defined. It is not unreasonable if such a theorist asks why such histories deserve attention.

3 HISTORIES AND THEIR PURPOSE

In the last forty or fifty years, canonical histories of this kind have fallen into disfavor (although there have recently been some signs of a revival¹). The best-known alternative in English, associated with the work of Quentin Skinner and others,² has taken

¹ For example, Coleman (2000); she might not accept the adjective “canonical.”

² Skinner (2002, i); Tully and Skinner (1988); Palonen (2003); Pocock (1962, 1985, 1987).

the form of a close scrutiny of the history—a key word has been “context”—in which texts and patterns of political discourse may be situated and said to have happened. It will be seen that the distance, mentioned earlier, between the questions asked by the theorist or philosopher, and by the historian, has grown wider. Historians of this school look upon the political literature of any period as composed of acts of speech or writing, articulations performed by authors in the language or diversity of languages available to them. These languages have histories; they can be seen in formation and in change; the performances of authors act in and upon them; and this is the sense in which they can be termed the primary “context” in which texts and debates happen in history. There are of course further contexts, the political, religious, social, and historical situations in which authors and their publics were situated; and what these were is to be discovered as much from the implications of their languages as from the researches of historians. What actors thought was happening is of equal importance with what historians think was happening; history is the study of subjective behavior.

In this multiplicity of “contexts”—both linguistic and situational—historians pursue the interactions between an author’s intentions, the language available for him or her to use, and the responses of those who read, or were informed concerning, the text and its author; the tensions between what an author “meant” to say and what a text “meant” to others, are often complex and productive of ambivalences. It may be the case that an author wrote in more than one “context” and was read in contexts other than those he intended. To give examples: *Leviathan* was written in both English and Latin, and one may differentiate between Hobbes’s intention and reception in a circle of philosophers in Paris, the court of the exiled Stuarts, the pamphlet-reading public in London, and the Dutch and German universities. The works of Machiavelli were written in manuscript for discussion groups in the politics of Florence, and it was by others after his death that they were released on the print networks of Europe, where they were read and responded to by other groups and publics, in ways it is not immediately certain he intended. The happenings of communication and performance are of primary concern to the historian, but not to the political theorist. The former is interested in what an author “meant” and in what a text “meant” to actors in history; the latter in what it “means” to a theorist, in the context of the enquiry she or he is conducting.

Works on the history of political thought, written in the above manner, tend to be microhistories rather than macrohistories, studies of particular performances, actions, and compositions, focused on the immediate context of the action rather than its long-term consequences. If confined—as there is no reason why they should not be—to a particular text or group of texts, and to the state of the language culture at the time these were written, they will be synchronous rather than diachronous in their emphasis; and it has been asked whether the contextualist approach is capable of supplying a history of contexts. This, however, can be done in several ways. The text and its author can be shown innovating in and acting upon the language in which the text is written, obliging the language to say new things and modify or reverse its implications. The text can be studied as it is read and responded to by others,

becoming what it means to them as distinct from what its author intended. Lastly, texts sometimes outlive both their authors and the contexts in which they are written, traveling both in space and in time to act and be acted upon in contexts of language and circumstance sharply unlike those in which they received their original meaning. There will now be the possibility of historical narrative, recounting both how the text underwent changes in use and meaning, perhaps and perhaps not continuing to convey its author's intentions in situations he cannot have foreseen, and how the language context underwent change for reasons not reducible to the intended performances of identifiable speech actors. It may even be possible—although it seems that it must be questionable—to supply unified “histories of political thought,” in which one pattern of consensus and challenge is progressively replaced by another, although recent *Cambridge Histories* have tended to present several such histories going on concurrently in contexts distinguishable from one another.³ If anything like the former canonical histories is restored, it will probably be the work of political theorists desirous of a usable past, rather than of historians not interested in supplying them with one.

4 THE ENCOUNTER RESUMED

To suppose a direct encounter between a political theorist and a historian, each engaged in studying the same text, we must make two assumptions. In the first place, we should suppose the theorist to be carrying out a programme of theoretical enquiry, possessing its own discipline and means of validating the statements it advances; this will enable us to juxtapose the theorist's propositions with those put forward by the historian, and enquire into any meeting or collision that may appear between them. In the second place—and here it is hard to avoid placing an additional burden on the theorist—we must suppose that the two actors are studying the same text, which has not been written by the theorist but by some other agent at some point in history. It is hard, although in principle not impossible, to imagine the historian studying a text written by a contemporary theorist as if it were a historical phenomenon. Historians are typically concerned with the past; they let time go by, during which evidence may assemble and perspectives emerge and alter. But once we suppose the theorist to be engaged with a text written by another hand, and itself a historical document, we must ask why this is happening, and what role a text written by another and—the historian instantly adds—in another context plays in the self-discipline and self-validating enterprise we have supposed the theorist to be conducting. The answer to our questions may emerge in literary and almost serendipitous terms. The theorist has, for whatever reason, read the historic text and finds its language to serve the purpose of some enterprise in political theory being conducted in the present; the

³ Burns (1988); Burns with Goldie (1991); Goldie and Wokler (2006).

language of the text is therefore presented as a proposition to be evaluated in the terms and by the criteria of the present enterprise. The historian now appears, asking questions and making statements concerning the intentions of the text's author and the meaning (a two-faced term) of his words in the context or contexts he and they occupied in history. In what ways, if any, will the propositions advanced by theorist and historian affirm or deny one another?

The theorist may assert that the author in the past was engaged in a programme of political theorizing identical with, or very closely resembling, that being conducted by the theorist in the present; so that the author's language may be quoted, cited, or paraphrased as language employed in the theorist's enterprise. The historian will scrutinize this assertion. We will suppose her or him capable of understanding a programme of political theory conducted in the present, as well as of reconstructing the languages in which programs of a similar kind have been conducted in past historical contexts. Such a historian will therefore be capable of pronouncing the theorist's assertion valid or invalid. If the former, the past author's language can be employed in the present theorist's enterprise without doing violence to the former (with which the historian, as historian, is primarily concerned); that is without doing violence to the past author's intentions or the meanings of the words used in the text. It is not in principle impossible that this will be the outcome of the historian's enquiry.

But the historian's business is with then, not now; with what the author was doing,⁴ with what was happening and happened when the text was written, published, read, and answered. The former's concern is with contexts, rather than programs; with the multiplicity of contexts in which the text may have had meaning and may have been intended; with the diversity of languages (or conceptual vocabularies) in which it will have been read and may even have been written (since authors are not incapable of recognizing multivalence and taking part in it). The theorist's reading of the text will therefore have been an act of selection, a decision to read the text as engaged in a particular program, even if the author proves to have made the same decision. The historian is interested in the multiplicity of the things that have happened and the contexts in which they happened, and will probably respond, even in the extreme case where it can be shown that an author wrote in only one language and was engaged in only one enterprise, by enquiring if that is the only way in which others read and have read that author's works. When texts outlive the historical situation in which they were first written and read, intended and understood, the likelihood of a diversity of effect becomes greater.

The theorist is performing an act of selection on grounds which are not those on which the historian acts. We have so far supposed a situation in which this selection raises no problems for the historian and is even acceptable as a historical statement about the text's or the author's "meaning," but it is methodologically interesting to move away from this supposition. Suppose instead that what the theorist is doing is less quotation than translation; a removal of the author's words from the meanings and implications they bore in a past historical context to those they may bear in a

⁴ Skinner (1978, i, xiii).

present context—one, that is, defined by the enterprise the theorist is engaged in rather than by any other language situation. The last stipulation implies that the enterprise is purely theoretical and is not being carried on into practice, since practice takes place in a world of multiple contexts and history. Given this condition, however, the theorist may still be asked why the historically distant text has been chosen as the subject of this act of translation. The answer may be that it has happened accidentally; the theorist happens to have read this text, and it happens that its language lends itself to this theoretical purpose. The circumstance that the author had similar intentions, or alternatively that his or her language can be so interpreted, is itself accidental; we are in a situation where history is accidental, or incidental, to theory. These hypothetical circumstances, however, entail different historical statements; the former is about the author acting in her or his moment in history, the latter about the action and moment of the theorist. The latter claims to be acting now, making a statement whose validity does not depend upon the historical context in which it is performed. It may be called positivist in the sense that it offers its own conditions of validation and appeals only to them.

This is of course wholly justifiable; it is valuable to set up laboratories and construct hypotheses subject to validation under rigorously controlled conditions. A common consequence of falsification, however, is the discovery that something was present which the experiment did not foresee or succeed in excluding, and here our theorist's enterprise may be the better for knowing its own history; what exactly are the conditions it specifies, and why does it specify these and not others? This question becomes all the more pressing as we enter the realms of practice and history, where the conditions under which, and the contexts in which, we operate can never be defined with finality. Here we pass beyond the simple dialogue between theorist and historian, beyond the problem of congruence between a text's meaning in the present and those it has borne in pasts. The historian has begun to resemble a post-Burkean moderate conservative, reminding us that there is always more going on than we can comprehend at any one moment and convert into either theory or practice. One has become something of a political theorist in one's own right, advancing, and inviting others to explore, the proposition that political action and political society are always to be understood in a context of historical narrative. There is room therefore for consideration of historiography as itself a branch of political thought and theory, literature and discourse.

The theorist, however, may be imagined using historical information, making historical assumptions either explicit or implicit, or reflecting upon historical processes as these appear relevant to the enterprise in political theory being conducted.⁵ The question now arises whether these operations are entailed by the method of framing and validating statements in which the theorist is engaged, or whether they are incidental or accidental to it. If the former, the theorist is claiming to make historical statements validated in either the same ways as those the historian practices, or in other ways which must be defined and defended. If the latter—and this the historian

⁵ Schochet (1994).

finds easier to imagine—the distinction between “political theory” and “political thought” has begun to disappear: that is, the former has begun to coexist with other modes of political discourse, and we are re-entering the historical world in which discourses interact, modifying, changing, confusing, and distorting one another. There are historians who study and narrate what goes on in this world; it is possible that there may be a “political theory” which addresses the same phenomena.

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CHAPTER 5

JUSTICE AFTER RAWLS

RICHARD J. ARNESON

IN the mid-twentieth century John Rawls single-handedly revived Anglo-American political philosophy, which had not seen significant progress since the development and elaboration of utilitarianism in the nineteenth century. Rawls reinvented the discipline by revising the social contract tradition of Locke, Rousseau, and Kant. A series of essays starting with “Justice as Fairness” in 1958 culminated in a monumental treatise, *A Theory of Justice* (Rawls 1999a [originally published 1971]). That theory of justice was in turn qualified and set in a new framework by an account of legitimate political authority to which Rawls gave a definitive formulation in his second book, *Political Liberalism* (Rawls 1996 [originally published 1993]). Rawls also produced an important monograph on justice in international relations, *The Law of Peoples* (Rawls 1999c). Rawls’s achievements continue to set the contemporary terms of debate on theories of social justice. This chapter comments on the present state of play in the political philosophy discussions that Rawls initiated and stimulated.

1 RAWLS’S THEORY OF JUSTICE IN A NUTSHELL

Rawls’s theory consists in an egalitarian vision of justice, specified by two principles, and the original position, a method for comparing and justifying candidate principles

of justice that is supposed to single out his proposed principles as uniquely reasonable. The vision is recognizably liberal in its striving to combine the values of equality and liberty in a single conception, and controversial both in the kind of equality that is espoused and in the particular freedoms that are given special priority. The principles are claimed to be ones that free and equal persons could accept as a fair basis for social cooperation.

The principles are as follows:

1. Each person has an equal claim to a fully adequate scheme of equal basic liberties, which scheme is compatible with the same scheme for all; and in this scheme the equal political liberties, and only those liberties, are to be guaranteed their fair value.
2. Social and economic inequalities are to satisfy two conditions: first, they are to be attached to positions and offices open to all under conditions of fair equality of opportunity; and second, they are to be to the greatest advantage of the least advantaged members of society (quoted from Rawls 1996, Lecture 1).

The first principle is called the *equal liberty principle*. In discussion, the second is often divided into its first part, *fair equality of opportunity*, and its second part, the *difference principle*.

The equal basic liberties protected by the first principle are given by a list: “political liberty (the right to vote and to hold public office) and freedom of speech and assembly; liberty of conscience and freedom of thought; freedom of the person, which includes freedom from psychological oppression and physical assault and dismemberment (integrity of the person), the right to hold personal property and freedom from arbitrary arrest and seizure as defined by the concept of the rule of law” (Rawls 1999a, 53). Roughly, the idea is to protect civil liberties of the sort that might well be entrenched in a political constitution.

The protection accorded to the basic liberties is augmented by the further stipulation that the first principle has strict lexical priority over the second. This means that one is not permitted to trade off basic liberties for gains in the other justice principle. In addition, fair equality of opportunity, the nondiscrimination principle, has strict lexical priority over the difference principle. The principles just stated make up Rawls’s special conception of justice. This conception does not apply at all historical times, but only when economic growth produces a situation in which the basic liberties can be effectively exercised. Rawls’s more general conception of justice holds that social and economic advantages must be arranged to be of greatest benefit to the least advantaged members of society.

The measure of individual benefits in Rawls’s theory is the individual’s holding of multi-purpose goods known as “primary social goods.” In *A Theory of Justice* these goods are defined as those it is rational for a person to want more rather than less of, whatever else he wants. In later writings, primary social goods are defined as goods that any rational person would strive to have who gives priority to developing and exercising two moral powers, the capacity to adopt and pursue a conception of the