Understanding Multilateral Institutions in Easy and Hard Times

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Abstract
In my scholarly work, I have sought to understand institutionalized multilateral cooperation in world politics, and the context of such cooperation: extensive economic interdependence, or globalization. What are the political implications of economic interdependence? Under what conditions are states facing globalization willing to share their authority with multilateral organizations over whose policies they exert only indirect and collective influence? I have developed interpretive frameworks and some theory to address these issues. In other work, I have helped to develop precepts for qualitative research design, and I have explored some normative issues associated with institutional accountability and legitimacy. My work on multilateral institutions, which was done in a period of increasing multilateral cooperation, is challenged by increasing inequality in capitalist democracies, financial crisis, and nationalist forms of populism. I am now seeking to understand the international and comparative politics of climate change, which I regard as an existential crisis.
INTRODUCTION

I was born in 1941, on the verge of America’s entry into World War II, and came to maturity during the height of what Henry R. Luce earlier that year had called the “American Century.” The Soviet Union launched the Sputnik satellite during my first few weeks in college, and the Cuban Missile Crisis took place while I was a second-year PhD student at Harvard University. When I was an assistant professor at Swarthmore College, I became an anti–Vietnam War activist, deeply engaged in the campaign of Eugene McCarthy for President in 1968. Concerns about world politics were deeply embedded in my identity, but, perhaps due to my activism on Vietnam, I never identified with the American foreign policy establishment or sought influence on American foreign policy. The story I tell in this essay is one of intellectual discovery rather than of policy judgments and accomplishments.

I focus on the central theme of my work: how to understand institutionalized multilateral cooperation in world politics. In the context of extensive global interdependence, why do states seek to share their authority with multilateral organizations over whose policies they exert only indirect and collective influence? What are the functions of these organizations and of the enduring rules and practices—political institutions—in which they are embedded? What costs are likely to be incurred by devaluing and destroying them? These issues have important implications for contemporary policy and for human security and welfare.

CREATING INTERDEPENDENCE AND COOPERATION

THEORY, 1970–1989

Having studied the United Nations as a graduate student, I was interested early in my career in institutions, although I did not have an analytical framework with which to study them (Keohane 1969). Around 1969, I became interested in the politics of the world economy, affected by the difficulties the United States was having in maintaining the value of the dollar relative to other currencies and in terms of gold. I taught a course on multinational corporations at Swarthmore with Van Doorn Ooms, a young economist who later had a distinguished career in Washington. Most fateful, in early 1967 I met Joseph S. Nye, then an assistant professor at Harvard, and soon thereafter we began to work together, pursuing what were then vaguely specified ideas about the roles of nonstate actors in world politics. We started by exploring transnational relations and then developed arguments about economic and political interdependence. Later I sought not only to describe multilateral institutions but to explain their existence and modes of operation.

The first step in my collaboration with Joe Nye was a conference and a special issue of International Organization on “transnational relations and world politics,” which was soon published in book form (Keohane & Nye 1972). The source of this volume was our curiosity about organizations such as multinational enterprises that seemed active in the world political economy but were invisible to students of international relations who defined their subject as interstate relations. Essays in this volume discussed the Ford Foundation, the Roman Catholic Church, the International Confederation of Free Trade Unions, and a variety of other organizations, viewed by us as transnational because they were nonstate actors engaged in activities across state borders.

Transnational Relations and World Politics engages in descriptive inference, with some simple typologies but no explanatory theory. For the purposes of my own intellectual trajectory, the most important aspect of this work came from the fact that studying transnational relations called the attention of Nye and myself to issues of dependence and interdependence, prompted by the...

1Life Magazine, February 17, 1941.
2Keohane
essays in the volume by Morse (1972) and Gilpin (1972), and by a recent book by Cooper (1968). We immediately began work on what became *Power and Interdependence*, first published in 1977 (Keohane & Nye 2012).

The analytical core of *Power and Interdependence* is its attempt to link economic interdependence, as discussed by scholars such as Cooper, with the concept of power, so crucial to traditional Realist theories of world politics. Nye and I used a conventional conception of power, associated with Dahl (1957) and Harsanyi (1962): “the ability of an actor to get others to do something they would not otherwise do (and at an acceptable cost to the actor)” (Keohane & Nye 2012, p. 10). However, we dissociated power from its common identification (in international relations theory) with force. For us, power did not emanate only from the barrel of a gun, but from industrial productivity and financial position and resources.

With respect to interdependence, we drew a distinction between “sensitivity” and “vulnerability” interdependence. What we called sensitivity interdependence involved “how quickly do changes in one country bring costly changes in another, and how great are the costly effects?” Policies to cope with sensitivity interdependence take the political framework as given. In contrast, we defined vulnerability interdependence as “an actor’s liability to suffer costs imposed by external events even after policies have been altered” (Keohane & Nye 1977, pp. 10–11). In making this distinction, we cited Waltz (1970). Our thinking also reflected Hirschman’s (1945) early work on what he called the structure of foreign trade, which we had read although we did not cite it. He identified how a large state can use the size of its market to exert political influence over smaller trading partners—what he called the “influence effect” of foreign trade (Hirschman 1945).

From a political standpoint, vulnerability is more fundamental than sensitivity since it takes power into account. Powerful actors can alter adverse political frameworks: They can set and reset the rules of the game. Vulnerability interdependence therefore includes the strategic dimension of interdependence omitted by a focus on sensitivity alone. The crucial proposition of *Power and Interdependence* follows from this analysis: asymmetrical vulnerability interdependence is a source of power in world politics. To understand power relations between two actors—such as the United States and China today—in a given issue area, one needs first to understand the structures that generated asymmetrical vulnerability.

Note that although this analysis could be considered “liberal” since it focuses on nonstate as well as state actors and examines how they interact, power is central to it. Indeed, we showed in *Power and Interdependence* that a sophisticated conceptualization of power, and its application to particular issue areas, is consistent with taking economic interdependence seriously. Our analysis was not at all idealistic—indeed, it was realistic in the social scientific sense—and power lies at its heart, tightly linked to economic analysis.

*Power and Interdependence* took some steps toward understanding cooperation and discord in world politics, but it did not arrive at an explanatory theory. Nye and I defined international regimes [a concept borrowed from Ruggie (1975)] quite loosely as “the sets of governing arrangements that affect relationships of interdependence” (Keohane & Nye 2012, p. 16). We sketched a theory of regime change based on changes in overall power, especially shifts in hegemony; and a modified theory focused on issue-specific power. In our view, these structural approaches provided the most parsimonious starting point for the explanation of regime change; they were valuable because they began with material resources that could be translated into political power. Once again, power lay at the heart of our theory.

In Chapter 6, we developed an analytical narrative of regime change in the issue areas of money and oceans between 1920 and 1970, which bears some resemblance to later historical institutionalism (Pierson 2004, Fioretos 2018). We recognized that this narrative fell short of an innovative
theory of change and that international structural theories fall short of complete explanation. Most glaring is their omission of domestic politics. They also omit variations in perception, inherently unpredictable bargaining strategies and outcomes, and institutional inertia: the tendency of established regimes and organizations to persist.

Having read extensively in the history of international political economy in the twentieth century, I began, while on leave at the Center for Advanced Study in the Behavioral Sciences at Stanford University in 1977–1978, to try to understand contemporary policy coordination issues as embedded in modern capitalism, and the biases of neoclassical economics in confronting these issues. I was influenced by the work of Polanyi, most notably *The Great Transformation* (1944). I wrote a biting critical essay (Keohane 1978) on a 1977 report for the OECD by a leading neoclassical economist, I participated in a project led by Goldthorpe (1985) on capitalism and the role of the state, and I was intrigued by Gramsci’s (1971) conception of ideological hegemony.\(^2\)

Although I was attracted to these political economy issues, I was ultimately more interested in how to think about world politics. Two years after Bull (1977) characterized world politics as “the anarchical society,” a powerfully logical book by Waltz (1979) also used the anarchy metaphor and developed a theory of “structural Realism” to explain interstate behavior. Kindleberger (1973) and Gilpin (1975) had developed arguments about a materialist form of hegemony, defined in terms of political-economic dominance. In a world of structural Realism and hegemony, the institutionalized policy coordination that we were increasingly observing in the late 1970s seemed an anomaly. How could it emerge consistent with the theory, or was something missing in the theory itself? Was leadership by a single state, based on dominance or hegemony, essential for cooperation? I did not understand the puzzle clearly, much less have an answer, until I attended a meeting at the University of Minnesota, organized by the economist Anne Krueger and sponsored by the National Science Foundation, at which Charles P. Kindleberger spoke about the implications for international relations of “transactions costs,” risk, and uncertainty. I had not even heard of transaction costs before this time, but when I returned to Stanford I began thinking about these issues, aided by friends and colleagues who knew about “the new economics of organization” (Coase 1937, 1960; Williamson 1965; Akerlof 1970; Williamson 1975). I can still remember the “aha” feeling that I had in December 1979, in my Stanford office, looking over the campus, when I recognized the significance of these theories for the understanding of international cooperation.

It took me four years of painful intellectual struggle to work out the argument for what became what I regard as my most important theoretical work, *After Hegemony: Cooperation and Discord in the World Political Economy* (Keohane 1984).\(^3\) In Chapter 2, I came to terms with connections between world politics and global economics, and in Chapter 3, I built on Realist and Marxian arguments to analyze the concept of hegemony and its relevance to understanding world politics. My specification of the requirements for hegemony was decidedly materialist. Hegemonic powers, I wrote, “must have control over raw materials, control over sources of capital, control over markets, and competitive advantages in the production of highly valued goods” (Keohane 1984, p. 32). Indeed, I identified my argument closely with that of non-Leninist Marxists. “My contention,” I wrote, “is that the common interests of the leading capitalist states, bolstered by the effects of existing international regimes (mostly created during a period of American hegemony), are strong enough to make sustained cooperation possible, although not inevitable” (Keohane 1984, p. 43). I discussed Gramsci’s (1971) notion of ideological hegemony but ultimately wrote that in world politics, “the potential for challenges to hegemonic ideology always exists” (Keohane 1984, p. 46).

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2 These notebooks were compiled by Gramsci, who died in 1937, and were published in 1971.
3 An early version of the argument is in Keohane (1982).
Notice the irony here. Although most of my analysis of cooperation is conventionally considered to be liberal, my conceptualization of hegemony relies on elements of both Realism and Marxism rather than on classic liberalism. According to my argument, hegemony rests on power and wealth, not persuasiveness or the attractiveness of one's system to others. My discussion of hegemony reveals how thin labels such as “liberalism” can be when applied with insufficient understanding of an author’s argument. I anticipated this labeling but did not forestall it when I wrote in the first chapter of *After Hegemony* (Keohane 1984, p. 13):

Is [my argument] “liberal” because I discuss cooperation or “mercantilist” because I emphasize the role of power and the impact of hegemony? Am I a “radical” because I take Marxian concepts seriously or a “conservative” because I talk about order? The simplenessidedness of such inferences should be obvious.

Chapters 8–10 pursue many of the issues raised in Chapters 2–3, discussing political economy issues in the period between 1945 and 1981. My original plan was to include these chapters in the middle of the book, relegating my theory to the end; but I decided that, whatever the logical merits of such an organization, placing dense empirical chapters in the middle of the book would make readers less likely to come to grips with my theory. I therefore put the key theoretical chapters in the middle—as rowing teams place their most powerful rowers in the middle of the boat. As a result, they received the attention I desired for them, and the empirical chapters were relegated to relative obscurity. Since I regard my theoretical contributions as more important than my empirical findings, which were historical and descriptive, I have always been pleased by this outcome.

The distinction that opens the door to the most innovative aspects of *After Hegemony* is the one I draw in Chapter 4 between harmony and cooperation. In situations of harmony, each agent’s policies, pursued without regard for the interests of others, are regarded by these other agents as facilitating the attainment of their goals. By contrast, cooperation arises from discord—a condition under which agents regard others’ actions as hindering their own goal attainment. Cooperation requires a process of policy coordination to overcome discord. Unlike harmony, cooperation is therefore inherently political, requiring mutual adjustment and often bargaining accompanied by the explicit use of power.

My basic explanation of international cooperation relied heavily on the theory of nonzero-sum games and especially Axelrod’s (1981, 1984) brilliant tournament, in which he demonstrated the robustness of reciprocity as an effective strategy in indefinitely repeated prisoners’ dilemma games. The key parameter for Axelrod is “the shadow of the future”. Only if participants sufficiently value long-term gains can reciprocity sustain cooperation. In the short run, each participant will benefit by reneging on promises to cooperate, but in the long run, retaliation by the other player will nullify these gains.

Yet I needed to explain not just sporadic cooperation but institutionalist cooperation. To do this, I turned to what might seem to be an odd source, the Coase theorem. Coase (1960) had stated that efficient solutions to externalities problems could be devised without regulations imposing limits on polluters, through market-oriented bargaining. I noticed that Coase had specified three crucial conditions for his theory to hold: an authoritative legal framework, perfect information, and zero transaction costs. Since “it is absolutely clear that none of these conditions is met in
world politics” (Keohane 1984, p. 87), I inverted the Coase theorem, using it to identify the key conditions that international institutions could provide, thus enabling bargaining that could move the parties closer to the Pareto frontier.

What followed was my “functional theory of international regimes” (Keohane 1984, p. 85). It was a functional theory in the sense that the anticipated effects of action explain the action (Keohane 1984, p. 80; Cohen 1978, p. 278). I had read critiques of functional theory, which are quite telling with respect to teleological forms of functionalism. However, functional theory defined in terms of anticipated effects seems entirely valid to me. Microeconomic analysis rests on functional theory in this sense; for instance, anticipated profits explain investment. As long as actors are forward looking and at least boundedly rational, functional theories are essential to make sense of their actions. The functional theory of After Hegemony holds that international institutions are created in a world of interstate politics without common government because their existence, within a legal framework, is expected to lower the transaction costs of bargaining and reduce uncertainty. Institutions can alleviate the negative consequences of asymmetrical information (Akerlof 1970) and help to prevent a political version of market failure—a situation in which mutually beneficial deals cannot be made due to problems of credibility, uncertainty, and the costs of bargaining.

One major implication of the theory is that the process of creating institutions is fundamentally different from the process of maintaining them, and much harder. Creating institutions involves large transaction costs of bargaining to arrive at a particular set of agreements, which then act as focal points that, as long as they are better than the noninstitutional alternative, have a tendency to stick.

My functional theory begins with individuals as agents, who have interests and the capacity to estimate with some degree of accuracy the consequences of their own strategies. It assumes that unless hindered by barriers such as asymmetrical information or asymmetrical power, more efficient solutions are likely to prevail over less efficient ones because they generate more gains for the actors involved. Yet the theory is deeply rooted in an understanding of power and informational asymmetries. Because these asymmetries are intrinsic to world politics, my theory does not predict efficient outcomes, although it identifies incentives to move in the direction of increased efficiency. As I argue in Chapter 7, my theory relies neither on perfect rationality nor on pure egoism but can be adapted to situations in which actors display bounded rationality and behave to some extent in empathetic ways toward others.

From the perspective of strategic advice, what stands out from my theory is the same lesson drawn by Axelrod (1981, 1984): Reciprocity can generate cooperation, insofar as the actors’ preferences are sufficiently convergent. However, for me, this benign result is likely only within the context of institutions that facilitate such cooperation, particularly by making it more sensible to put greater value on the future (Axelrod & Keohane 1985). In a 1986 article, I distinguished two forms of reciprocity: specific reciprocity, as in Axelrod’s tournament “in which specified partners exchange items of equivalent value in a strictly delimited sequence” (Keohane 1986, p. 4), and diffuse reciprocity, in which agents abide by generally accepted standards of behavior and expect others to do the same. I attributed the creation of multilateral trade agreements based on generalized norms to the insurmountable difficulties encountered by the practice of specific reciprocity in multilateral situations. Without being explicit about it, I was applying my functional theory to the analysis of trade institutions (Keohane 1986).

How well has the “cooperation under anarchy paradigm,” as it is often called, held up to subsequent analysis? The most compelling response has been in work by Fearon (2018), which builds on a much earlier paper by Powell (1991). Fearon develops a rigorous two-state bargaining model in which either state can use force, either to attack the other or to gain bargaining advantages on
nonterritorial issues. He finds that the Axelrod-Keohane reliance on a long shadow of the future indeed can promote cooperation in his model, but that the Axelrod-Keohane result only holds when military effort is above a certain level: Disarmament does not generate peace (Fearon 2018, p. 535). Fearon's conclusion that interdependence can have both positive and negative implications for conflict resonates with the intuitive conclusion of Power and Interdependence that interdependence will not make international conflict disappear: “On the contrary, conflict will take new forms and may even increase” (Keohane & Nye 2012, p. 7).

It is probably a general rule in social science that there are no unconditional laws: One always asks under what conditions certain outcomes will appear. Nye and I were right to see the conflict potentials of interdependence as varying depending on other conditions in world politics. It seems now that Axelrod and I correctly identified important conditions for cooperation—a long shadow of the future, often enhanced by institutionalization—but that our analysis was incomplete since it did not fully take into account the effects of anarchy combined with the option of military force.

A more serious deficiency of my work in this period was my persistent overlooking of domestic distributional politics, despite the urgings of students such as Helen Milner and Andrew Moravcsik. I recognized that domestic politics was important but did not know how to incorporate it into my theory in a theoretically coherent way. Ruggie's (1982) concept of “embedded liberalism” was more insightful about connections between institutionalized multilateralism, on the one hand, and domestic politics, on the other (see also Helleiner 2019). Probably I was too much under the spell of Kenneth N. Waltz, whose work I targeted for substantive critique but whose theoretical parsimony I admired and whose systemic perspective I adopted.

INSTITUTIONAL THEORY IN EASY TIMES, 1990–2008

Critiquing Conventional Approaches and Identifying Change

The end of the Cold War led to a burst of optimism on the part of scholars of international relations. A chorus of voices proclaimed the triumph of democracy and the “end of history” (Fukuyama 1989, 1992; for a critique see Mearsheimer 2018). I refrained from joining this particular liberal choir. I have always regarded democracy as difficult to construct and to maintain, and therefore unlikely to appear merely because a particular imperial despotism, such as that of the Soviet Union, disappears.

The direction of change that I did anticipate was toward increasing interdependence (now known as globalization), the proliferation of international institutions that came about in the 1990s, and increasing legalization as exemplified by the creation of the World Trade Organization (WTO) in 1995. By one scholarly count, the number of intergovernmental organizations in the world rose from less than 100 in 1949 to about 350 in 2000 (Eilstrup-Sangiovanni 2018), appearing to vindicate my functional theory of international institutions. Interdependence was rising, generating more functions for international organizations to perform, and the numbers of such organizations rose accordingly. Prospects for international cooperation seemed relatively unconstrained by international structures or domestic politics. The 1990s are the “easy times” of my title.

Responding to a critique of liberal institutionalism by John Mearsheimer, I engaged in a much-hyped “neorealist versus neoliberal debate” (Baldwin 1993, Mearsheimer 1994–1995, Keohane & Martin 1995), in which I argued that contemporary events vindicated my liberal institutionalist perspective.5 In my view, although Realists made shrewd observations about world politics

5In popular depictions of this debate, important and controversial issues became overly stylized. As my comments in the previous section suggest, I do not accept a dichotomy between “Realist” and “liberal” modes of
under classical conditions, they had not adapted sufficiently to the contemporary world. Although I never adopted the view that democracy was destined to triumph as a political system or that peace was the natural condition of world politics, my commitment to liberal institutionalism made me susceptible to optimistic interpretations of changes taking place. These interpretations helped to generate three research projects—on reputation, the role of ideas, and legalization.

The biggest problem with institutionalizing cooperation, as Realists had long emphasized, was the lack of centralized enforcement in an anarchic international system (Waltz 1979). My conjecture was that perhaps this deficiency could be rectified, insofar as the shadow of the future was long, by concerns about reputation. According to this notion, even great powers such as the United States might find it expedient to keep their commitments in order to maintain their reputations for trustworthiness. I spent much of my time between 1988 and 1995 writing a book-length manuscript designed to substantiate this idea. Tentatively titled *Contested Commitments in United States Foreign Policy, 1783–1989*, this work focused on commitments made by the United States, typically in the form of treaties, whose fulfillment was later challenged by major actors within US domestic politics. My purpose was to show that these commitments, and therefore the international institutions in which they were embedded, had significant impacts on American foreign policy even under these adverse conditions.

The research I conducted relied on the secondary historical literature, so it did not provide new knowledge except for the evaluation of my conjecture. Alas for this project, my research did not support my hypothesis. In the eighteenth and nineteenth centuries, the United States reneged on commitments to weak powers (Spain, Mexico, and Native American tribes) when convenient, although the United States was more reluctant to renge on commitments to the powerful United Kingdom. During the Cold War, the United States reneged on commitments in the Caribbean, to Taiwan, and sometimes to its allies. Reputation seemed to be a weak reed after all. I consulted the editor of a major university press whom I trusted and took his advice not to continue to pursue the project.

More successful was a project on the role of ideas in world politics that Judith Goldstein and I launched after participating in an informal faculty discussion group in 1987–1988, when I was again a Fellow at the Center for Advanced Study in the Behavioral Sciences at Stanford. If structural constraints were less severe than Realists believed, human cognition—including new ideas about how to act—could be of great importance in world politics. Eventually, Goldstein and I organized two conferences on the role of ideas in foreign policy, supported by the Social Science Research Council, and published an edited volume (Goldstein & Keohane 1993).

*Ideas and Foreign Policy* began with the assumption of rational behavior as a theoretically useful simplification of reality “but not a true reflection of it.” We adopted the pure rationality assumption as a null hypothesis, by which “variation in policy across counties, or over time, is entirely accounted for by changes in factors other than ideas” (Goldstein & Keohane 1993, pp. 5–6, emphasis original). We created a typology of three types of beliefs: world views, exemplified by religions and by scientific rationality; principled beliefs, which distinguish right from wrong; and causal beliefs. We then proceeded to sketch three possible causal pathways by which ideas could potentially affect policy outcomes. Ideas, we argued, could serve as road maps; illustrations included Keynesian economics and or post–World War II reservations in the imperial powers about colonialism. They could also serve as focal points, helping to solve coordination problems (Schelling 1960), as illustrated by the idea of mutual recognition in the European Community during the 1980s. Finally, ideas could help shape institutions, such as law, which could then exert powerful effects, as
illustrated by the idea of sovereignty and its impact on sovereign statehood. This typology helped me think more clearly about the role of ideas in world politics, although it is only a typology, not a theory. The most important impact of Ideas and Foreign Policy probably derived not so much from the volume itself but from the impact of our discussions on the later path-breaking work of several contributors, who included John Ferejohn, John Ikenberry, Peter Katzenstein, Stephen Krasner, Kathryn Sikkink, Geoffrey Garrett, and Barry Weingast.

From thinking about reputation and about the role of ideas in world politics, it was a short step to rethinking the role of law, influenced by my friend Anne-Marie Slaughter. I had been taught in the 1960s to be quite skeptical of the role of international law, partly as a result of aversion to the overly idealistic and legalistic works of the 1950s that heralded “world peace through world law” (Clark & Sohn 1958). When Slaughter and others drew my attention to international law in the 1990s, I continued to see it less as an independent force than as dependent on politics for its impact. The clearest pathway to the efficacy of law runs through interests, so a political theory of international law links closely to an interest-based theory of international cooperation such as my own.

Since no universal supranational state exists in world politics, enforcement must be decentralized and operate principally through some form of institutionalized reciprocity or because law and its benefits have shaped domestic interests in ways that make interests more compatible with one another. That is, even interests that are discordant in the short run must be trumped by long-term considerations of self-interest mediated by reciprocity or concerns about reputation; or preferences must change as a result of legal practice, experience of benefits, and socialization.

Political support for effective legalization must be stronger than support simply for more episodic cooperation, since legalization implies precise, binding rules that are more difficult than vaguer agreements to evade and because legalization often involves delegation of authority to international courts that are not directly accountable to sovereign states. My colleagues and I developed these and related arguments in a 2000 volume of International Organization, later reprinted as an independent volume (Goldstein et al. 2001).

Since legalization is politically demanding, its success is conditional on favorable political conditions (Keohane 2002, ch. 6). In the 1990s, conditions were indeed favorable, and we observed advances in legalization as well as increases in the number of intergovernmental organizations. Since the beginning of this century, the growth in the number of intergovernmental organizations has ceased (Eilstrup-Sangiovanni 2018), and there has been increasing opposition to the spread of international law and the authority of non-national courts, most notably in the referendum in Great Britain to exit from the European Union. In our studies of legalization, we did not anticipate this reversal.

Other reversals derived from domestic politics. During the easy times of the 1990s, Helen Milner and I explored the domestic politics of what we called internationalization, later known as globalization (Keohane & Milner 1996). We looked at the impact of internationalization on domestic politics, showing in an edited volume that the fundamental institutional innovations of this period, including the creation of the WTO in 1995, had major impacts on domestic politics. We correctly anticipated that internationalization would make major capitalist economies more vulnerable to externally generated shocks. We also argued, I think correctly, that internationalization would more seriously constrain left-wing than right-wing governments because capital is more mobile than labor. In a globalized world economy, capital can gain concessions under the threat of capital flight, while labor has no such option (Keohane & Milner 1996, pp. 18–19). All of these impacts would adversely affect social democracy in open economies, as Ruggie (1982) had anticipated and Rodrik (2011) later argued in much more detail. In retrospect, we should have more seriously followed the lead of Frieden & Rogowski (1996), whose essay in our volume...
examined more closely the domestic distributional effects of globalization. These effects, as I note below, have turned out to be adverse to the working classes in advanced industrial societies.

In the 1990s I also edged my way into the analysis of environmental politics, both because I have long been an environmentalist and because the creation of new international environmental institutions was intriguing. *Institutions for the Earth* (Haas et al. 1992) and *Institutions for Environmental Aid* (Connolly & Keohane 1996) applied the conception of international regimes discussed in *Power and Interdependence* and *After Hegemony*. In essence, my colleagues and I discovered that international environmental regimes had been instituted in ways that were broadly consistent with functional theory. The work generalized regime theory to environmental issue areas but did not break new theoretical ground.

More significant for understanding the collective action problems inherent in environmental issues was a project on “local commons and global interdependence” that I organized with the late Elinor Ostrom, a political scientist who won the 2009 Nobel Prize in economics (Keohane & Ostrom 1995). In reading her path-breaking volume, *Governing the Commons* (Ostrom 1990), I had noticed the similarity between her design principles and my own emphasis in *After Hegemony* on reciprocity. So began a correspondence that led to an intellectual partnership. The resulting volume compared collective action problems across scales, from local to global commons, focusing attention on the number and heterogeneity of the actors involved and their effects on cooperation. In both domains, institutions—generated by incentives and changing incentives—played a major role.

In these easy times, my colleagues and I succeeded in identifying new patterns of world politics, in particular the roles played by multilateral institutions, ideas, legalization, and attempts to provide global public goods. We failed to show that these trends were fundamentally transforming world politics, and we did not anticipate reversals to these developments in the twenty-first century. Our crystal ball was clouded because we lacked a strong theory of change.

**Normative Issues: Accountability, Legitimacy, and Enhancing Democracy**

I have always been interested in normative political theory, so it was not surprising that the opportunities apparently available at the beginning of the twenty-first century led me to reflect on normative issues in world politics.

Institutionalization and legalization raise issues of accountability and legitimacy, since to be legitimate in a democratic age, international institutions need to have procedures for accountability. More generally, international institutions generate questions about their compatibility with democracy itself. These issues were salient in the first decade of this century and are even more salient now with the debate in the United Kingdom about Brexit. Taking advantage of having been on the Duke University faculty and then at Princeton University, I wrote a trilogy of papers on these subjects with colleagues at these institutions. All of these papers are exercises in normative international political theory, consistent with classical liberal theory in their emphasis on the value of certain types of nonmajoritarian governance institution.

Ruth Grant and I focused on accountability in a 2005 article. “Accountability, as we use the term, implies that some actors have the right to hold other actors to a set of standards, to judge whether they have fulfilled their responsibilities in light of these standards, and to impose sanctions if they determine that these responsibilities have not been met” (Grant & Keohane 2005, p. 29). We distinguish two general models of accountability for nation-states: a participation model, in which agents affected by powerful actors are entitled to hold those actors accountable, and a delegation model, in which those who authorize authority are the accountability holders.
We argue that the relevance of the participation model is somewhat limited by the absence of a global public. Although issues of participation are important to some extent, in our view accountability mechanisms in world politics should be designed principally to prevent abuses of power rather than to achieve the illusory goal of participatory democracy. We should “be seeking to create processes for checking abuses of power with the full recognition that every type of power is subject to abuse” (Grant & Keohane 2005, p. 41). We describe seven accountability mechanisms to limit abuses of power in world politics: hierarchical, supervisory, fiscal, legal, market, peer, and public reputational.

“The Legitimacy of Global Governance Institutions” was coauthored with Allen Buchanan, an eminent philosopher at Duke. In this paper, we begin with a clear distinction between normative and sociological meanings of legitimacy. “To say that an institution is legitimate in the normative sense is to assert that it has the right to rule... An institution is legitimate in the sociological sense when it is widely believed to have the right to rule” (Buchanan & Keohane 2006, p. 405). Both conceptions of legitimacy therefore have a normative basis, which we explore in the article by specifying the criteria of a global public standard for legitimacy. Three of these criteria are substantive: (a) minimal moral acceptability; (b) comparative benefit—that the institution be preferable to feasible alternatives; and (c) institutional integrity. Institutions must also meet epistemic standards: They must be accountable in the sense used by Grant & Keohane (2005), reasonably transparent, and oriented toward encouraging “ongoing, principled contestation of their goals and terms of accountability” (Buchanan & Keohane 2006, p. 432). This article builds on and extends the argument made by Grant & Keohane (2005).

The third paper in this early-twenty-first-century trilogy takes the institutional argument further, perhaps too far. In “Democracy-Enhancing Multilateralism,” Stephen Macedo, Andrew Moravcsik, and I argue not only that multilateral institutions are pragmatically valuable in a world of globalization but also that they can actually enhance democracy, despite not being highly participatory. We do so by developing a constitutional conception of democracy, in which elections are necessary but not sufficient for democracy. Fair elections, we point out, require institutions to ensure fairness; nonmajoritarian institutions such as politically independent courts and agencies with expertise are essential to check potential abuses of power and protect individual rights; and procedures for public deliberation must be built into the system. “Well-designed constitutional constraints enhance democracy, understood as the ability of the people as a whole to govern itself, on due reflection, over the long run” (Keohane et al. 2009, p. 6).

Equipped with this institutionalist conception of democracy, my coauthors and I consider the impact of multilateral institutions in various issue areas. We argue that shifting trade policy toward multilateral institutions reduces the power of domestic special interests, putting power into the hands of more insulated leaders, providing information that enhances the quality of democratic deliberation, and leaving more issues to impartial international adjudication. What we do not sufficiently address is the extent to which such insulation of leaders and emphasis on adjudication transfer authority from democratic publics to a transnational elite, acting in its own interests.

**RE-EVALUATING GLOBALIZATION IN HARD TIMES, 2009–2019**

The financial crisis of 2007–2009 (Tooze 2018), the rapid rise of Chinese power in the second decade of the twenty-first century, and the election of Donald J. Trump as US President have dealt severe blows to multilateralism, cosmopolitanism, and the promotion of liberal political values such as human rights. No longer does the arc of history seem to bend toward constitutional democracy, increased globalization, and multilateralism. The Democracy Report of Freedom House has
recorded thirteen straight years of decline (Freedom House 2019); world trade plunged in 2009, and since that year has only kept pace with GDP growth rather than comfortably exceeding it (WTO 2018, p. 28); and the number of international organizations peaked in the first decade of this century before leveling out (Eilstrup-Sangiovanni 2018). Even more striking, there has been an upsurge of populist, anticosmopolitan movements in Europe and the United States during the last decade. Populist and nativist parties have made electoral gains in the core countries of the European Union—France, Italy, Germany—as well as on the periphery, and have done so in the United States as well. Prime Minister Theresa May of the United Kingdom said at the Tory Party Conference in 2016 that “if you believe you’re a citizen of the world, you are a citizen of nowhere.”

We find ourselves in hard times. This shock should make us reflect on the backlash against increasing interdependence, larger roles for intergovernmental institutions, and cosmopolitan elites.

With respect to many outcomes, the last 30–40 years have seen major advances. There has been no great power war, and rapid economic development, especially in Asia, has led to improvements in the health and welfare of billions of people. GDP per capita (in purchasing power parity terms) in 2017 was almost 17 times as great as in 1990 in China, and more than six times as great in India (World Bank Data Group 2018). The greatest beneficiaries of global growth have included people who in 1980 had incomes between the tenth and fiftieth percentiles. Yet the very rich have also benefited; indeed, between 1980 and 2016, the richest 1% captured more than twice as much additional income over that period than those who had been in the bottom 50% (World Inequality Lab 2018, figure E 4).

The backlash against globalization seems to have had several causes, including opposition to large-scale immigration from poor countries; but declining economic conditions for middle-class and working-class people in wealthy countries seem to be part of the story. In the United States, rates of growth in household income, which were similar for those at the ninety-fifth, fiftieth, and twentieth percentiles between 1945 and 1980, began to diverge sharply around 1980, with the rich capturing almost all of the gains thereafter (Stone et al. 2018, figure 1). The social democratic compromise of embedded liberalism broke down in the United States just when Ruggie (1982) articulated it. Again, Hegel’s Owl of Minerva spreads its wings at dusk.

Liberalized trade, particularly after China’s entry into the WTO, accelerated both the decline of poverty in Asia and income stagnation for Middle America, and it seems to have driven some of the antiglobalization backlash, at least in the United States. Autor et al. (2017) find evidence that congressional districts most affected by trade shocks became more polarized, and that polarization helped more extreme right-wing candidates in majority-white districts while pushing Democrats to the left in majority-minority districts. Overall, trade shocks seem to have helped right-wing populists (Autor et al. 2017).

Those of us who celebrated globalization for its impact on overall prosperity and peace did not sufficiently take these economic and political effects into account. WTO arbitration procedures did not ensure that trade policies or arbitral rulings would be fair; nor did the WTO encourage vigorous compensation that would have maintained embedded liberalism. Investment treaties signed in the 1990s enabled corporate lawyers to gain advantages at the expense of consumers; interjurisdictional tax competition meant that corporations such as Apple could get away with paying minimal corporate taxes; and wealthy individuals could also take advantage of tax havens. In a globalized society, transnational mobility becomes a competitive and bargaining advantage—which accrues overwhelmingly to the rich. As Hirschman (1971) argued half a century ago, the possibility of exit magnifies voice: Wealthy, mobile corporations and individuals gain outsized

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6The Telegraph, October 5, 2016.
advantages in politics. I was well aware of Hirschman’s argument and should have taken its distributional implications more seriously.

Looking back at my work from the perspective of 2019, I think that my analysis did not adequately take into account the predatory aspects of capitalism, combined with the power and privilege that would accrue to multilateral corporations and billionaires from globalization. I continue to believe that multilateral institutions promote cooperation as my theory claims. However, elites have benefited disproportionately from this cooperation. As Jeff Colgan and I wrote in 2017, the system was rigged (Colgan & Keohane 2017).

DESIGNING SOCIAL INQUIRY AND RESEARCH DESIGN

In this essay I discuss one work that does not analyze global politics: my book with Gary King and the late Sidney Verba, Designing Social Inquiry: Scientific Inference in Qualitative Research (King et al. 1994). Writing this book with King and Verba gave me a language in which to interpret my qualitative and historical work as scientific despite its lack of numbers and math.

Designing Social Inquiry was not generated by puzzles of world politics. Instead, it was the result of serendipity. Sid Verba and I were friends, and when I joined the Harvard Government Department in 1985, he said that we should teach a course together. I regarded this remark as a welcoming pleasantry, typical of Sid’s grace and warmth. Three years later I became chair of the department and in my first year as chair was forced to listen to 24 job talks. Most of these talks were dead on arrival, since the speaker had made fundamental mistakes in research design. I complained to colleagues, including Sid, and Gary King. Gary said the three of us should teach a course on research design together. I had never taken or given a PhD-level seminar in research design, but Harvard had the resources and wisdom to give full teaching credit to faculty teaching jointly, and I was sure I would learn from Gary and Sid. So I agreed, and we taught the course the following year.

The course was fun to teach. We enjoyed each other’s company, I learned a lot, and the students seemed to benefit as well. After the semester was over, Gary said: “We should teach the course again. And this time, we should write a book on this subject.” The next year we met regularly for a bag lunch, discussing not only themes of the course but drafts that one of us—most often Gary, which is why his name appears first on the book—had produced.

The principal theme of Designing Social Inquiry is announced by its subtitle: “scientific inference in qualitative research.” When we wrote the book, it was generally assumed that only quantitative research could be scientific. Qualitative research was anecdotal, relying on the ineffable instinct of the researcher. Our book, in contrast, argued that the same logic of inference is the basis for both the best quantitative and the best qualitative research. Quantitative researchers, such as Sid and Gary, had explicated and formulated principles of research design, while qualitative researchers had not done so. Our task was to adapt the principles of research design used in quantitative work to qualitative political science.

The key example that opens the door to a scientific qualitative analysis is a unique event: the sudden extinction of the dinosaurs 65 million years ago. This event “does not fall neatly into a class of events that could be studied in a systematic, comparative fashion through the application of general laws” (King et al. 1994, p. 11)—yet natural scientists have studied it scientifically and have devised tests of theories that purport to explain it. Science often benefits from quantitative analysis, but the scientific method does not depend on having large numbers of cases to examine.

Theory is crucial in good social science research because it systematically generates observable implications that scientists can evaluate systematically and insert into a coherent broader pattern of
knowledge. The best research often begins with a puzzle and a conjecture rather than a large data set. Conjectures can be elaborated, with the help of existing scientific knowledge, into theories. The next design step is then to identify “observable implications” of a given theory: what will be true if the theory is correct. The investigator should devise theories that are “capable of generating as many observable implications as possible” (King et al. 1994, p. 19).

Instead of mining data for correlations, we recommend beginning with theory, using it to generate multiple, dependent variables. The investigator should consider the observable implications of the theory—potential dependent variables—in any domain, not necessarily the one in which the investigation started. In the dinosaur extinction example, scientists found clues to the cause of the extinction event by inspecting the Earth’s crust for the chemical element iridium, guided by a theory implying that a cosmic catastrophe would have left traces of iridium in the Earth’s crust (King et al. 1994, p. 11; Gould 1989, p. 318). A sufficiently precise theory with an ex ante prediction could be rigorously and precisely tested with a particular and limited set of observations.

Focusing on the observable implications of a theory has many productive implications. It enables researchers to maximize leverage: to explain “as much as possible with as little as possible” (King et al. 1994, p. 29). It teaches us, insofar as possible, to select observations on our explanatory variables, multiplying the number of dependent variables, which constitute the observable implications of a theory. Finally, emphasizing the observable implications of a theory should lead the investigator to be dissatisfied with mere correlation but instead to articulate the causal pathways posited by her theory, so that these implications indeed follow logically from the theory.

Throughout the book, we recognized that it is more difficult in qualitative than in quantitative research to overcome the fundamental problem of causal inference. One can never observe both what happened and the counterfactual: what would have happened in the absence of the hypothesized explanatory variable. We did not, therefore, simplistically or in a patronizing way advise qualitative researchers mechanically to follow rules established by quantitative researchers, or to abandon interesting and important problems because they could not handle them with large-n data sets. We tried instead to provide helpful advice to qualitative researchers seeking to understand important problems in as scientific a manner as possible.

Two aspects of contemporary social science that are distressing to me are the relative absence of interesting theory and the exclusion of certain important problems from scientific analysis on the grounds that neither rigorous quantitative research, nor experiments, can be conducted on them. Graduate students are encouraged to do experiments on individuals, conduct network analyses, and study relationships among identifiable variables, using ever more sophisticated quantitative techniques. All of these methods can be helpful, but they all limit the targets of study, and the emphasis on methods often seems to crowd out creative theorizing. At the same time, students are discouraged from studying political organizations and path-dependent changes in organizations, and institutions more broadly, on the grounds that one cannot clearly and quantitatively identify causality. Yet political organizations, from bureaucracies to political parties to international organizations, lie at the heart of politics. If scholars were more willing to employ the key insight of Designing Social Inquiry—that we should look for the observable implications of theory in any domain, with qualitative as well as quantitative measurement—we could create opportunities for enlightening new theory and reopen essential domains for analysis.

**CONCLUSION: THE EXISTENTIAL POLITICS OF CLIMATE CHANGE**

Political science has two missions: to help us understand politics better and to improve the world. We can improve the world through theory and science, if this work opens doors to reflection, understanding, and reform. Much of my work has had that long-term objective. We can also
improve the world more directly through the analysis of pressing, immediate policy issues such as income inequality and climate change. I am increasingly drawn to making a difference in this way.

Beginning during the first decade of this century, I became convinced that climate change is not just another environmental problem; it is the existential problem of our age. Climate change is already sending huge numbers of species to extinction and could threaten human civilization; yet the human race keeps increasing its emissions of gases, especially carbon dioxide, that generate climate change. Policy responses have been anemic. The international regime for governing climate change policy is weak and fragmented (Keohane & Victor 2011, Keohane 2015). On current trajectories, we are becoming the authors of our own extinction.

Political science has been shockingly oblivious to this existential crisis. Very few senior political scientists study this subject, and few articles on it have appeared in the top political science journals. Graduate students are discouraged from studying it since it has not reached so-called mainstream status. Admittedly, it is difficult to study the politics of climate change because fears of climate disaster are only now beginning to generate salience in electoral politics or to stimulate mass movements. Political scientists will have to look harder for signals of political movements and use their imaginations to generate scenarios of strategic interaction based on both material interests and ecological values. In this difficult challenge, there are opportunities for young, creative political scientists to develop new theories and modes of analysis that can make a difference in the world.

Rather than write one more book on international institutions, I have turned my attention to trying to bring climate change into the main arenas of political science by motivating others to work on this crucial topic. Taking advantage of the Balzan Prize, which I received in 2017, I organized a group of younger political scientists—all born in 1977 or later—in a project on “the comparative and international politics of climate change,” with annual meetings planned (2018–2020) at the Center for Advanced Study in the Behavioral Sciences at Stanford. I stressed the comparative method because there is considerable variation in political responses to climate change, and comparative analysis is crucial to understanding the sources of this variation. International agreements on climate change are limited, in my view, more by the lack of intense pressure for action in many countries than by failures of international institutions and negotiations.

Five research projects have emerged. Two of them operate at the individual level of analysis, using survey experiments to study effects of gender and of compensation schemes. One is at the country level, seeking to describe and explain intercountry differences in levels of effort and types of policy; one is focused on the distributitional politics of renewable versus fossil fuel resources in a variety of political units; and one seeks to compare policies of oil and gas companies. I am working more and more with natural scientists and engineers to understand the barriers to rapid decarbonization as well as opportunities to promote decarbonization, and in particular to understand political barriers and how to overcome them through innovative political strategy.

It is clear from the reports of the Intergovernmental Panel on Climate Change, convened under the auspices of the United Nations, both that the consequences of raising global temperatures by more than 1.5°C or 2°C will be dire and that the world is headed precipitously in that direction. It is also increasingly clear that technologies are being invented that hold the promise of creating an industrial economy with zero net emissions of carbon. It seems likely that the key barriers to solving this problem will be political rather than technical. Decarbonization will generate losers as well as winners, and potential losers will fight hard to retain their positions. In my view, a massive social science effort is needed to understand both the nature of these barriers and how to devise strategies to overcome them. I plan to devote what remains of my social science career to helping generate work on the comparative and international politics of decarbonization and to creating an epistemic community devoted to understanding this existentially important subject.
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