

John Kingdon and the Evolutionary Approach to Public Policy and Agenda-Setting

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Political science in the 1960s was riven by disputes that were at the same time intellectual and political. E.E. Schattschneider's (1960) proposal that the scope and dimensions of political conflict were themselves the result of political mobilization led to various discussions of agenda-control, agenda-setting, and agenda-building (see Cobb and Elder 1972, Cobb Ross and Ross 1976). Bachrach and Baratz' (1962) introduction of the concept of the "second face of power" further fanned the flames of radical criticism, soon picked up by Jack Walker (1966) in his "Critique of the Elitist Theory of Democracy." As described by Bryan Jones in the previous chapter in this volume, Roger Cobb and Charles Elder (1972) were the first to produce a book-length study of the agenda-setting process, and John Kingdon's book followed in 1984 (with later editions in 1995 and 2010). In this short essay I put Kingdon's contribution in its historical context with a focus on the reasons why the book may have come about when and how it did, paying particular attention to the intellectual community and context at the time of its development. I will not review the extensive literature that has followed on Kingdon's contribution or even focus so much on the theory itself, as several reviews have recently appeared that do exactly that. For example, Zahariadis (2014) explains the development of the multiple streams literature; Greer (2015) summarizes the contribution of the work; Cairney and

Jones (2015) discuss the intellectual impact of Kingdon’s work on future scholars (including my own development with Bryan Jones of punctuated equilibrium theory); and Jones et al. (2015) provide a comprehensive meta-analysis of works using the multiple streams approach. With almost 14,000 citations as of September 2015, and with these four recent and highly prominent reviews, it is clear that the book has had a major impact, and that there are many sources available for scholars interested in learning why.¹ My focus therefore will be quite different. I focus on where the book came from.²

The Behavioral Study of Political Elites

In the period before the publication of this 1984 book, Ann Arbor, like many other university environments, was awash in discussion of policy communities and the interesting jobs of high level bureaucrats. Hugh Heclo (1974) had discussed the idea of how policymakers in and around governments engaged in “collective puzzlement on society’s behalf” and then later (1978) described the emergence of highly complex “issue networks” which he believed had already displaced the smaller and more easily understood issue subsystems or iron triangles of the past. While scholars used many different phrases for the term, there was clearly a return to what Ernest Griffith (1937) had called “whirlpools” – communities of experts, in and around

¹ With 30 years of hindsight since the publication of *Agendas*, it is clear that the book has great staying power and impact. However, at the time of publication, this was not guaranteed. Shortly after Kingdon published his book, Nelson Polsby published *Political Innovation in America* (1985). Polsby’s book was initially given much more attention than Kingdon’s, including high profile and laudatory reviews in such places as the *Los Angeles Times*, the *New York Times Book Review*, and the *Washington Post*. That book, while engaging and readable, has not had the long-term impact of Kingdon’s work. A google scholar search in September 2015 shows 480 citations; certainly a good number but far from the 13,755 found for *Agendas, Alternatives and Public Policy* at that same time.

² I studied in Ann Arbor from 1976 through 1986 and have long been close to Kingdon; he has been one of my closest mentors in the profession throughout my career. But it should be clear that if he had written this chapter his own recollections might differ from my reconstructions. After drafting this article, however, I did send it to him for comments and he provided a number of improvements and corrections. Thanks to John, to former Kingdon research assistants (and my graduate school colleagues HW Perry, Roy Meyers, and Mark Peterson), as well as to Tom Gais, Bryan Jones, Andrew McFarland, Joel Aberbach, John Creighton Campell, Robert Putnam, Robert Axelrod, and Kirsten Kingdon for comments.

government, who puzzled and strategized together on various issues of public policy. Whereas the behavioral revolution in political science is often referred to as one relating to the study of voting, and today my colleagues often refer to political behavior as a synonym for the study of voters and their attitudes, the impact of behavioralism in Ann Arbor was felt not only in the National Election Studies or at the Center for Political Studies.

At Michigan a cohesive, close, and powerful group of younger scholars focused not on voting, but on the behaviors of elites in politics. Department Head Sam Eldersveld brought Pat Crecine from Carnegie Mellon to head the revamped Institute of Public Administration, soon renamed the Institute of Public Policy Studies (and eventually transformed into the Gerald R. Ford School of Public Policy). Crecine brought with him a concern for bounded rationality and the behavioral study of decision-making in government settings, similar to the works of Herbert Simon, Richard Cyert, and James March (see Simon 1947, 1985, Cyert and March 1963).

Kingdon was familiar with the Carnegie school through his graduate training in organizational theory with Rufus Browning at Wisconsin. A new generation of scholars sought to revolutionize the study of public policy and public administration just as the mass-oriented behavioralists were changing the study of voting and elections. Jack Walker was an early leader in this area at Michigan, both methodologically and conceptually. His early article (1969) on the “Diffusion of Innovations Across the American States” was focused on the concept of policymaking communities. How did bureaucrats in the 50 state capitals understand how best to organize new government programs? By speaking with each other at national conferences. In his 1977 “Setting the Agenda in the US Senate,” Walker also focused on the linkages between Washington policymakers and technically-oriented policy experts far from the halls of government, but knowledgeable about the technical issues of crafting or implementing successful

programs. His 1966 “Critique of the Elitist Theory of Democracy” had already put the concept of the role of elites in the agenda-setting process at center stage.

Joel Aberbach, Robert Putnam, Edie Goldenberg, Pat Crecine, Tom Anton, Samuel Eldersveld, and other Michigan faculty members of the time focused on elite policymaking systems in the US and elsewhere. (Kingdon met regularly for an informal faculty discussion group on agenda-setting with Goldenberg, Aberbach, Putnam, Walker, and occasionally others, for years.) Warren Miller and Donald Stokes felt that their studies of voters could not be complete without parallel surveys of Members of Congress (see Miller and Stokes 1963). Phil Converse and Roy Pierce (1986) replicated the work in France, producing a monumental work equally of interest to those concerned with legislative behavior as to those concerned with voting and elections. So the study of elites were linked with the study of masses, and the department set out deliberately to be strong in both areas, with individual scholars involved in both types of behavioral research (see for example Aberbach and Walker’s study of mass politics in Detroit (1973) or Kingdon’s own study of opinion leaders in the electorate (1970)). With the authors of *The American Voter* studying elites, and the institutionalists also studying masses, the department clearly had more cross-over and cooperation than we often see today between institutionalists and behaviorlists. The reason is that distinction did not really exist; both sets were behavioralist, but some focused on elites and some on mass publics.

Aberbach, Putnam, and Rockman’s (1981) completion of their *Bureaucrats and Politicians in Western Democracies* was perhaps a miracle of perseverance (the project was ready for its *bar mitzvah* by the time of its completion), but it was only one of many books to be written on the basis of formal surveys of elites. Robert Putnam had recently produced *Beliefs of Politicians* (1973a; see also 1971, 1973b) and a textbook entitled *The Comparative Study of*

Elites (1976). Tom Anton (1980) and Samuel Eldersveld with colleagues (1981) published elite-based interview studies of Swedish and Dutch policymakers, respectively, as did John Campbell on Japan (1992). Other important faculty colleagues working on related issues from a variety of perspectives included John Chamberlin and Larry Mohr in political science, Ned Gramlich and Paul Courant in economics, and Mayer Zald and Bill Gamson in sociology. Kingdon was surrounded by colleagues interested in the behavioral study of bureaucrats, interest groups, and elected officials just as much as those concerned with voters.

The strong linkages among comparative and American scholars studying policy communities and the roles of elites stemmed perhaps from their common desire at Michigan to establish the study of elites as equally important as the study of members of the mass public. While Kingdon himself remained firmly an Americanist, he was clearly interested in the generalizability of political science concepts beyond US borders, and in fact, the next book he wrote after *Agendas* was *America the Unusual* (1999). It put US politics in comparative perspective and was designed to be used in the large introductory lecture in American politics. This linkage of American and comparative studies of public policy, policy processes, and elites in general, and the focus on policy communities as an object of study, was one major element of the environment that generated *Agendas and Alternatives*.

Congressional Studies

A second major influence clearly to be seen in the book is Kingdon's own background as a congressional scholar. His first book, based on his dissertation, was a study of election campaigns. In *Candidates for Office* (1966), he used a stratified sampling procedure, with half the sample consisting of a census of all candidates (winners and losers) in Wisconsin for federal or state-wide office (e.g., US Senate, House, Governor and other state-wide posts), and the other

half a random sample of state legislative candidates, stratified by region within the state. His response rate, 62 of 64 candidates selected, was typical of his work, but no longer typical today. The interview was a highly structured survey, quite different from his later work.

His second book, and the one for which he was most widely known for many years, focused on congressional decision-making: *Congressmen's Voting Decisions* (1973) established him as a leader in the field, and remains a prominent contribution to the congressional literature to this day. With Richard Fenno's contributions (1966, 1973) to understanding congressional budgeting and work in committees, Kingdon's highly empirical and rigorous work on how Members of Congress scan the environment and decide how to cast their votes has been a touchstone of congressional research since the time it was published. The two scholars, with Charles O. Jones, perhaps defined the generation of scholars following Donald Matthews with a rich empirical knowledge of Congress and its procedures.

Some of these influences are clear from the acknowledgements in his books. He credits throughout his works the influences of such scholars as his graduate advisor Ralph Huitt, congressional luminaries Richard Fenno and Charles O. Jones, and Lewis A. Dexter. These scholars, perhaps most exemplified by Fenno, have a rich understanding of the details and day-to-day workings of Congress. Fenno's *Homestyle* (1978) achieves a readability that is rare in political science, similar to Kingdon's *Agendas*. Further, Fenno, like Kingdon, is clearly comfortable describing and explaining something that is highly complex. Both have great complexity and richness in their descriptions, and neither wants to impose a structure that might not ring true to an intelligent staffer or long-standing observer of the Washington scene, be they a journalist, a policymaker, or a well informed political scientist. So there is a primacy of observation and a strong desire to write at a level of abstraction that enables the insights the

research brings to bear to be interpretable both within the profession and inside the beltway. As is clear with his acknowledgements to such scholars as Dexter, Fenno, and Jones, Kingdon is a scholar who puts observation first.³ A research project can only be as good as the observations on which it is based, and in Kingdon's case these ideas and hunches were based on years of close interactions with congressional members and staff, other Washington policymakers and journalists as well as the intellectual environment in Ann Arbor. But they did not remain ideas and hunches; interviews were in fact highly focused on a few key ideas, and after the field work was over a long period of analysis led to put these findings into a coherent and powerful theoretical structure combining induction and deduction.

Lewis A. Dexter was a gifted scholar of interest groups, congressional policymaking, and the study of elites, publishing a manual on *Elite and Specialized Interviewing* (1970) as well as classic works on interest groups and congressional influence (Bauer Pool and Dexter 1963; Dexter 1969). Kingdon fit his work clearly within this group of empirically oriented, interview-based, field-work focused individuals who sought to write books of interest to scholars but which made sense to policymakers as well.

How could the ideas of genetics, adaptive behavior, cooperation and norms, and random couplings be applied to the congressional process? Other scholars had typically described it as highly structured. Kingdon's *Voting* was clearly more structured and deductively focused than Fenno's *Homestyle*. Even in *Agendas*, he clearly imposes a deductive structure to the complicated processes he is describing. The book is unusual in that it is both readable and familiar to a Washington insider at the same time as it imposes very powerful theoretical

³ See his revealing epigram to the Appendix on Methods in *Agendas*, by Bertrand Russell: "Aristotle could have avoided the mistake of thinking that women have fewer teeth than men by the simple device of asking Mrs. Aristotle to open her mouth."

structure on the description of an often chaotic-seeming reality. The structure he chose was highly unusual at the time and could not have been possible without a collection of colleagues interested in similar issues.

Natural Selection and the Biological Analogy

Michael Cohen, Robert Axelrod, and William Hamilton merit special mention by Kingdon in his preface to *Agendas*, in particular Cohen (who had the office next door and who was in constant conversation with Kingdon throughout the writing of *Agendas*). Kingdon's theoretical structure through the book is a simplification of Cohen et al.'s (1972) garbage can. Richard Dawkins (1976) *The Selfish Gene* was a popular and highly influential explanation of natural selection and its potential to help understand various elements of human behavior. Robert Axelrod, of course, was completing his *Evolution of Cooperation* at the same time as Kingdon's book (both were published in 1984 and Kingdon cites Axelrod as forthcoming; see also Cohen and Axelrod 1984). The impact of biological thinking and of natural selection on Kingdon's thoughts is clear on almost every page of the book. I recall reading drafts of Axelrod's and Kingdon's books in graduate seminars before they were published; the ideas were ubiquitous.

Whereas many in political science took inspiration from Newtonian physics, classical economics, and linear algebra, one of the most distinguishing characteristics of Kingdon's book is its use of biological metaphors and the concept of natural selection and randomness as essential drivers of the policy process. Some within the profession initially responded negatively to Kingdon's decision, and were disappointed by the clear difference in this book as compared to his previous *Congressmen's Voting Decisions*, which fit in much more easily with a rationalist view. This stemmed fundamentally from a misunderstanding of theory and randomness. To assert that a process includes a random component, as Kingdon, Axelrod, Cohen, and anyone

working from a natural selection perspective does, of course has powerful research design ramifications. Typically in political science we seek to test our theories at the individual level. But if individuals are affected by random effects, how can we predict their behavior and then compare it to what we observe, for a test of the theory? Since I recall discussing this particular problem with Kingdon (and Walker) many times, his attitude was clear: you don't understand. First, if the process truly does include random (or "pseudo-random") elements, there is no value in suggesting it is determinative; that will never work. Second, the inclusion of randomness has not turned biology and genetics into unscientific areas of research; it has merely changed the unit of analysis at which scientists seek to test their theories. It is the difference between genetics and medicine, population ecology and veterinary science, climate science and meteorology. Whereas the doctor cannot predict if a child will be born with a genetic mutation, a geneticist can tell how often various mutations occur across a population. Whereas no weather reporter can tell us when a particular tropical storm will become a hurricane, nor where and when it will make landfall, climate scientists nonetheless understand the process by which these events occur. The Policy Agendas Project, in fact, is driven by a desire to make possible population-level tests of ideas that had been studied only at the individual level in the past. Kingdon pushed the profession very far in a direction that has enormous potential for the future.

Kingdon's theory was definitely a theory, not just a set of metaphors. It lays out a set of factors that explain why agendas change as they do. Kingdon would distinguish between "essential" and "residual" randomness. In his view, important institutional and other factors do indeed structure the agenda-setting process. Elections occur at regular intervals; the public mood changes only slowly; entrepreneurs seek to take advantage of windows of opportunity that are open for only certain periods of time; institutional gate-keepers have privileged positions in the

policy process. Even after these structures have their impact on the process, however, there is a residual randomness: The window of opportunity may close unexpectedly; the election may yield an unexpected winner; a gate-keeper may lose their position. This is why Kingdon talked about probabilities of outcomes rather than point predictions. Similarly to how climate scientists or evolutionary biologists understand structure but discuss probabilities, Kingdon did so, most explicitly in the most recent edition of *Agendas*, where in the epilogue he discusses President Obama's health care initiative (2010).

While there is no question that the fluid metaphors derive more from biology and evolution than from physics or astronomy, Kingdon develops a powerful theoretical model based on a firm understanding of decision-making processes derived from the Carnegie school of organizational theory, centered on the works of such authors as Herbert Simon, Johan Olsen, James March, Richard Cyert, and Michael Cohen. These works take seriously issues of human cognition and psychology, but are amenable to mathematical modelling and computer simulations of complexity (e.g., the works of John Holland, Robert Axelrod, and the garbage can model) rather than the highly deductive mathematical models that are common in other areas of political science or economics. Those approaches simply did not capture the diversity of behaviors that he observed in the process.

Kingdon's *Agendas* took seriously the biological approach to the study of public policy because it jibed with his understanding of how policymakers interact with one another in the complex world of Washington. This would not have happened were it not for the influence of Michael Cohen, Robert Axelrod, William Hamilton, John Chamberlin, and other colleagues who surrounded Kingdon during the time this book was developed.

Constructing the Book

It was no easy task to take a deep knowledge of Washington policy processes and congressional procedures and to put that into a book that would be coherent and understandable with a theoretical focus drawn from evolutionary biology. Certainly, it was not apparent how to do so, and no major political science theories at the time gave a prominent place to randomness, except for Cohen's garbage can model. So how did he construct a book with such staying power?

This book was a long time coming. Kingdon's project was funded by an NSF grant in 1978 which called for four years of field research. Kingdon remarked how surprised he was that NSF would support the work since it proposed a more inductive research approach than is typical. A careful reader of the book will see a bifurcation between the main elements of the book, described in the main chapters, and the careful coding of many individual policy proposals in the appendix. That work clearly informed the writing, but eventually the structure of the book would not be, as in his previous book on congressional voting, the straightforward reporting of tables and figures.

Besides the quality of the writing, a key distinctive feature of the book is that while it is clearly a research report, it is also a teaching tool. The structure of the book allowed its use as supplemental material in a wide range of courses, from introduction to American politics to Congressional procedure, to interest groups and policymaking courses. The chapters parallel the institutions of government, at the same time as they are not traditional chapters on institutions. Rather, the book was striking because it was a behavioral look at who actually plays which role. The section on interest groups does not talk about Olsonian membership dilemmas, but how lobbyists are "outside of government, but not just looking in" (*Agendas*, Chapter 3). While the book reflects important institutional distinctions, it does not fetishize institutional role. Like a true behavioralist, and harkening back to a previous generation of scholars looking at the

interactions of policy communities in Washington, he lets the observations tell the story. If ideas can come from congressional staff, interest groups, academics, or from government officials, then all must be discussed, and not necessarily in order of their institutional position. The book simply implemented the ideas discussed above about the interactions of players within a community of experts—in this case, the health and transportation policy communities in Washington.

Like Richard Fenno's *Homestyle*, Kingdon's *Agendas* is also remarkable for its methodological appendix describing the questionnaire and broader interview procedures. Louis Dexter, in his book on *Elite and Specialized Interviewing*, focuses on how to interview elites, rather than members of the mass public as in a public opinion survey. For Kingdon, like Fenno, the real distinction might be between elite and mass interviewers, however. It is particularly instructive to track the changes in the forms of interviews that he conducted from his earliest work on candidates. The first questionnaire (see Kingdon 1966, Appendix B) was a highly formal survey research document complete with show cards and extensive demographic questions. In the second book, citing Robert Peabody and Ralph Huitt, the questionnaire starts out with a citation to a particular vote chosen through a procedure to identify "big votes" (Kingdon 1973, 279–84) and begins: "How did you go about making up your mind? What steps did you go through?" The questionnaire includes just nine questions (287).

In both his congressional voting book and in his later book on agendas, the interviews were conversations. In this book, he starts out by asking "what major problems are you and others in the [health / transportation] area most occupied with these days?" and probes only as necessary from there on (the questionnaire has just seven questions; see Kingdon 1984, 221). He makes sure he touches all the points on which he will need information, if the official does not

volunteer it and might know it. Then again, if the information is volunteered, or if he has the sense that the respondent has no reason to be aware of the answer, he does not ask a question that need not be answered. Kingdon, like Fenno, was such a skillful interviewer that he was able to extract from his respondents a feel for the process of policymaking in Washington. This could only come if these Washington policymakers understood that were discussing their work with a peer, not a neophyte. It is also clear that the conversational tone of the interviews allowed the respondents to go into greater detail on whatever points they felt needed elaboration. To do this kind of work, the author had to know the substance of what they were discussing, which is why Kingdon limited himself to just two policy domains (it is hard to get up to speed to understand the jargon and details of a new policy domain in order to have an intelligent conversation with an insider). Indeed, while the questionnaire listed in the appendix to the book lists just a few questions, the probes were extensive, requiring considerable research for each interview, as they focused on particular issues if not spontaneously mentioned by the respondent. Soaking and poking allowed insights to come from the respondents and generated a mass of information that then had to be systematically compiled and assessed, and was the right way to do these interviews in areas where the research questions were general probes, not precise measurements of narrowly defined questions deduced from a previously refined theoretical model. As the first to study these topics, he needed a more inductive and open research approach to allow the respondents to convey what they knew, as if to a peer. In the interview-based work that I have done, I have always used Kingdon's short but essential interview schedules as a model. The key difference in interviewing may not be mass and elite survey populations, but mass and elite interviewers. He was in the elite.

Later Work and Influence

Having published a book about campaigning for office, one on congressional decision-making, and one on agenda-setting, Kingdon planned to write one on policy implementation. While he had done the agenda-setting book perhaps out of order, his idea was to write a book about each stage of the policy process. It never happened. Having written three books based on extensive field-work and years of study, he simply tired of the hassles and the rare but occasional uncooperative or unpleasant interview subject. As is clear from the discussion of his interview techniques above, this was intellectually tiring work: learning the substance of a range of policy issues well enough to engage in an intelligent conversation with a policymaker is exhausting, especially when each day brings another interview, to be followed by coding and analyzing for several years to follow. He designed an intensive study, but ironically never implemented his book on implementation. Rather, he continued to explore ideas of complexity (and wrote another book, *America the Unusual*). My own book, with Bryan Jones, benefitted from Kingdon's explorations of complexity and his continued forays into the literature on biology and evolution. He was the reviewer for The University of Chicago Press for our book (initially entitled "The Politics of Disequilibrium") who pushed us strongly to adopt the language of punctuated equilibrium, which was absent from the initial draft. Of course, that language is now fundamental to how our book is understood.

In some ways we sought to respond explicitly to the research design Kingdon had used in *Agendas*. Where he based his research on four years of fieldwork, and covered just two issue-domains, we wanted to cover many decades and many policy domains. This of course required that we could not base our work on interviews and perhaps also explains one of the only ways in which our works do not square: the role of the media. Kingdon's respondents simply did not suggest to him that the media did more than convey ideas they already knew; Bryan Jones and I

found a more important role for the media. This finding has led to many commentaries and seminar discussions (can it really be true?), but John's response was to stay true to the data: that's what his respondents said.

There is no doubt about the impact of Kingdon's work on the field, beyond only the thousands of cites and the many scholars who have adopted a multiple streams approach; it is also a driving force of the punctuated equilibrium approach—in fact, he was present at the creation. The Policy Agendas Project, in large part, is driven by our desire to construct the next logical step in the study of agenda-setting, following his contribution. Of course, it succeeds in some areas better than others, and one way in which it has not improved on his work at all is in the area of studying the power of ideas, still a major contribution of Kingdon's approach, and one that many of us have followed up on in various works.

Benefitting from the brain-power of John Kingdon was no monopoly for any of us who worked with him. He gave to many students over the years but, remarkably and consistently, never attempted to push or pull us in to his orbit, to adopt any of his particular ideas. Kingdon's *Agendas* was a product of many forces, but most prominently the genius, creativity, and unending curiosity of its author.

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