Chapter 3

William H. Riker and the postwar Political Science

This chapter will explore William H. Riker's life and early works, from the late Forties until 1962. In that year, Riker published his most ambitious theoretical work, *The Theory of Political Coalitions*, which represented the first full-breadth attempt to employ Game Theory to provide a model of political behavior. In the same year, the administrators of the University of Rochester appointed him Professor of Political Science and Chairman of the Department. At Rochester, Riker established the first Ph.D. program in political science in which formal analysis, decision and Game Theory, and mathematical modeling were central.

Riker, as seen, was not the first to employ game theory in politics. For example, the economist Martin Shubik edited a brief volume which collected essays and excerpts illustrating the potential fertility of a game-theoretic approach in political science (Shubik 1954). Shubik also authored, with Lloyd Shapley, a concise theoretical paper applying a cooperative Game Theory solution to political issues (Shapley and Shubik 1954).

Probably the most famous work employing Game Theory to address political issues was Thomas Schelling's *The Strategy of Conflict* (Schelling 1980). Schelling was an economist and eventually won the Nobel Prize in Economics in 2005, together with Robert Aumann. However, whereas the latter was a mathematician and his contributions were extremely formal, Schelling did not develop new solution techniques; instead, he provided innovative and valuable insights, especially in international politics. He employed Game Theory to address coordination problems among players who do not know what the other is doing. In other words, he explored the fact that some Nash Equilibria are better than others, and even that some Nash Equilibria are not Pareto-optimal, as well as the problem of multiple Nash Equilibria and how to select among them. However, Schelling remained an economist rather than a political scientist and accordingly, he neither participated in the methodological debates of the Fifties nor advanced a reformist agenda within the discipline.[1]

Anatol Rapoport also offered important contributions to the analysis of international conflict and its resolution in a similar vein. However, despite the bold interdisciplinarity of his work, Rapoport did not engage with the

1 Although, in the first chapter of his classic 1960 work, he discussed how his idea of the "strategy of conflict," applied to international politics, differed from more traditional approaches.

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methodological and disciplinary issues that concerned political scientists in the Fifties, and his contributions do not fit the narrative developed here. His work did not decisively and durably affect political science, nor did it contribute to the creation of a methodology-driven and theory-driven subfield of the discipline. A similar assessment applies to other scholars who offered formal contributions or discussed game-theoretic techniques in their early works, most notably Herbert Simon, Morton Kaplan, and Karl Deutsch.

Simon, perhaps the most famous among them, studied in Chicago in the late Thirties and early Forties, as seen, and was socialized as a professional political scientist (Simon 1996). He was also among the first reviewers of *Theory of Games* (Simon 1945), where advanced several suggestions concerning how scholars of politics might extend the analysis developed by Neumann and Morgenstern to political science. Furthermore, Simon presented his most influential contribution to the study of rational decision-making, namely, the notion of "bounded rationality", in a work that examined organizational behavior from the perspective of organization and policy science rather than economics (Simon 1947). However, his intellectual trajectory, spanning econometrics, systems theory, and computer science, was arguably too broad to translate into a focused and direct influence on political science (on his intellectual career, Simon 1996).

Deutsch was another political scientist who anticipated the possible use of game theory in international relations (Deutsch 1954). However, also he did not pursue the formal and technical development of the theory, and therefore his analysis cannot be regarded as game-theoretical in any strict sense.

A similar case is that of Kaplan. In one of his early works on international politics, *System and Process in International Politics* (Kaplan 1957), Kaplan devoted an entire chapter to the discussion of Game Theory. In his view, this had relatively little to say about systematic choice patterns in international politics, but it was useful for understanding strategic choice. Accordingly, Kaplan, much like Schelling, separated the strategic analysis of conflict from the broader study of international politics. He framed this discussion within a more general systems-theory approach, arguing that Game Theory is not a substitute for other social and political theories but a tool for understanding the functioning of a system (Kaplan 1957, p. 220).[2] Kaplan classified international systems in a way that, as noted by the economist Kenneth Boulding, resembled market structures, ranging from perfect competition (a balance-of-power system with many actors) to monopoly (international hierarchy), passing through monopolistic competition and oligopoly (Boulding 1958).[3] His taxonomy of actors was more complex and

[2] In brief, systems analysis treats politics as systems of action, defined by stable relationships among variables and between these variables and their environment.

[3] Kenneth Boulding was also deeply interested in conflict resolution and a committed pacifist. He contributed to the establishment of the *Journal of Conflict Resolution* and the Center for Research on Conflict Resolution at the University of Michigan, an interdisciplinary research center active between 1958 and 1971, to which Schelling and Rapoport also contributed (Erickson 2015).

involved different patterns of choice. Nonetheless, he focused primarily on 2PZSG and minimax solutions, offering only a brief presentation of von Neumann and Morgenstern's cooperative solution.

These scattered references indicate that, during the Fifties, some political scientists tentatively employed Game Theory in various ways. However, these analyses and their proponents did not use it to promote the construction of economics-style formal models in political science. Nor did their investigations (apart from some of Schelling's intuitions) significantly influence the development of game theory itself. The case of Riker and of "Positive Political Theory" was fundamentally different.

The following pages reconstruct Riker's early life and academic career up to his appointment at Rochester. They also examine the methodological and philosophical papers he authored, as well as his first contribution to game theory, a partially empirical test of the power index developed by Shapley and Shubik.

3.1 Riker in the Fifties: from Harvard to Rochester

A historian can derive some information on Riker's life from the brief biographical memoir written by Kenneth Shepsle and Bruce Bueno de Mesquita for the biographical series of the National Academy of Sciences, which admitted Riker in 1974, the first political scientist ever.[4] This memoir provides revealing accounts of Riker's personality and family life, as well as of his role as teacher and mentor. However, given the nature of the series, its general tone is often acquiescent and celebratory. For this reason, the most important source for reconstructing Riker's early life is the long and detailed interview he gave to Shepsle in 1979, as part of the "Political Science Oral History Program" (Riker and Shepsle 1979).[5] This 150-page typed interview ranges from recollections about undergraduate and graduate education to theoretical and methodological issues.

Riker also offered other retrospective accounts of his intellectual trajectory (for instance, in the paper presented at the first academic conference on the history of game theory in 1992 (Weintraub 1992. See also Riker 1992; Riker 1997). These texts reprise several themes but generally do not add substantial new information. Moreover, the narrative is often generic and lacks historiographical precision. Accordingly, the primary source remains his interview with Shepsle.

Furthermore, by discussing his life, his training as a political scientist and his early scholarly initiatives, the following pages also provide insights

4 Shepsle was a graduate student at Rochester, in the Ph.D. program focused on Rational Choice and Game Theory that Riker established there starting in 1964. Bueno de Mesquita arrived at Rochester in 1972 and remained there until 1986, becoming close to Riker.

5 There are inherent risks in relying too heavily on oral history and personal reminiscence. Still, given the nature of the topic, namely contemporary intellectual history, these materials provide an essential source. For an interesting methodological discussion of oral history in the history of contemporary economics, see Düppe and Weintraub 2019.

into the broader condition of political science in the Forties and Fifties, thereby complementing the preceding historical reconstruction.

3.1.1 Harvard and Lawrence College: graduate years and early works

William Harrison Riker was born in Iowa in 1920 and grew up in Michigan and later Indiana, where his father, during the Great Depression, established a bookstore (Bueno de Mesquita and Shepsle 2001). In his recollections, the most important influences on his interest in political science were the family atmosphere, due to his father's involvement in local politics and the broader climate of the New Deal (Riker and Shepsle 1979, 32 et ss.). He enrolled at DePauw University (IN), where he obtained a B.A. in economics in 1942, and later spent some time during the war working for the RCA (Radio Corporation of America).

In his interview, he reflected on the state of political studies at the time. In the late Thirties and early Forties, according to him:

> "[. . .] There was not clear conception of what the field was, in my impression. It was hard to tell the people who studied political parties and American politics from historians, and indeed they were often the same people. And it was hard to tell the people who studied constitutional law and things like that from lawyers and indeed they were often the same people. And it was very hard to tell political philosophers from historian of ideas or from people in philosophy departments, and indeed they were often the same people. So that the main activities that one associated with departments of political science [were] just very difficult to distinguish them from other fields, though that is equally true of the people who taught about public affairs." (Riker and Shepsle 1979, pp. 32–3) [6]

In his view, this situation was common both in undergraduate training and in graduate education. His bachelor's degree in economics might advance some speculation toward his future interest in formal methods. In reality, undergraduate economics at the time was likely of limited interest and largely devoid of theoretical ambition, especially with respect to mathematical analysis. In his remarks, Riker attributed influence to these studies mainly in terms of an intellectual "mindset," rather than specific technical training: "I [...] believed that the traditional study of constitutions which political scientists have engaged in, was a kind of study of purpose in behavior [...]." (Riker and Shepsle 1979, p. 21)

Despite this perception of the discipline, he decided to apply to graduate school in political science. Riker's set of choices included Harvard, Columbia,

[6] Another similar account is that provided by Charles Lindblom, who, following Daniel Bell's analysis of the second postwar American social sciences, defined political science in the Forties and Fifties as "a weak discipline, hardly worth explicit comment in an account of the great and exciting issues in social sciences of that period." (Lindblom 1997, p. 229)

and Chicago, which were "[t]he three schools that were producing substantial numbers of political scientists at the time." (Riker and Shepsle 1979, p. 36) Chicago was associated, as seen, with Charles Merriam and the "Chicago School of Political Science," whose members emphasized empirical methods and quantitative analysis. Indeed, his political science professor at DePauw, Harold Zink, advised him to apply to Chicago.[7] However, at the time Riker was influenced by the work of Pendleton Herring, a professor at Harvard, and therefore enrolled there in 1945. Herring, a generation younger than Merriam, was close to the latter in advocating scientific methods in the social sciences. He also played, as seen before, a pivotal role in the Social Science Research Council in the late Forties and Fifties and, more broadly, in the Committee on Political Behavior.

At Harvard, Riker studied under Herring for two years before the latter's appointment at the Social Science Research Council. He recalled him as "an excellent person to work with, although ultimately I found what he was teaching was not terribly interesting." (Riker and Shepsle 1979, p. 38) Herring's approach relied on case studies, focused primarily on public administration. Almost thirty-five years later, Riker was notably dismissive of this method, describing it as "simply artistic investigations of events," devoid of generalization and therefore incapable of grounding a political theory (ibidem). This judgment was already formed during his graduate years:

> "I was aware of the limitations of case methods, indeed. I remember writing a case in which I abandoned all pretence of objectivity or anything like that. After all, these case study things were supposed to be objective records of events and I quickly realized that they were not, that they were simply rambling memoirs of individual participants. [...] I was clearly aware of the inadequacies of the case method, and indeed of the dissertations that Herring had sponsored." (Riker and Shepsle 1979, pp. 39–40)

Despite these criticisms, Herring was the teacher to whom Riker felt closest. Still, the person "who was the dominant figure in that department at that time and who certainly influenced all the graduate students" (Riker and Shepsle 1979, p. 41) was the German scholar Carl J. Friedrich. Friedrich came to the United States in the Twenties and remained there after Hitler's rise to power. His main research areas were constitutional theory, especially federalism and comparative analysis, and the history of political ideas. Moreover, Friedrich was among the relatively few scholars in American political science during the Thirties who sought to situate empirical research within an original theoretical framework by developing a theory of power (Easton 1951).

Riker's relationship with Friedrich was turbulent, due both to the professor's personality ("he was an extremely opinionated man") and to

7 Of Zink, Riker remarked that "he did have some sense of the discipline, although he never quite conveyed it to me" (Riker and Shepsle 1979, p. 35).

the nature of his scholarly activity. Riker later recalled: "He may have had, [at] an earlier period of his life, an interest in political science as such; but, by the time that I was around there, his sole interest was in teaching about the history of political ideas." (Riker and Shepsle 1979, p. 42) This assessment is arguably too severe, since Friedrich continued to produce political science research and published several important and well-received works in the Fifites, Sixties, and Seventies.[8] Yet, in Riker's recollections, Friedrich came to symbolize the general orientation of Harvard's political science department and, by extension, of the American discipline as a whole. Thus, although Friedrich was hostile, as Riker, to primitive empirical approaches such as case studies, this hostility, according to the latter, reflected opposition to the description of political events as such and, consequently, did not contribute to establishing a science of politics (Riker and Shepsle 1979, p. 43).

These remarks are valuable insofar as they capture how a young practitioner perceived the discipline in the Forties. On the one hand, there were scholars such as Herring who aimed at scientific rigor but relied, in Riker's view, on inadequate methods. On the other hand, Friedrich defended the need for theory but, again, according to Riker, at the expense of practical and cumulative inquiry. One consequence was that Riker found graduate education intellectually unsatisfying. Graduate training at Harvard, he argued, was deeply compartmentalized, and the faculty emphasized historical studies. This compartmentalization allowed the coexistence of divergent approaches, such as those associated with Herring and Friedrich, but did not favor intellectual exchange even within the discipline. As a result, Riker observed that "nothing that anybody studied in my group, at least nothing anybody studied in graduate school, had any significance for their subsequent intellectual development, which is probably a pretty good picture of the state of Harvard at that time." (Riker and Shepsle 1979, p. 44)

The "Behavioral Revolution," which reshaped American political science from the late Forties onward, emerged from concerns similar to those expressed by Riker and other young scholars. Consider, for instance, David Easton, who also passed through Harvard's graduate program. Easton's early work advanced a critique of the historicist orientation of modern political theory (Easton 1953). In an interview for the "Political Science Oral History Program," Easton remarked that "by the time I left Harvard, I just didn't know what political science was all about." (Baer, Jewell, and Sigelman 1991, p. 199) This statement closely parallels Riker's view that "people go out of Harvard without having any sense of doing anything in political science" (Riker and Shepsle 1979, p. 48), and that he "had no sense of what one did as a scholar in political science when I got through

8 Friedrich was especially known for his studies of totalitarianism, federalism, and political theory. Among his notable works: *Constitutional Government and Democracy* (1950); *Totalitarian Dictatorship and Autocracy* (1956), coauthored with Zbigniew Brzezinski, who became National Security Advisor under President Jimmy Carter; *Man and His Government: An Empirical Theory of Politics* (1963).

and finally [got a] Ph.D. [at] Harvard." (Riker and Shepsle 1979, p. 44)

Finally, in Riker's account, even dissertations were not "a real investigation to discover truth or anything of that sort," but rather "simply an exercise without any real expectation of scholarly achievement" (Riker and Shepsle 1979, p. 40). He chose Pendleton Herring as supervisor and worked on the relationship between the Congress of Industrial Organizations and political organizations in the late Thirties and early Forties, adopting a case-study approach. The dissertation was submitted and successfully defended in 1948, under the title *'The CIO in Politics. 1936-1946'*, and the supervision of Merle Fainsod (a change prompted by Herring's departure from the department).

The years immediately after the Second World War witnessed a massive expansion in undergraduate and graduate education, thanks in part to federal programs that provided de facto free education to veterans (the Servicemen's Readjustment Act of 1944, commonly known as the G.I. Bill). This surge in demand led universities to expand supply by hiring new faculty and increasing graduate cohorts. By the late Forties, however, demand for teaching began to decline, and a difficult hiring climate persisted until the mid-Fifties. Riker, who had married in 1943 after an unsuccessful job application at Swarthmore College (PA), was hired by Lawrence College in Appleton (WI), where he remained for almost fourteen years before moving to Rochester.

He recalled that the intellectual atmosphere at Lawrence was stimulating, due in part to lighter teaching requirements that allowed more time for scholarship. During this period, his main scholarly activity was writing a textbook on the American political system (Riker 1953), largely based on his teaching in American politics (Riker and Shepsle 1979, 50 et ss.). He attributed particular importance to these years, especially insofar as they pushed him to reflect on the foundations of political science. Indeed, he later recalled that soon after publication, he realized that "it would be hard to say that any sentence in it was true" (Riker and Shepsle 1979, p. 60). The central issue thus became what political science was, and whether it could utter true sentences.

Riker later referred to the development of political science in that period as "the ferment of the 1950s" (Riker 1997). Reformist ambitions and practical interest in public affairs were part of that ferment. In his case, however, the decisive incentive was the perceived need for a rigorous foundation of the discipline's methodological premises. He began reading philosophy of science, especially logic, but soon realized that logic bore more on the validity of arguments than on their truth. He therefore complemented these readings with more applied mathematics (linear algebra and calculus), before discovering von Neumann and Morgenstern around the mid-Fifties.

In a paper written to reconstruct the history of Game Theory's entry into political science, Riker provided a somewhat different account of how he became acquainted with Game Theory (Riker 1992). He traced this acquaintance to his reading of Shapley and Shubik's paper in the *American Political Science Review* on power distribution in committee systems (Shapley and Shubik 1954; Shubik 1954). This paper, though not excessively technical, was nonetheless strictly theoretical and rested on cooperative n-person solution concepts developed by Shapley. Alongside it, Riker also read Kenneth Arrow's work on Social Choice, and these readings led him back to von Neumann and Morgenstern's foundational text. As he recalled: "There I discovered what I thought that political science needed for constructing theory" (Riker 1992, pp. 207–9).

Riker devoted much of the second half of the Fifies to expanding his knowledge of Game Theory, especially cooperative Game Theory, in order to test Shapley and Shubik's conclusions and to explore Social Choice Theory further (Riker 1959a; Riker 1961). He also published two philosophical papers addressing how to circumscribe events so as to formulate descriptive generalizations about politics (Riker 1957; Riker 1958a).

Riker quickly became "something of a publicist" for Game Theory in political science. For example, he pushed for the political theory panel at the Midwest Conference of Political Scientists at the University of Michigan, Ann Arbor (April 1958) to include a session devoted to Game Theory alongside another on more traditional themes (Harry Davis to Riker, January 10, 1958, Riker n.d., Box 18, Folder 2). On that occasion, Riker presented a brief working paper introducing game theory to political scientists ("Contributions of Game Theory to Political Theory" (mimeo), Riker 1958b).[9]

Riker opened his presentation by arguing that the main difficulty in explaining game theory to political scientists lay in their limited mathematical training: "[...] [P]olitical scientists are not usually trained in mathematics and are somewhat afraid of or at least diffident about pursuing it. Hence they have been diffident about pursuing the relationship of game theory to politics." (Riker 1958b, p. 1) Game theory, he suggested, concerned "a series of theorems about how to play particular categories of games most profitably." (Riker 1958b, p. 2) A game, in turn, is a set of rules comprising the possible moves available to the actors involved.

Riker's conviction that game theory was highly relevant to politics is evident in the following passage:

> "The category of zero-sum, two-person games is clearly a model for those political situation in which two persons are each trying to do the other in. The cooperative two-person games, in which the players can, by cooperating, obtain a greater payoff than by opposing each other, bear an obvious resemblance to, e.g., oligopolistic situations so often found in economics. The n-person game is clearly a model of the contemporary nation-state system or of the free market of classical economy or of

9 The chair of the session was the political scientist Ralph M. Goldman (Michigan State), and the discussants were Stanley Gabis, Theodore Mitau, James M. Roherty, and Glendon Schubert (Schubert would be a fellow at the Center for Advanced Studies in the Behavioral Sciences with Riker) None of them was a Game Theory expert or shared Riker's commitment. Goldman and Roherty were specialists in war and international politics. Gabis studied under Leo Strauss in Chicago and was a political theorist. Mitau worked on public affairs and educational policy.

> legislatures with undisciplined parties. etc.." (Riker 1958b, pp. 2–3)

In his view, Game Theory was primarily normative and therefore distinct from the descriptive models traditionally employed by political scientists. However, it "differs notably from the kind of normative theory heretofore found in political science. Most normative political theory is concerned with distinguishing between the just and the unjust. [...] Not so game theory. It is concerned rather with distinguishing between the smart and the stupid. It establishes and justifies standards of rationality and then uses the standards to separate wise from foolish behavior." (Riker 1958b, p. 4) Moreover, he suggested that "a verified normative theory might conceivably lead to a political engineering" (Riker 1958b, p. 5).

To support this claim, he offered an example drawn from international relations and the "balance of power system." In such a system, each member must oppose any actor that becomes predominant, otherwise the balance collapses. Riker argued that game theory could advance this analysis. In a simple model with two opposing coalitions and a neutral one, he used an elementary version of the Shapley–Shubik power index to identify conditions under which the neutral actor would prefer to join the stronger coalition, thus breaking the balance, and conditions under which it would instead support the weaker one.[10]

> "Using a simple bargaining model we have discovered circumstances in which the neutral might have a substantial motive for joining the weaker side. Incidentally, we have also uncovered circumstances in which this motive disappears, which explains why balance of power systems breaks down sometimes. Summarising, it may be said that the neutral can be expected to join the weaker coalition (a) when the stronger coalition is such that the neutral cannot dominate it for he can usually expect to dominate the weaker coalitions and (b) when the comparative disutility of annihilation to the weaker side is greater than the disutility of the restoration of the balance to the stronger side." (Riker 1958b, p. 12)

Riker acknowledged several shortcomings of this model, notably its lack of precision and generality. He also recognized a technical issue: it relied on the contested notion of interpersonal utility comparisons, which economists and formal theorists were increasingly trying to eliminate. He further conceded that the discipline was still far from delivering the robust normative analysis he envisioned. Accordingly, the contributions he described were

10 Riker's simplified model assumes two opposing coalitions and a neutral one, implying two outcomes: the neutral joins the weaker coalition and the balance is maintained, or it joins the stronger coalition and the system breaks down. Using a power index, he computed coalition power and inferred what each side would have to offer the neutral actor. He also suggested an optimal strategy for the neutral coalition. However, the model is not developed in a general way, and even the application of the power index relies on arbitrary values.

"better thought of as potential contributions rather than actual ones until bot game theory is improved and the models are more carefully fitted to political applications." (Riker 1958b, p. 5)

Despite these caveats, the general tone of the presentation was optimistic and anticipated later, more ambitious work. Riker nevertheless recalled that, although he received praise, he did not receive substantial feedback at the conference (Riker and Shepsle 1979, p. 8). Seeking a more supportive environment for his research agenda, he joined the Center for Advanced Study in the Behavioral Sciences at Stanford (1960–1).

3.1.2 1960-1: Research Fellow at Stanford

In 1954, thanks to the funding of the Ford Foundation, the Center for Advanced Study in the Behavioral Sciences was established at Stanford University. It evolved from an earlier program, the "Behavioral Sciences Program" in Pasadena, California, and quickly became part of the constellation of non-academic institutions that shaped the so-called "Cold War rationality" (Amadae 2003, pp. 78–9; Erickson et al. 2015). Unlike RAND and Cowles, however, the Center was more explicitly devoted to interdisciplinarity across the social sciences, psychology, and the behavioral sciences. It attracted "many psychologists of a less hawkish persuasion than your typical RAND fellow," yet "the same tools (optimization, Bayesian statistics, game theory) we[re] to be found in its offices as well." (Erickson et al. 2015, p. 14) The lists of fellows included political scientists, sociologists, economists, psychologists, historians, jurists, philosophers, and others.[11]

The Center benefited from considerable funding from the Ford Foundation. Its purpose was twofold: to contribute to the development of the behavioral sciences and to support the development of individual behavioral scientists. It also sought, indirectly, to improve the quality of faculties in these fields (CASBS 1959, Riker n.d., Box 10). The staff, largely connected to Stanford, assisted fellows in their work and organized seminars and conferences to review debates across the social sciences.[12]

By the late Fifties, Riker was looking precisely for these opportunities. He contacted Ralph W. Tyler in early 1958, but the cohort for 1958–9 was already complete. He therefore joined the following year (Tyler to Riker, October 15, 1958,Riker n.d., Box 10, Folder 1).

In his exchange with Tyler, Riker summarized his work as follows:

> "[...] My published work (aside from a textbook on American government) is largely concerned with federalism. [...] While, I am sure, I will continue to have interest in federalism - I have a small research project in process on this subject now - I have for

11 The Center for Advanced Study in the Behavioral Sciences at Stanford is still an active research center. Its website provides a comprehensive list of fellows. Among the first cohorts are eight future Nobel Prize winners in economics.

12 A detailed account of the Center's goals, activities, and selection process appears in an article by Ralph W. Tyler, the first director, published in *Science* in spring 1956 (Tyler 1956).

> the past several years been developing an interest in a formal sort of political theory. The essay on the paradox of voting is directly the product of this interest and the essay on disharmony in federal government combines my two interests. If I were to be granted a fellowship by the Center, I would like to devote it to the latter interest, especially the analysis of coalitions, partly, at least, from a formal point of view. As indicated by the two essays in the Journal of Philosophy I also have a continuing interest in methodology which I might possibly pursue in part of my time at the Center."(Riker to Tyler, March 21, 1958, WHRP, Box 10, Folder 1, underlined in the text)[13]

In a subsequent letter, he added that he wanted to work on the "new formal or mathematical political science" and aimed "to attempt to formulate some mathematical statements about coalitions and to devise tests of the adequacy of these statements" (Riker to Tyler, June 22, 1959,Riker n.d., Box 10, Folder 1).

Riker joined the Center for one year in 1960. He recalled that the political scientists in his cohort were highly heterogeneous. Still, that year proved important for his training in quantitative methods and formal analysis:

> "It was a very strange bunch the year I was there. On the one hand there was Schubert and me, and on the other hand, there was Marty Diamond and his teacher, Leo Strauss, and a student of Strauss, the then-current student of Strauss. It was a very strange group of political scientists. We had nothing to say to each other [...] [T]he thing that was very nice for that year was that I got to know some people at the Center itself who read a good portion of what I wrote and it was their criticism and help that encouraged me to go on with the Coalitions book, especially a man named David Wallace who is a statistician at Chicago and he was quite encouraging. An anthropologist named Nur Yalman, who is now at Harvard, was extremely encouraging also. So that I got some real help from people who knew more about formal matters than I did, especially David Wallace. David Wallace was a collaborator and probably a student of Mosteller's [sic] [...] And Glen Schubert was helpful also, although in his case it was the blind leading the blind." (Riker and Shepsle 1979, pp. 12–3, underlined in the text)[14]

13 I will examine these two works below.

14 For the people mentioned above: David Wallace is known for coauthoring, with Frederick Moesteller, a statistical analysis identifying the authors of 12 out of 85 of the *Federalist Papers*, and for his early role in computational statistics. Nur Yalman was a social anthropologist whose research focused on Middle Eastern politics and culture. Glendon Schubert was among the founders of the study of judicial behavior, with a strong interest in judicial decision-making from both cultural and quantitative perspectives.

These remarks illustrate the Center's interdisciplinary environment. Among the fellows in Riker's cohort were the mathematical economists Debreu and Robert Dorfman, as well as the (less formal) economic theorist Abba P. Lerner.[15]

At Stanford, Riker drafted a significant portion of his book on political coalitions and wrote a detailed review article on social choice and voting paradoxes, later published in the *American Political Science Review*.

This essay represented one of the first systematic presentations of social choice and formal voting theory to political scientists. The discipline remained largely indifferent to social choice analysis, both to Arrow's original contribution and to the extensive literature it generated.[16] The publication of Black's *Theory of Committees and Elections* (1958) prompted Riker to write this review (Black, 1958). In particular, he was troubled by the lack of attention to the scottish author's book, which received some favorable reviews (for example, in the *Journal of Politics* and the *Midwest Journal of Political Science*), but only a brief note in the leading journal, the *American Political Science Review*. Riker then wrote to Avery Leiserson of The Vanderbilt University, Nashville (TN), who edited the journal's book review section:

> "I think this book is one of the half-dozen most important books on political theory to be published in this century (H. Eulau agrees); yet the review relegated it to a footnote in a manual of parliamentary law. If Black were American, I wouldn't be so upset, for I'd expect his work to get known by friendship; but since he is English,[17] I'm afraid his work may simply be ignored and this I would regard as a great loss to the discipline." (Riker to Leiserson, January 31, 1961, Riker n.d., Box 5, Folder 1)

He proposed to write "a bibliographical article on developments in the theory of voting and summation of preferences from 1950 to 1960" (ibidem). This, in his opinion, would complement Black's own contribution, whose second part reconstructed the history of mathematical voting theory with unusual care.[18]

15 https://casbs.stanford.edu/people/past-fellows-research-affiliates-and-visiting-scholars

16 For example, consider a well-known work by Dahl and Lindblom, a political scientist and an economist, which explicitly compared its approach to classical welfare economics. There, the voting paradox was dismissed as "a minor difficulty in voting that people with a mathematical turn in mind enjoy toying with" (Dahl and Lindblom 1953, p. 422). As Riker wrote: "on the whole political scientists have tended to ignore this literature. [...] There are at least two exceptions: In Robert A. Dahl, *A Preface to Democratic Theory* [. . .], the problem of the paradox is elucidated in several footnotes, pp. 42- 44; and in Anthony J. Downs, *An Economic Theory of Democracy* [. . .] the problem is dealt with fairly extensively. pp. 60-68." (Riker 1961, p. 911)

17 Actually Black was Scottish.

18 Black presented a detailed history of voting theory from Jean-Charles de Borda to Charles Dodgson (better known by his literary name, Lewis Carroll). In this section,

Riker also stressed that his being currently at the Center put him in a uniquely favorable position:

> "I'm in an ideal position this year to do this. Ken Arrow is at Stanford, and I gather from conversation that he has kept up with the rain of articles occasioned by his theorem (which was published in 1951). Clyde Coombs, who has pondered the problem of summing preferences as much as any psychologist is at the Center this year. And there is a statistician here who can guide me through the literature on inconsistent triads. With the help of these people, I think I could bring together the work in economics, psychology and statistics and focus it on political theory. I don't pretend to be an authority on this subject, of course, although I probably know more about it than any other political scientist except Black. But I'm pretty certain I can interpret for political theorists the significance of the paradox of voting (and of attempts to bypass it).[...] My concern is not so much to praise Black, although he does deserve more notice than he got, as it is to render political theorists aware of the importance and significance of work in this area. Since Black's first articles were published in 1948, there have been, I suspect, about one hundred more on the problem of adding votes or preferences or utiles. Yet only <u>one</u> of these has appeared in a political science journal, and it is my impression that hardly any political theorists are even aware that the problem exists or has relevance for them."(Riker to Leiserson, January 31, 1961, WHRP, Box 5, Folder 1)

The new editor of the *American Political Science Review*, Thomas Eliot of Washington University in St. Louis (MO), accepted Riker's proposal promptly. Riker then sent a copy of the project to Duncan Black, asking for bibliographical advice. Black responded supportively, even though he could not offer extensive additional references (Black to Riker, March 13,Riker n.d., Box 5, Folder 1). In the same exchange, Black also noted that he had "began looking into the Theory of Games to try to get a link-up with Voting and made a collection of some of the literature." (ibidem) Consequently, in spring 1961, Riker wrote an "interpretive bibliography" intended to present the post-Arrovian developments in social choice and voting theory, although without heavy mathematical detail.

The paper is divided into five parts. The first three address, respectively: voting paradoxes, recently rediscovered by Black; Arrow's theorem; and a verbal review of the debate it provoked among economists, mathematicians, and social scientists. The last two parts examine the relevance of Arrow's theorem and social choice for political theory. Riker also used the essay to articulate what he thought political science should become. Thus, even if it

the Scottish economist displayed notable sensitivity for the historical reconstruction of ideas and contributed to Carroll scholarship through attention to fragmentary materials.

lacks theoretical novelty, it represents a significant step in his intellectual development.

As outlined previously, Arrow generalized the social choice problem, namely, the relation between individual and collective choice, by restating it in the language of Tarski's relational logic. Arrow began from two axioms defining preference as an ordering relation (P, strict preference, and R, a combination of indifference and strict preference). An ordering has two intuitively acceptable properties: it is connected (for any pair of alternatives a and b, it is possible to compare them by R, so that either aRb or bRa, without specifying $a \neq b$), and it is transitive (so that $a > b$ and $b > c$ implies $a > c$). The social problem, then, is to aggregate individual orderings into a unique social ordering (a Social Welfare Function).

Riker described a Social Welfare Function as a "[...] set of instructions fed into a vote-counting machine to inform the machine how to select a victor from the set of ballots. The goal of the theory of summation of preferences is to discover a social welfare function which, from a set of weakly ordered preferences of individuals, produces a unique weakly ordered preference for society." (Riker 1961, p. 902) Arrow showed that no such function exists if a set of apparently reasonable conditions is imposed.

A standard response to Arrow's theorem was to relax at least one condition, or to reintroduce some form of interpersonal comparison of utility.[19] Riker briefly discussed an argument by Leo Goodman and Harry Markowitz, which combined interpersonal comparisons with a critique of Arrow's Independence of Irrelevant Alternatives (Goodman and Markowitz 1952). In their view, if one person has a strong preference for a over b while another has a very weak preference for b over a, it can be reasonable for society to select a. "Irrelevant" alternatives might reveal such intensity differences (for instance, when one ranking is a, c, d, e, b while the other is b, a, c, d, e).

Riker criticized this position, defending Arrow's independence condition as a heuristic device that discourages strategic misrepresentation.[20] He also reviewed James Buchanan's critique of Arrow's analogy between social and individual choice, especially the claim that markets should yield transitive social orderings (Buchanan 1954b; Buchanan 1954a). In Riker's view, whatever one might say about markets, "[...] [w]ith respect to voting systems, as distinct from other methods of summation, it seems that transitivity of outcome is an essential requirement." (Riker 1961, p. 905)

The relevance of these issues for political theory seemed evident to him: indeed, even if political philosophers invoked concepts such as Rousseau's "General Will," the aggregation problem resurfaces whenever citizens vote.

Riker drew here on a typology proposed earlier by one of his Center's colleagues, Schubert. Schubert distinguished three groups of political theorists:

19 It should be noted, however, that Arrow explicitly rejected this approach at the start of his book, in contrast to traditional welfare theorists.

20 Riker acknowledged that this argument resembled that of Duncan Luce, as presented in his work titled *Individual Choice Behavior* (1959). Luce argued that if decisions are made across varying partitions of a choice set, outcomes may depend on the partition structure itself.

rationalists (who assume public interest can be found through preference aggregation); idealists (who deny that public interest is reducible to individual preferences); and realists (who deny that a substantive public interest can be adequately defined). For idealists and realists, the aggregation problem is not central; it is fundamental for rationalists. “Rationalists [...] who play the same role in political science as welfare economists in economics should be intensely concerned with the work of Black and Arrow (Riker 1961, p. 906). Political scientists, then, face the same problem as economists: they must either accept impossibility or restrict Arrow’s conditions.

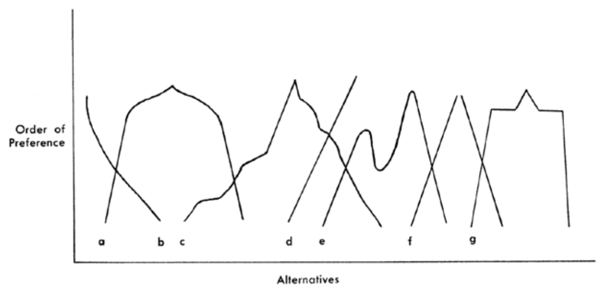

Fig. 1. Examples of preference Curves

Note: All curves but e and g are single-peaked. Curve g is allowed in Dummett and Farquharson's condition for decision, but is not single peaked within Black's definition. Curves a and f are symmetric and strongly monotonic. Curves d and f are, in addition, linearly descending.

Figure 1. Preferences curves.
Source: Riker 1961, p. 905

Riker firmly rejected the idealist position, but his location between realism and rationalism is less straightforward. He began as a methodological rationalist and later moved toward a realist conclusion. In the late Seventies and early Eighties, he famously advanced a radical “realist” position as a dead-end implication of Arrow’s theorem and the subsequent impossibility literature, even while retaining the necessity of rational-choice premises for understanding political action (Riker 1982). Yet, in 1961, he maintained that "until political theorists avail themselves of the appropriate tools for discussion [...], it is inappropriate to conclude that the discussion can have no worthwhile outcome." (Riker 1961, p. 906) At that time, several powerful impossibility results were still undiscovered in formal political theory, and it was still plausible that appropriate restrictions on Arrow’s conditions might restore stability.

As noted above, Black offered one of the most successful strategies for avoiding cyclical majorities by restricting the admissible domain of preferences. In particular, he showed that simple majority voting produces a unique transitive outcome if individual preference orderings are single-peaked. By “single-peakedness,” Black meant a curve with a single optimum point. Since each curve represents a committee member’s preference ranking, the intuition is that “the further a motion departs to the right (or to the left) of a member’s optimum, the less he desires that that motion should be adopted." (Black 1958, p. 10) Riker summarized the condition as follows: "By 'single-peaked' is meant that all the individual orderings can be represented on a graph with the rank of preference on the ordinate and the alternatives themselves on the abscissa such that each ordering (or preference curve) appears as a curve with one and only one peak." (Riker 1961, p. 906). Single-peakedness is a necessary condition for Black's median voter theorem.[21]

Black also attempted to compute the a priori probability of cyclical majorities, though without deriving a general rule. The main takeaway, imperfect but suggestive, was that, for $n \geq 5$ (where n is the number of alternatives), the a priori probability of obtaining a decisive majority ordering is small (Riker 1961, pp. 908–10). This observation points, for Riker, to a core puzzle of political science: given these low probabilities, "the surprising fact is that majority decisions occur at all." (Riker 1961, p. 909)

Applied to American political institutions, especially the Congress, social choice analysis can illuminate classic strategies of parliamentary maneuver. A prospective loser can transform the structure of preferences by introducing a new alternative, potentially converting a single-peaked configuration into a multi-peaked one. Such tactics recur in congressional history (Riker's example is the debate on the 17th Amendment). Similarly, voters or committee members can misrepresent preferences to secure a more favorable outcome.[22]

Riker concluded by summarizing why the literature he reviewed mattered for political science:

> "The main point of this bibliographical essay has been to suggest that the work of Black and Arrow is of great importance both for political theory and the study of political behavior. [...] *If students of political behavior were to discover and explain the range of mechanisms and social conditions leading to that agreement on norms sufficient for a set of single-peaked curves, then political theorists might be able to evaluate such notions as the public interest and the general will on the basis of empirical knowledge, a kind of procedure which is, I regret, almost unprecedented in the study of politics".* (Riker 1961, p. 911 my italics)

The emphasis above reinforces what was suggested earlier about Riker's intermediate position within Schubert's taxonomy. Formal analysis could support a "positive" program oriented toward empirical evaluation of political mechanisms and normative concepts. While Riker's expectations later cooled, this does not diminish the importance of the theoretical vocabulary he helped to introduce:

21 Arrow clarified the intuition using a "betweenness" relation, $B(x, y, z)$. A weakening of Black's condition was proposed by Michael Dummett and Robin Farquharson (Dummett and Farquharson 1961), who argued that a sufficient condition for stability is the existence of at least one outcome x such that, for every other outcome y, some majority regards x as at least as good as y.

22 Robin Farquharson explored this idea. See (Farquharson 1970; Dummett 2005).

> "Even if such a happy outcome is not possible, many spheres of political life can, I am certain, be more perceptively explained than they have been by the use of the theory here reviewed. (I think especially of legislative strategy, which most writers have treated as a mystical art, but which may, on examination by this theory, turn out to be a science with quite coherent rules.) And it is in the hope that at least some such explanations will be forthcoming that this review was undertaken."(ibidem)

Riker sent a copy of the article to Black, who read it favorably. In autumn 1962, Black led a graduate seminar at Virginia Polytechnic, where he met, among others, Buchanan, Tullock, and Coase (Black to Riker, May 14, 1962, Riker n.d., Box 5, Folder 1). The following year, Black joined Rochester for a semester. Riker's effort to reshape the department was about to begin.

3.2 The foundations of a "formal, positive political theory"

Riker described the work he aimed to pursue at Stanford as "formal, positive political theory" in a letter he exchanged with the Center's staff ("Supplementary Statements," Riker to Tyler, December 4, 1959, Riker n.d., Box 10, Folder 1). In his words, "formal" meant that the theory was to be expressed in algebraic rather than verbal symbols. "Positive," by contrast, referred to the descriptive, rather than normative, aim of his analysis.

In an inventory of personal interests that each fellow at the Center was asked to complete, he summarized his interests and current activities as follows:

> "At present, my main interest in political science is the development of a positive political theory. I am concerned, e.g., with such preliminary problems as an acceptable definition (and perhaps, measure) of power. Beyond this, I visualize the growth in political science of a body of theory somewhat similar in its role in the science to the neo-classical theory of value in economics. It seems to me that a number of propositions from the mathematical theory of games can perhaps be woven into theory of politics. Hence, my main interest at present is attempting to use game theory for the construction of political theory.
>
> Growing out of my interest in a formal theory of politics is an interest in determining whether or not any of the assumptions about behavior made in game theory are empirically valid. Just as the thoroughly deductive neo-classical theory of value acquires most of interest from the fact that it seems also in some ways to be descriptive, so also a thoroughly deductive political theory would be much interesting if had some descriptive validity."(Riker 1960)

Well before he arrived at Stanford, Riker had already begun working on several of the issues mentioned above. Those lines therefore condense the outcome of a sustained period of reflection and study that led him toward a more explicit commitment to formal analysis.

Setting aside, for the moment, his relationship with the community of game theorists (which will be addressed in a later section of the next chapter), Riker's interest in game-theoretic applications to political science began with Shapley and Shubik's paper on the measurement of power in a committee (Shapley and Shubik 1954). He then attempted to test it with empirical data (Riker 1959a). This early effort is revealing: it shows how Riker tried to work with game theory's core premise, individual, purposive action, while also retaining a strong interest in broader questions of social-scientific methodology (even if he sometimes seemed to exhibit a derogatory nuance toward these kinds of debates). He also published two brief but dense papers in this philosophical vein (Riker 1957; Riker 1958a).

3.2.1 Squabbling over methodology: Riker's philosophical papers (1957–1958)

Due to his frustration with the state of contemporary political science, Riker decided to engage directly with the methodological debates animating the discipline. He did so, however, "in a spirit of reluctant temerity," given his skepticism about the practical value of such disputes. At the opening of his first philosophical paper he wrote: "The social sciences are today so beset with squabbles over methodology that it seems we are more intent on talking about learning than we are on learning itself." (Riker 1957, p. 57) He did not aim at a single opponent, author, or narrow controversy. Rather, in two papers published in the *Journal of Philosophy*, he advanced a firm position on fundamental questions: what social science is for, and what it must do to achieve that aim.

These papers did not address, at least not explicitly, the themes that later became central to "Positive Political Theory", for instance, the role of models, the use of mathematics, or the detailed content of rationality assumptions. Nor did he, at this stage, frame his project as an "economic theory" of politics in the manner of Black or Downs (Black 1950; Downs 1957).

In his later interview with Shepsle, Riker recalled that, in his attempt to put political science on firmer foundations, he initially turned to the philosophy of science and to logic before undertaking more systematic mathematical training (Riker and Shepsle 1979). The questions that mattered most to him concerned whether political science could produce "exact" statements about the world, and how positive aspiration could be reconciled with normative judgment. In this perspective, a necessary starting point was to clarify what, exactly, the discipline studies and how its objects relate to one another. Hence, his focus on "events" and their causes.

From a philosopher of science's standpoint, these arguments can be read as vulnerable in several respects, including an occasionally stylized picture of natural-scientific practice. For present purposes, however, it is more useful to treat them as a kind of manifesto: less an intervention in professional philosophy than a programmatic statement of what Riker took scientific inquiry in political life to require. They also became touchstones in his later methodological defenses of formal political theory.

In the first paper, Riker emphasized the need to circumscribe the events that a scientific inquiry purports to explain. He criticized both the ambition to construct universal "theories of society," which he associated with a "poetic tradition in the social sciences" (Riker 1957, p. 70), and the opposite tendency to dwell on the idiosyncratic detail of singular, historically rare occurrences. Given what he described as the ultimate aim of social science, namely scientific explanation, both approaches fail, above all, because they are insufficiently clear about what, precisely, is to be explained.

His argument proceeds as follows. The raw material of social inquiry is "motion" and "action," understood as conduct taking place within a surrounding context. The basic unit is an "event," defined as a "subjectively differentiated portion of motion and action" (Riker 1957, p. 58). Events form a continuous, beginningless and endless stream. But because individuals cannot grasp the totality of continuous reality, they introduce boundaries—starts and stops—into that continuum. Events are what lie between those imposed boundaries (Riker 1957, p. 59). The motion and action are "objectively existent," but the boundaries are "subjectively imposed" (Riker 1957, p. 60). Riker calls these imposed boundaries "situations" (an initial and a terminal situation). Unlike events, situations contain no temporal "portion" of reality; they are treated as instantaneous snapshots.

A situation is characterized by its "form" (the condition of movers and actors and their arrangement in space) and by its "condition" (its history). Any situation can serve as the initial or terminal situation for indefinitely many events. Within this setup, Riker therefore defines an event more precisely as: "*the motion and action occurring between an initial situation and a terminal situation such that all and only the movers and actors of the initial situation (or the component into which they are formed in the course of the event) are included in the terminal situation*" (Riker 1957, p. 61, italics in the original). On this view, an event is "ambiguous" if it fails to include all movers and actors needed to explain it.[23]

To defend his argument, he uses as a historical example the outbreak of the First World War: analysts set different initial situations for what is nominally "the same" event: sometimes the Austro-Serbian controversy, sometimes the configuration of alliances, sometimes "the whole world" (Riker 1957, p. 63). In his terminology, event A is unambiguous: it spans the war from its beginning to its end. Event a, by contrast, is ambiguous: it shares

[23] To complete his discussion of ambiguous and non-ambiguous events, Riker also lists a taxonomy which comprises some "general canons" for determining the ambiguity of events: i) Some events are so complicated that they are inherently ambiguous (for instance, where there is a vast number of actors and movers, like in the "world histories"); ii) Some events with no clear initial situations are probably ambiguous; iii) Some events with no clear terminal situations are probably ambiguous; iv) Events that are presently occurring and then without terminal situation are probably ambiguous; v) Large events with many movers and actors and a great extent and duration are likely more ambiguous than small events with few movers and actors. This point is worth mentioning because, in Riker's view, is one of the chief lessons that social scientists can learn from natural scientists (Riker 1957, p. 65).

the same terminal situation as A, but it can be assigned different initial situations, ranging from "the whole world" to the Austro-Serbian controversy, or to the broader configuration of relations between the Allied and Central Powers. Hence, for Riker, clarifying what we mean by "the World War" requires comparing these alternative (and ambiguous) delineations, and identifying which initial situation, and which set of relevant actors and conditions, actually belongs to the event under explanation. This example supports his central methodological claim: social inquiry must begin by carefully specifying the event to be explained, which often means prioritizing smaller events over larger ones, because the latter are more likely to be ambiguous.

Riker explicitly links this prescription to what he takes to be the successful development of the natural sciences:

> "It is commonly said that the natural sciences have been more successfully developed and systematized than the social sciences because, for one thing, the natural sciences have a longer tradition and a vastly greater body of observation, and because, for another thing, they deal with an unimpassioned subject matter in which the observer need not become morally and emotionally involved. While these advantages of the natural sciences are undoubtedly great, the greatest of all seems to me that from the beginning they have dealt with small events. [...] One of the chief advantages of this method is that, in a tract of time, unnoticed movers and actors are eliminated from events, or at least they are identified and their motion and action described." (Riker 1957, p. 68)

Near the end of the paper, Riker briefly invokes von Neumann (without developing the point), suggesting that von Neumann's work exemplifies the analytical payoff of focusing on smaller, more tractable phenomena. The remark is also compatible with the "necessary limitations of the objectives" stressed in the opening chapter of von Neumann and Morgenstern's *Theory of Games* (Neumann and Morgenstern 1944, 5 et ss.). Compare, for instance what the two authors said:

> "It is necessary, to begin with, those problems which are described clearly. Even if they should not be as important from any other point of view. It should be added, moreover, that treatment of these manageable problems may lead to results that are already fairly well known, but the exact proofs may nevertheless be lacking. [...] The great progress in every science came when, in the study of problems which were modest as compared with ultimate aim, methods were developed which could be extended further and further." (Neumann and Morgenstern 1944, pp. 6–7)

Even so, the two positions are not identical. In the *Theory of Games*, the move toward "modest" problems is tied directly to individual behavior and simple forms of exchange. In Riker's 1957 paper, by contrast, the delimitation of events does not yet imply a substantive theory of agency.

The second philosophical issue Riker addressed concerns causal relations between events (Riker 1958a). He begins from a familiar definition of cause as a necessary and sufficient condition, but argues that the notion of sufficiency is especially problematic in social inquiry, where multiple candidate "causes" commonly coexist. He therefore proposes an alternative definition: "One event causes another if and only if the terminal situation of the causing events is identical with the initial situation of the caused event." (Riker 1958a, p. 282) Identity, here, is defined in space-time terms. From this, follow additional assumptions about the identity of movers and actors across situations.[24]

Riker then argues that this definition entails both necessity and sufficiency, in the sense that if the terminal situation of A is identical with the initial situation of B, then A contains (and is) a necessary condition of B, and, in the sense specified by his assumptions, also sufficient.[25] A key implication is that, since any situation can be the terminal situation of multiple events, an event B may have many causes $A_1, \ldots, A_n$ (potentially indefinitely many) so long as each satisfies the identity conditions. He therefore lists constraints that all admissible causes of B must satisfy.

The advantage of this approach for the study of events in the social sciences is that it provides a clear and operational definition of causality. Causality is defined by separating two events, A and B, such that the terminal situation of A coincides with the initial situation of B. Now suppose that a given situation can serve as the terminal situation of an infinite number of events. In that case, any situation that functions as the initial situation of an event B may also be the terminal situation of a set of events $A_1, A_2, \ldots, A_n$, each of which can be regarded as a cause of B. Even if the number of such causes is infinite, they must all satisfy the same constraints:

1. All causes of B share an identical terminal situation.

2. The movers and actors involved in each cause of B are identical to those involved in B itself.

24 Riker advanced a series of assumptions, which determine the more general definition of cause in the context of social sciences. Thus: i) Two situations are identical if and only if their locations are identical; ii) Two situations are identical if and only if their movers and actors are identical; iii) Two locations of situations are identical if and only if the movers and actors of the situations are identical; iv) If two locations of situations are identical, then two situations are identical (Riker 1958a, 285 et ss.

25 The proof of sufficiency is the following: A preceded B in time-space (by the assumed identity of locations of the terminal situation of A and the initial situation of B). By assuming the identity of actors and movers in the terminal situation of A and the initial situation of B, all the movers capable of affecting B are in A. Then, A is at least a sufficient condition of B, and B would not have occurred unless A or something in A had occurred. Given the conditions above, these can be extended to say that the things capable of affecting B are only in A (proof of necessity). (Riker 1958a, 288 et ss.)

3. Each cause of B includes all and only the movers and actors included in every other cause of B.

4. The initial situation of each cause of B includes all and only the movers and actors included in the initial situations of the other causes of B.

At the same time, while acknowledging that causality necessarily implies antecedence, Riker argued that its theory did not collapse causality into mere antecedence: the real issue is the need to restrict the kinds of antecedence that are admissible. Such restrictions are already built into the definition of causality he proposed, and they are precisely what allow one to avoid the standard logical fallacies associated with causal inference.

Even on their own terms, the arguments raised in both papers leave important questions open. Riker does not take a clear position on the balance between deduction and induction, nor does he discuss the role of models, differing standards of explanation, or the ways in which "causal" claims function across different social-scientific practices. The distinction between natural and social science is explained primarily in methodological terms: natural science progressed by identifying repeatable patterns in small, carefully bounded events, which are "less subject to ambiguity than large events: small events can often be precisely bounded; and, failing that, statistical techniques can often be used to resolve such ambiguity as remains." (Riker 1957, p. 69) Social science, instead, too often pursues either sweeping universal claims or overly particular narratives.

It must also be noted that, despite some concessions to the role of individual actors, Riker's argument is by no means a theory of rational action. It seems primarily a theory of 'objective facts' in social sciences. Therefore, it is close to a form of solid realism, although mitigated by the idea that at least "situations" can be subjectively imposed.

From the standpoint of intellectual history, it is also useful to place this argument against the background of mid-century economics. Riker's prescription to focus on small events sits uneasily alongside the contemporaneous consolidation of highly general results in mathematical economics, most notably existence proofs in general equilibrium theory. Whether this reflects unfamiliarity with the cutting edge of economics, or rather a selective reading of "economics" through the lens of his own training and his early engagement with the *Theory of Games*, remains an open interpretive question.

3.2.2 "Does the Political Man seek to maximize 'power'?"

> "The economists once invented the Economic Man whose aim in life was to maximize profit or a suitable generalization of it. Game theory suggests the possibility of a theory of coalitions. Presumably, such a theory relates to the Political Man. Does the Political Man seek to maximize 'power'? To determine this one must develop an index of power and then discover whether in actual cases real men attempt to maximize what it measures." (Riker 1959a, p. 120)

This quotation, from the abstract of Riker's first article on Game Theory in political science, summarizes the purpose of his 1959 paper published in *Behavioral Science*. In his correspondence with Tyler, Riker described it as "an attempt to estimate the adequacy of an important assumption in the new formal or mathematical political science" (Riker to Tyler, June 22, 1959, Riker n.d., Box 10, Folder 1). The assumption at issue concerned Shapley and Shubik's power index, a result that, by Riker's own retrospective account, played a crucial role in his intellectual trajectory (Riker 1992; Shapley and Shubik 1954). It is therefore unsurprising that his first explicitly game-theoretic contribution was framed as an empirical probe of that measure.

Empirical work testing game-theoretic concepts had already begun at RAND in the early Fifties. A well-known example is the set of laboratory experiments conducted in the summer of 1952 on cooperative games, especially bargaining, negotiation, and coalition formation, by a RAND group that included Nash (Kalisch et al. 1952). Other experiments focused on non-cooperative settings, including the prisoner's dilemma and related conflict games.[26] These efforts ran in parallel with psychological testing of the axiomatic theories of utility and decision-making associated with the postwar formalization of choice (Moscati 2018).

Riker was aware of this broader empirical turn. He also explored experimental coalition games later, especially after his move to Rochester, when institutional resources became available (Riker 1970).[27] His 1959 test of the power index, however, was not a laboratory study but an application to observational material: he collected data on party switching in the French National Assembly, drawing on his comparative politics teaching at Lawrence (Riker and Shepsle 1979, p. 54). Relative to set-valued solution concepts (such as the stable set), the Shapley–Shubik index had an obvious advantage: it yields a single number, and is therefore straightforward to compute. Neither Shapley nor Shubik was trained as a political scientist. Shapley was a mathematician (closely connected, personally and intellectually, to Nash) and spent most of his career at RAND, producing foundational work in cooperative game theory.[28] Among his most influential contributions is the Shapley value, a solution concept for n-person transferable-utility games with binding agreements. Unlike von Neumann and Morgenstern's stable set, and unlike the core, the Shapley value is not

[26] More in general, on the history of Experimental Economics see: Maas and Svorencik 2016; Smith 1992.

[27] In 1958, Riker created a 5-person parlor game, called *Talleyrand* in which the object of the players is to form a winning coalition to take any amount of money in the course of the play away from the losers. This game is an extension of von Neumann and Morgenstern's game, *Couples*, a three-person game where it was asked to each player to choose the number of one of the two other players. If two players have chosen each other's number, they form a couple and share $\frac{1}{2}$, whereas the excluded member loses -1 (Neumann and Morgenstern 1944, pp. 222–3; Riker 1962b, pp. 52–3).

[28] Shapley was awarded the Nobel Prize in Economics in 2012, alongside Alvin Roth. For further biographical information, see also: https://www.nobelprize.org/prizes/economic-sciences/2012/shapley/facts/

built on stability criteria. Rather, it formalizes each player's "reasonable expectation of reward" via an a priori evaluation of marginal contributions across possible coalitions (Shapley 1953; Roth 1988; Taylor 1971).[29] Shubik, by contrast, was an economist with a Princeton Doctorate. In their collaboration, Shapley provided much of the mathematical innovation while Shubik oriented the work toward substantive social-scientific problems.[30]

One such problem was the perennial political question of "power." Shapley and Shubik approached it through a deliberately narrow, technical definition, and their short (non-axiomatic) paper appeared in the *American Political Science Review* in 1954 (Shapley and Shubik 1954). As Riker later summarized their result:

> "[...]Most persons who have tried to analyze power have interpreted it as the ability of one person to make another person do something the other would not otherwise do. [...] it is clear that Shapley's definition is quite different. It involves not the ability to control persons but the ability to control outcomes by means of being the pivot or the marginal person between winning and losing coalitions: the last added member of a minimal winning coalition." (Riker 1992, p. 212)

In their framework, "power" is the probability that a committee member is critical to the success of a winning coalition (Shapley and Shubik 1954, p. 787). The index is conceived as an a priori property of the voting rule and the committee's composition, abstracting from sociological structure and partisan organization. Even so, Shapley and Shubik suggested that deviations between theoretical expectations and observed behavior could serve as indicators of political solidarity, factionalism, and related phenomena.

Formally (and following Riker's own summary), consider a voting body with n members and a victory rule (e.g., simple majority $\frac{n+1}{2}$ if n is odd or $\frac{n}{2}+1$ if n is even). Let $n!$ denote the number of possible voting orders. Define a Minimal Winning Coalition as one that ceases to be winning if any member is removed. In any given voting order, the pivot is the member whose vote turns a losing coalition into a winning one. This means that the marginal value of the vote after simple majority is attained is zero.[31] The power index P_i for member i is the fraction of voting orders in which i is pivotal. Hence $\sum_{i=1}^{n} P_i = 1$.

29 As Riker and Ordeshook stated: "The V-solution is inferred from the characteristic function in answer to the question: how might players in each coalition be expected to divide its value? On the other hand, the Shapley value is inferred from the characteristic function in answer to the question: how much might players expect to win, given various possibilities of coalitions?" (Riker and Ordeshook 1973, p. 163)

30 For a first-hand narrative of their collaboration see: https://www.informs.org/Resource-Center/Video-Library/H-T-Videos/INFORMS-History-and-Traditions-Interview-with-Martin-Shubik

31 As Shapley and Shubik wrote: "Put in crude economic terms, the above implies that if votes of senators were for sale, it might be worthwhile buying forty-nine of them, but the market value of the fiftieth (to the same customer) would be zero." (Shapley and Shubik 1954, pp. 787–8) Note that in 1954 U.S.Senate comprised 97 members.

Shapley and Shubik also sketched several properties of the index (without formal proofs, likely reflecting their intended political-science audience). For example, in a pure bicameral system with simple majorities, each chamber carries half the total power, implying that (holding other features constant) power is inversely related to chamber size. In multicameral settings, power depends on the majority thresholds in each chamber: raising a threshold increases that chamber's relative power. Under unanimity rules, each individual has equal pivot probability, and chamber power becomes proportional to size.

They also proposed the possibility of empirical validation: one might use voting records, suitably aggregated, to compare observed patterns with theoretical expectations. Riker's 1959 article attempted precisely this, studying party switching in the second legislature of the French National Assembly. The assembly's multiparty structure, strong party discipline, and relatively frequent migrations made it attractive for the hypothesis he wished to examine.

Riker's central behavioral hypothesis was power maximization: legislators change party affiliation in order to increase their Shapley–Shubik power. If so, the index should rise after a migration. As he put it:

> "The economist knows of course that there is no such thing in the real world as an economic man who singlemindedly maximizes profit; still the economist is quite certain that this abstraction is worth discussing because he is also quite certain that most people in the real world do want money. But the political scientist is not so certain that his abstractions have any relevance at all to life. A political man who singlemindedly maximizes power is worth discussing only if it can be shown that people in the real world want power, or at least the kind of power that is measured by the power index. Hence, one of the pressing necessities for a political *science* is some evidence on whether or not men seek power." (Riker 1959a, p. 121, italics in the text)

It is worth stressing that this is Riker's hypothesis, not an assumption built into Shapley's or Shubik's framework. The Shapley value was derived axiomatically, without commitment to the behavioral rationality assumptions embodied in von Neumann and Morgenstern's solution concepts. Indeed, Shapley explicitly framed his analysis as pertaining to abstract "roles" within a game rather than to psychologically specified "players" external to it (Shapley 1953, pp. 31–2). Likewise, Shapley and Shubik did not themselves posit a strong power-maximization model of legislative behavior; the index is introduced primarily as a structural measure of pivot potential.

To operationalize the maximization hypothesis, Riker relied on weighted majority games, namely cases where voting weights and "power" diverge.[32] He did not assume that legislators explicitly compute the index. Rather,

[32] Riker's example is the following: a 3-person game where the players a, b, c are weighted respectively 50, 49, and 1. Given six possible voting sequences, the

he conjectured that they can "sense" relative bargaining opportunities and behave in ways that tend to preserve or increase them (Riker 1959a, p. 122).

Formally, if the power of a party A is distributed among its m members as $P_i = (\frac{P_A}{m}, G_\alpha)$ (a function of the party's total power, its size, and the decision rule) and if a legislator moves to party B, the legislator's power becomes $(\frac{P_B}{m}, G_\beta)$. Riker defines the individual gain/loss for each migrating legislator j as $R_j = (\frac{P_A}{m}, G_\alpha) - (\frac{P_B}{m}, G_\beta)$, and considers the aggregate $\sum_{j \in M} R_j$ for the set M of movers. He distinguishes the two broad cases:

1. $\sum_{j \in M} R_j > 0$
2. $\sum_{j \in M} R_j \leq 0$

and connects these outcomes to four interpretive hypotheses:

1. Legislators increased their power consciously
2. Legislators increased their power by chance
3. Legislators consciously rejected favorable outcomes (e.g., for ideological reasons)
4. Legislators decreased their power by error

Riker associated a large positive number with case 1. A small positive number with cases 1 and 2. And similarly, a large negative number with case 3 and a small negative number with cases 3 and 4. Finally, he added a further hypothesis regarding the gain of the party migrated to, namely $Q_j = \frac{m'(P_B, G_\beta}{m} - (P_A, G_\alpha)$, where m, m' are the size of parties A and B.

He then tests these implications using data from the second legislature of the French National Assembly, focusing on 1953 and 1954 (34 party changes; 61 individual moves; 46 members) (Riker 1959a, pp. 124–8). The result is, at best, ambiguous.[33] Riker offers three possible interpretations. First, the a priori index may be irrelevant to the empirical case because it abstracts from strategic and institutional features, for instance, the presence of "quasi-permanent" winning coalitions, which would make pivot probabilities a poor guide to actual bargaining leverage. Second, legislators may want power but be unable to identify power-improving moves with any reliability. Third, legislators may simply be indifferent to the kind of "power" captured by the index—an interpretation that, within Riker's own framing, pushes "ideological" motivation toward the label "irrational" (Riker 1959a, p. 131).

difference with simple majority games is that a majority of 2 out of 3 represents a winning coalition in the latter. The second voter in the voting sequence is ever pivotal. Therefore, P_i for each member corresponds to $\frac{1}{3}$. However, for a weighted majority, where the minimal winning coalition needs 51 votes out of 100, c and b are pivotal only once, whereas a is pivotal four times. Then, $P_a = \frac{2}{3}$, $P_b = \frac{1}{6}$ and $P_c = \frac{1}{6}$.

33 Indeed, according to Riker's computations, $\sum_{j \in M} R_j < 0$, which could correspond to cases 3 and 4.

Yet Riker did not regard the outcome as a failure. Even negative or inconclusive findings could be methodologically productive, insofar as they eliminate attractive but unsupported hypotheses and thereby help define what a genuinely scientific explanation of politics might require. He also noted that this single empirical application was insufficient to reject the index in general. Indeed, he returned to related questions later, including work on weighted voting with Shapley (Riker and Shapley, 1966) and an application of power indices to coalition formation with Brams (Riker and Brams 1972).

Finally, this early encounter with the Shapley–Shubik framework appears to have shaped Riker's subsequent choices in two ways. First, in *The Theory of Political Coalitions*, he relied more heavily on von Neumann and Morgenstern's original cooperative framework. Second, he moved away from treating political rationality as straightforward maximization of "power," and instead placed greater emphasis on the preference for winning, an assumption that becomes foundational for the argument its next main work.