Looking for a 'genuine Science of Politics'. William H. Riker and the Game Theoretical turn in Political Science

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Abstract

The paper aims to show how the formal revolution in economics has influenced the developments of Rational Choice and Game Theory in Political Science. Our focus will be on American political scientist William H. Riker (1920-1993). We want to show how Riker used game theory and adapted it to fulfill his disciplinary agenda, contrasting the main trends in postwar American Political Science. Our thesis is that, in doing so, Riker stressed some aspects of the theory that differed quite sharply from postwar mathematical economics to which game theory belonged. Sections 1 and 2 describe Riker's education and intellectual life until the late 1950s, showing how he became acquainted with game theory and how American political science as a discipline was changing in that crucial decade. Section 3 presents Riker's main arguments in the *The Theory of Political Coalitions*, focusing on his working through game theory to produce a suitable model of political coalition-building. Section 4 discusses some methodological aspects of Riker's commitment to game theory and economic analysis. In particular, we aim to outline and discuss an apparent "dilemma" in his theoretical production, namely his resting on an outdated idea of economics, despite his use of game-theoretic analysis. Finally, Section 5 offers concluding remarks.

Keywords: Game Theory, Political Science, Positive Political Theory, William H. Riker

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Introduction

Although game theory now occupies a central role in the toolkit of economic theorists, a remarkable feature of its history is that in the 1950s, 1960s, and

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1970s, this approach struggled to enter the economics mainstream. However, in a discipline striving to develop a formal, truly scientific, and possibly unified methodological canon, such as the 1950s Political Science, the kind of formal theory initially proposed by John von Neumann & Oskar Morgenstern was immediately perceived by some scholars as the ideal tool-box of methods, notions, and techniques. This aspect also points to another remarkable feature of game theory. Namely, it was a tool capable of being exploited across different disciplinary domains other than economics. This paper shows that in the 1950s and 1960s, game theory occupied a central place in a definite intellectual agenda, and this was not part of economics but political science instead.

Such a push for the adoption of game theory outside economics had its roots in some reviews of von Neumann & Morgenstern's opus (Neumann and Morgenstern 2007) like Herbert Simon's one (Simon 1945). For what concerns political science, the 1950s saw some early results obtained through the so-called "cooperative game theory," like those by the future Nobelist Lloyd Shapley and fellow economist Martin Shubik (Shapley and Shubik 1954). These must be added to the well-known attempts to use the theory of games in international relations. (Kaplan 1957; Schelling 1980) However, it was thanks to the commitment of a political scientist, William H. Riker, that game theory became the primary tool of the attempt to develop a truly scientific political science. As Riker wrote in the first chapter of his most ambitious theoretical work, *The Theory of Political Coalitions* (1962), "the main hope for a genuine science of politics lies in the discovery and use of an adequate model of political behavior." (Riker 1962b, p. 9). Such a model to him was eminently game theoretical.

From Riker's dedication, a long line of academic research in political science, which he defined as "Positive Political Theory," was established, where decision theory, game theory, and a rigorous formal fashion in assessing political phenomena represented the methodological canon to be adopted. (Amadae and Mesquita 1999; Riker and Ordeshook 1973)[1]

Riker's story fills different narratives, to which this paper aims to contribute. The first is the History of Game Theory and, more generally, postwar mathematical economics, following the trail blazed by the works of Weintraub, Giocoli, and Leonard, among the others (see: Weintraub 1992; Weintraub 2002; Giocoli 2003b; Leonard 2010). To these works must be added the reconstructions of the development of the postwar American Social Sciences, and the notion of "cold-war" social sciences (see: Amadae 2003; Erickson 2015; Erickson et al. 2015; Hauptmann 1996). Finally, the development of American political science has been explored in many historical works. Indeed the dramatic changes that occurred in the discipline in the Postwar

[1] "Positive Political Theory" was the name adopted by Riker yet in the 1950s. It will later become the standard label to define this subfield of political science. Riker's approach has also been labeled sometimes as the "rational choice approach" in political science due to the emphasis put on methodological individualism and rationality. Besides, an important aspect of this story that is not discussed here is Riker's role as an 'intellectual entrepreneur" within the academic community of political scientists. Indeed, he did not just advance a methodological as well as theoretical agenda but was tireless in building up a group of scholars who shared his view. This came mainly from Riker's role in chairing the department of political science at the University of Rochester from 1962 to 1977, and especially in establishing a Ph.D. program, to which such scholars like Peter C. Ordeshook, Kenneth Shepsle, Richard D. McKelvey, John Aldrich, among the others, joined, namely some of the most important political scientists which employed formal theory in their works.

years (both in terms of its institutionalization and theoretical developments) framed Riker's intellectual enterprise. Then, in the 1950s, a young generation of political scientists, like David Easton, Robert Dahl, and David Truman, among the others, challenged the traditional approaches to political studies (history and law), advancing a plea for more empirical and quantitative analysis. Such a movement is known as the "behavioral revolution" in Political Sciences. (Dahl 1961; Somit and Tanenhaus 1967; Adcock 2009; Adcock and Bevir 2006)

Riker's story fits all these narratives since he was a political scientist doing political theory when such "behavioral revolution" occurred. Riker indeed joined the "protest" against traditional political science, but not the revolution as a whole, focusing mainly on some tenets like the "plea" for objectivity in political theory. In doing so, he explicitly saw economics as a role model and adopted game theory. Therefore, this paper also aims to contribute to the ongoing literature about the various attempts to extend the economics toolkit outside its traditional boundaries, closely following other works (see, for example, Medema 2000; Medema 2013; Fleury 2010). We intend to show that Riker's case was not a simple transfer of tools from one domain to another. The American political scientist indeed stressed those aspects of economic theory that fit his scientific political science idea. However, he interpreted economics, especially its methodology, differently from what postwar mathematical economics was becoming.

Riker's use of game theory and formal analysis was strictly influenced by his being a political scientist working within the political scientist' community. Then, whereas the development of game theory deeply affected the way the Postwar economists used to address theoretical problems, both in terms of mathematical sophistication and how they interpreted economic knowledge, Riker saw game theory differently. To him, this tool enhanced a strong predictive power, and his employment of it is justified on the basis of a robust positivistic attitude, which resembles Milton Friedman's case for "positive economics," but also the "nomological-deductive" approach. However, it is well known that such positions were far from being the main force that fuelled economics' radical postwar mathematization. This, in our eyes, represents a sort of "dilemma" that, as it will become apparent, Riker was not really able to solve. This work explores why Riker chose such a methodological point of view and how this affected his use of game theory or the immediate suitability of his model. In a nutshell, our thesis is that Riker presented to political scientists a relatively less sophisticated image of economics than what economics was becoming in the postwar years, and we try to advance some explanations for this.

Section 1 and 2 describes Riker's education and intellectual life until the late 1950s and how he became acquainted with game theory. Section 3 presents Riker's main arguments in *The Theory of Political Coalitions*, focusing on his working through game theory to produce a suitable model of political coalition-building. Riker's argument in this work was not mathematical nor axiomatic. Thus it is very different from the high theoretical game theory development in the 1950s (think of Princeton and RAND theorists). Consequently, it does not occupy a central place in the history of the development of game theory *qua* theory. However, it had an important role in how game theoretical ideas crossed domains different from economics. Section 4 discusses Riker's relations with economic theory and outlines his methodological "dilemma." Finally, Section 5 offers concluding remarks.

1 William H. Riker's education and early works

William Harrison Riker was born in Iowa in 1920 and grew up in Michigan and later in Indiana, where his father, in the years of the Great Depression, established a bookstore.[2]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). Riker, in his remarks, attributed influence to these undergraduate studies in economics only for what concerns the 'mindset' of economics and not for the specific training he received. Furthermore, it is likely that the general undergraduate education in economics at the time, especially in a relatively small private provincial college like DePauw, was of low interest and low quality (and devoid of any theoretical inclination). After having completed his undergraduate studies, he decided to apply for Graduate School in Political Science. Riker recollected that his set of possible choices comprised Harvard University, Columbia, and Chicago, namely, "[t]he three schools that were producing substantial numbers of political scientists at the time" (Riker and K. Shepsle 1979, p. 36). Especially the latter was associated with Charles Merriam and the "Chicago School of Political Science," who put much emphasis on empirical methods and quantitative analysis. (On the "Chicago School" see: Heaney and Hansen 2006) Thus, his political science professor at DePauw, Harold Zink, advised him to apply to Chicago. However, at the time, Riker was influenced by the writings of E. Pendleton Herring, a professor at Harvard. Therefore he decided to enroll there in 1945. Herring was a generation younger than Merriam but followed him in advocating scientific methods in social sciences and had a pivotal institutional role in developing the "Social Sciences Research Council," which he chaired from 1948 to 1968.(Riker and K. Shepsle 1979, 36 et ss)[3]

Young Riker's Harvard experience is extremely interesting to properly understand the state of Political Science in the late 1940s and 1950s. In

[2]One can derive information about Riker's life and career from the brief biographical memoir written by two scholars connected to him, Kenneth Shepsle and Bruce Bueno de Mesquita. The former obtained a Ph.D. in Political Science at Rochester in 1970 and was among the first Riker's graduate students. Bueno de Mesquita instead did not attend Rochester Graduate School, but he arrived as a professor in 1972 and was very close to Riker. They wrote this memoir for the biographical series of the National Academy of Science, to which Riker belonged since 1975 (Bueno de Mesquita and K. Shepsle 2001). It offers exciting accounts of Riker's personality and family life and his role as teacher and mentor, but, given the nature of the series, the general tone is sometimes acquiescent and celebratory. In addition, Riker himself offered some different historical accounts of intellectual development (Riker 1992;Riker 1997). There, he aimed to present a general reconstruction of his academic experience, especially concerning applying Game Theory in political science. Hence, his narrative is often generic and not precise, at least from a historiographical point of view. The most interesting source for reconstructing Riker's life is certainly the long and detailed interview Riker gave to Shepsle in 1979 as part of the 'Political Science Oral History Program. This program started in the late 1970s to preserve the experiences of major figures in the development of American political science to the benefit of future historians and practitioners of the discipline. (Riker and K. Shepsle 1979) This 150 typed pages interview spanned from graduate and undergraduate education reminiscences to theoretical and methodological issues.

[3]E. Pendleton Herring (1903-2004) was an American political scientist, a generation younger than Merriam but followed him in advocating scientific methods in social sciences. Besides, he also was strongly involved in the institutionalization of the discipline in the Postwar, chairing the "Social Sciences Research Council" from 1948 to 1968. Dahl, in particular, considered the "Social Science Research Council" one of the "six specific, interrelated, quite powerful stimuli" that backed the development of the "behavioral approach" in political science. (Herring 1947;Dahl 1961, 763 et ss.)

particular, it can be seen as a proxy of the problematic relationship between younger and innovative scholars within the unsatisfactory framework of a consolidated discipline. He arrived at Harvard before the period of intense methodological development and commitment to the quantitative method he would later define as "the ferment of the 1950s," which coincided mainly with the 'behavioral revolution' in political science and the development of new quantitative and qualitative analyses. (Riker 1997) At Harvard, in the 1940s, instead, the approach followed even by scholars more committed to the scientific method like Herring was mainly that of case studies, with a particular focus on public administration. Otherwise, the other important strand of research was the History of Political Ideas. Whereas Riker dismissed the earlier as "simply artistic investigations of events" (Riker and K. Shepsle 1979, p. 39), the latter instead embodied what David Easton, at the time Riker's fellow graduate student at Harvard, defined as the "historicist attitude in modern political theory," equally opposed to empirical and theoretical analysis. (Easton 1951) In Riker's own words, such a historicist attitude was defined as follows: "The idea was to have a clever interpretation of some event or a clever interpretation of some historical development and not to have a scientific approach to politics." (Riker and K. Shepsle 1979, p. 45) To his eyes, its main representative at Harvard was the famous german scholar Carl J. Friedrich, about whom he spent some harsh words in the interview with Shepsle.(Riker and K. Shepsle 1979, p. 41) In reality, in the pre-behavioral era, Friedrich was among the few scholars interested in setting empirical research within an original theoretical framework by establishing a theory of power. Consequently, he was pretty dismissive, as Riker was, of simplistic empirical analysis. But to Riker, Friedrich's work, equally hostile toward any general description of political events, was without any usefulness in establishing a science of politics.

Riker concluded his Ph.D. in 1947 with a fairly classical case study dissertation titled *The CIO in Politics. 1936-1946* (under the supervision of Merle Fainsod). (Fellman 1947. The "Congress of Industrial Organization" was a former American trade union federation). In general, Graduate School experience did not satisfy him, and this mirrored the discipline's scientific and intellectual state. On the one hand, there were people like Herring (to whom Riker felt personally close) who pursued interesting practical analysis but with totally wrong methodologies and devoid of theoretical exploration. On the other hand, people like Friedrich defended the case for a theoretical discipline, but at the expense of any practical purpose, at least to Riker. In such an intellectual environment, the "Behavioral revolution" occurred in the late 1940s and early 1950s. It originated from the same intellectual concerns and dissatisfaction with the present state of political science Riker and other young scholars had. Take, for instance, the aforementioned Easton, whose work will be pivotal in its development. (Adcock 2009) He remarked, in an interview given for the 'Political Science Oral History Program,' that "by the time I left Harvard, I just didn't know what political science was all about." (Interview to D.Easton, in Baer, Jewell, and Sigelman 1991, p. 199) These words are extraordinarily similar to that of Riker. Indeed he stated that "people go out of Harvard without having any sense of doing anything in political science" (Riker and K. Shepsle 1979, p. 48) and he "had no sense of what one did as a scholar in political science when I got through and finally [got a] Ph.D. [at] Harvard." (Riker and K. Shepsle 1979, p. 44)

The main tenets of the "Behavioral revolution" entailed the emphasis

on prediction and the explanation of political issues. These were based on observation and data collection, the development of interdisciplinary and "self-conscious criticism'" about its methods and results. But also on pure research, leaving aside any normative aspiration to establish the "truth or falsity of values" like democracy, freedom, or equality, which are not amenable to scientific validation. Theoretical development occupied an important role in orienting and directing research. (Somit and Tanenhaus 1967) But this did not represent an explicit unified paradigm or set of theories. Instead, in the words of Robert Dahl, "Those who were sometimes called 'behavioralist' [...] shared a mood: a mood of skepticism about the current intellectual attainments of political science, a mood of sympathy toward 'scientific' modes of investigation and analysis, a mood of optimism about the possibilities of improving the study of politics". (Dahl 1961, p. 255) Up to conclude that "[...]' the behavioral approach' might better be called the 'behavioral mood' or perhaps even the 'scientific outlook'" (Dahl 1961, p. 258). Besides, according to Riker, an important point was also occupied by reformist goals and practical interest in public affairs, which animated the young political scientists. (Riker 1997)

Riker certainly shared such a mood but focused his attention more on the theoretical aspects rather than on the empirical analyses carried forward by behavioralists like Dahl or David Truman. Indeed, despite resting on more sophisticated methods than before, especially about statistical estimations (although far less advanced than incipient econometrics), the methodology of political theory was to him still grounded on flawed and unspecified ways of reasoning. This flaws became apparent to him after he published his textbook on the American political system, *Democracy in the United States* (Riker 1953), primarily based on his teaching course in American politics at Lawrence College (WI), where he worked between 1949 and 1961. (Riker and K. Shepsle 1979, p. 50) In his vivid account, working on this book led him to rethink the foundations of political science. In fact, after having published it, he started to realize that "it would be hard to say that any sentence in it was true." (Riker and K. Shepsle 1979, p. 60) Indeed "I began to think that once you raise the question of what can you do to bring a particular moral position into some sort of effective institutional operation, why you also raise the question of whether or not institutions accomplish what they are intended to accomplish" (Riker and K. Shepsle 1979, p. 2). Therefore, entailing an answer to these questions needed more than a normative stance and more than a plain description of how institutions work.

In Riker's mind, the issue at stake became that of ascertaining what political science is and if utter, or not, true sentences. Therefore, in his case, the strongest incentive to develop his intellectual agenda was the perceived need for a rigorous foundation of the methodological premises of the discipline. To pursue this ambitious aim, Riker started reading the philosophy of science and logic. But he soon realized that logic bore more on the validity of argument than its truth content. So, he paralleled these studies with more applied mathematical courses (linear algebra and Calculus) before discovering von Neumann's and Morgenstern's *Theory of Games and Economic Behavior* around the mid-fifties. (Riker and K. Shepsle 1979, 5 et ss.).

2 The "ferment of the 1950s": Riker meets game theory

Riker recalled that "[...] von Neumann's book was the one that really turned me on because it seemed to me that there were some generalizations there that [...] one could look at in nature and see if those generalizations turned out to be true." (Riker and K. Shepsle 1979, p. 5). Clearly, it was an intimidating book, especially for a political scientist with little math training. However, Riker felt confident to grasp the most important results from it, and, as we will show in the following section, he developed his theory of political coalitions entirely out of von Neumann & Morgenstern, and not on other results of cooperative game theory.

Riker quickly became "something of a publicist" for Game Theory in political science. For example, he insisted that the political theory panel at the "Midwest Conference of Political Scientists" at the University of Michigan, Ann Arbor (April 1958) was devoted to Game Theory, paralleling more traditional issues. (Harry Davis to Riker, January 10, 1958, WHRP, Box 18, Folder 2) On that occasion, Riker presented a brief working paper that exposed some game theory principles for political analysis to fellow political scientists. ("Contribution of Game Theory to Political Theory" (mimeo), Riker 1958b) He started by addressing some difficulties applying game theory in political science, commencing with political scientists' lack of mathematical training. Nevertheless, he stated, the game-theoretical approach had much relevance to politics since "the category of zero-sum, two-person games is clearly a model for those political situations in which two persons are each trying to do the other in. [...] The n-person game is clearly a model of the contemporary nation-state system of the free market of classical economy or of legislatures with undisciplined parties." (Riker 1958b, pp. 2–3). In doing so, he stressed the normative features of the theory over the descriptive analyses usually outlined in political science. Riker concluded his brief exposition with a simple game-theoretical model of the "balance of power system." A "balance of power system" in international relations assumes that to avoid the system's collapse each member opposes anyone who gains a predominance position. Riker showed that game theory could carry this idea one step further. With three coalitions, two opposing and a neutral one, he looked for those situations where the neutral's interest was to join the strongest coalition (so destroying the balance), and those where it was dominant be otherwise.[4]

When Riker's path crossed game theory, in the second half of the 1950s, some scholars had made key refinements upon the original theory of von Neumann & Morgenstern so that this now was labeled as "cooperative game theory," juxtaposed to the "non-cooperative" one. In a nutshell, cooperative game theory refers to the presence of coalitions, where communication and, therefore, binding agreements are possible. On the other hand,

[4]Riker's oversimplified model assumed two opposing coalitions and a neutral one. Therefore there are two possible outcomes: the neutral coalition joins the weakest opposing coalition, and the balance of power is maintained; otherwise, the neutral coalition merges with the strongest one, and the balance system is broken. By using Shapley & Shubik's power index, Riker computed the power of each coalition. Then he showed how much each coalition should offer to the neutral one to ally with it. Besides, Riker also attempted to display the best strategy for the neutral coalition. However, his model is far from being detailed and general, and even his employment of the power indices is a very special case, with arbitrary values.

non-cooperative theory refers to situations where players cannot communicate with others. Both these situations entail strategic and rational choice considerations, but the rules of the game and, therefore, the solutions are different. Princeton Mathematician John Nash was the first to introduce this distinction. He, differently from what von Neumann and Morgenstern had done in their original 1944 work, treated a case where it is "assumed that each participant acts independently, without the collaboration of communication with any of the others" (Nash 2002b) (viz. where coalitions are not permitted). Nash famously defined such games as "Non-Cooperative Games." The earlier games addressed in TGEB came therefore to be labeled as "Cooperative Games."[5]

In the cooperative case, the fundamental issue is that of choosing which coalition to join, given each player's expectations regarding the payoffs, what he will obtain by selecting a coalition over another (through "side payments"), and finally, which partition of the game represents the solution to it. Besides, in this case, there are mutiple notions of solution, each with axiomatic and substantive properties that make them acceptable, both from a mathematical and rational point of view. To von Neumann & Morgenstern, for n-person zero-sum games, each coalition's worth can be represented by a "Characteristic Function," and what each coalition gives its members by a vector, defined as "imputation." Due to their vectorial nature, each imputation can be related to another, using the notion of "dominance." Then, a solution for a n-person game is offered by the set of all imputations that do not dominate each other and dominate all the imputations outside this set (this was called the "Stable Set"). However, this was not the only solution for such games. Other ideas came out, stressing one property or another of the definition of cooperative games (as the "Shapley Value," discussed below).

In 1954, Martin Shubik, a young economist who studied under Morgenstern at Princeton and was perhaps the only economist deeply interested in Game Theory, edited a brief collection of essays exploring the theory of games in political behavior. (Shubik 1954; Shubik 1992)[6] The same year, Shubik, together with Lloyd Shapley, a Princeton and RAND mathematician[7] published, in the leading journal of American political science, *The American Political Science Review* a short theoretical work addressing the everlasting issue of political power, using cooperative game theory. (Shapley and Shubik 1954)

They defined power as the chance, represented by a "Power Index," each

[5]It must be noted that Nash, in his last contribution to GT, advanced an attempt to establish the "non-cooperative" foundations of "cooperative games," presenting a model of bargaining where the determination of the bargaining solution was obtained through a non-cooperative threat game. (Nash 2002c). An entire research program, the so-called "Nash Program," was derived, especially from the 1980s onward, after John Harsanyi and Reinhardt Selten invented some pivotal extensions and refinements of Nash Equilibrium

[6]This brief volume contained the pages where von Neumann and Morgenstern discussed the meaning of the notion of solution verbally; some reviews of TGEB; some research application of game theory to military problems ("The Colonel Blotto's game"), and finally, although not, strictly speaking, game-theoretical analyses, the first pages of Arrow's work on social choice, where he presented voting as a problem of collective choice, and Black's 1950 paper about the "unity between economics and political science."

[7]Shapley was a Princeton Mathematics Ph.D. who worked at RAND during the 1960s and 1970s, providing critical applications of cooperative GT and non-cooperative as well. He was awarded the Nobel Prize in Economics in 2012. For biographical information: https://www.nobelprize.org/prizes/economic-sciences/2012/shapley/facts/

committee member has of being critical to the success of a winning coalition. (Shapley and Shubik 1954, p. 787) The authors conceived their method as a first step to addressing such problems as designing the size and type of legislative bodies, protecting minority interests, and even finding a criterion for "fair representation." Their analysis does not consider any sociological or political superstructure in a legislature; nevertheless, they argued, it could help set up norms or standards, "the departure from which will serve as a measure of, for example, political solidarity or regional or sociological factionalism in an assembly." (Shapley and Shubik 1954, p. 791). This index represents the apriori chance for each committee member to be pivotal for a minimal winning coalition. Then, one can interpret Shapley and Shubik's power index as a "technical definition of power" compared to the popular analysis of power, namely, the ownership and use of resources or the act of domination or influence of some individuals over others.

In a formal, although not axiomatic way, their model can be summed in the ensuing terms:[8] given a voting body of n-members and a rule to define victory in a vote (for instance simple majority rule, $\frac{(n+1)}{2}$ if n is odd, or $\frac{n}{2}+1$ if n is even), members vote according to a sequence. Letting $n!$ the total number of these sequences [9], and defining a Minimal winning coalition, that is a coalition such that, if one member is subtracted, then it is not winning anymore, the member who transforms a coalition from losing to winning in a sequence is said to be a "pivot" (this means that the marginal value of the vote after $\frac{n}{2}+1$ is zero).[10]Therefore, concerning each member i, the power index P_i is the ratio between the number of sequences in which i pivots and $n!$ (where $\sum_{i=1}^{n} P_i = 1$).[11] Such a result is elaborated upon what is perhaps Shapley's most famous contribution to game theory, viz. the value named after him, a general solution for n-person games, with transferable utility and binding agreements. (Shapley 1953)[12]

In a late reminiscence, Riker attributed Shapley & Shubik's paper a pivotal role in catalyzing his interest in game theory. (Riker 1992) Then, he

[8]This is the summary made by Riker in his 1959 paper. See below.

[9]This means the factorial of n: $n \times (n-1) \times (n-2) \times \ldots 2 \times 1$

[10]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." (Note that in 1954 U.S.Senate comprised 97 members.) (Shapley and Shubik 1954

[11]In simple terms, think of a group of individuals who must vote for some bill in a given order. As soon as a majority is reached, the bill is passed, and the last member who voted is given credit. If the order of voting is chosen randomly, one can compute the frequency with which a member belongs to a group of voters, and the frequency of a member is pivotal. Then an index can be construed, which measures the number of times that the action of the individual changes the state of affairs. If this formal model is applied to a committee chairman's tie-breaking function, in an odd committee, he is pivotal as often as an ordinary member; in an even committee, he is never pivotal. Then, for instance, applied to the case of the US Senate in the 1950s, the power index of the US vice-president was equal to $\frac{1}{97}$ $(96+1)$

[12]Unlike von Neumann and Morgenstern's "stable set" and the "core," which also applied to similar games, the Shapley Value is not based on stability considerations. Instead, it entails the players' "reasonable expectation of reward," based on an apriori evaluation of the entire game. Namely, the value added to every coalition by a player is multiplied by the a priori probability that the coalition will form. The players' Shapley values constitute a unique payoff vector as the game's solution. (Roth 1988; Taylor 1971). 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

summed up the importance of 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. While I have deep reservations about this (and most other definitions of Power [Riker 1962]), it is clear that Shapley's definition is quite different. It involves not the ability to control persons but the ability to control outcomes through 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)

Not surprisingly, the first paper Riker devoted entirely to game theory was an attempt to assess Shapley and Shubik's result empirically. (Riker 1959a) In particular, he interpreted their result as the assumption that people seek to maximize their power. Therefore, their preference for joining a coalition over another is led by this "maximization principle." [13] However, as Riker pointed out, even if it could be demonstrated that each member raised his power by moving from one coalition to another, it could be impossible to prove that this was the leading motive behind his decision. Furthermore, Riker's empirical test simply consisted of computing, through the roll-call of the French Legislative Assembly, how the power of each member who switched his side changed after his migration, and the final empirical assessment was uncertain at most.

From Shapley and Shubik's power index, a vast literature emerged, which applied this and other similar indices to legislatures, committee decisions, or even to the analysis of fairness criterion. (Straffin 1994) However, when Riker made his ambitious attempt to provide a full-breadth game-theoretical analysis of political coalitions, he took from Shapley and Shubik only the notion of a "minimum winning coalition." Instead, he rested on the original analysis of von Neumann and Morgenstern. Moreover, he explicitly rejected any definition of rationality in terms of "maximization of power."

A decisive step in Riker's commitment to formal analysis was provided by the year (1960-1) he spent as a fellow at the "Center for the advanced studies in behavioral sciences" (CASBS) at the University of Stanford. The Center was established in 1954, thanks to the funding of the Ford Foundation, and quickly became part of the set of non-academic institutions that shaped the "cold war rationality." (Amadae 2003, pp. 78–9; Erickson et al. 2015). The Center was well-funded and gave many opportunities for interdisciplinary research in social sciences, displaying, therefore, a less hawkish attitude toward game theory and formal analysis than, say, RAND. Then, among the lists of fellows, we find political scientists, sociologists, economists, psychologists, historians, jurists, and philosophers.[14] Furthermore, it filled exactly the research opportunities Riker was looking for. Indeed, as he wrote in a letter to Ralph W. Tyler, a renowned educator who was the CASBS' first director, Riker aimed to develop "a new formal or mathematical political science" and "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, WHRP, Box 10, folder 1) It was at Stanford that Riker wrote a significant part of his book on political coalitions, but he also explored the other main strands of formal political theory, namely social choice and voting paradoxes, after the pivotal contributions made by

[13]Note, however, that in the original paper by Shapley and Shubik, no such strong maximization hypothesis was advanced

[14]The CASBS is still an active research center. One can consult a comprehensive list of fellows in decades on its site. Among them, eight future Nobel prizes in Economics

Duncan Black, Anthony Downs, and especially Kenneth Arrow (who was a professor at Stanford, and a CASBS' member). (Black 1958; Kenneth J Arrow 1962; Riker 1961)

Despite the time spent at the CASBS, there are no clues that Riker was much in touch with the community of game theorists in the 1950s. In general, with very few exceptions, the intellectual communities of economists, political scientists, and game theorists remained disjoint. The game theorists' community was made up, in the 1950s, of young mathematicians interested in the most abstract developments more than in practical employment. In places like RAND, the emphasis was allegedly put on the applied strand of game theory, e.g., its use in strategic and international political issues, but pure theoretical research was easily funded even there. In that community, not surprisingly, Riker was an outsider, lacking the necessary advanced mathematical capabilities needed to produce new mathematical theoretical developments. Furthermore, political scientists at RAND were anyway much interested in issues like nuclear deterrence or, in general, strategic analysis (the most notable case was that of Albert Wohlstetter, but also Thomas Schelling spent research time at RAND. Amadae 2003). Riker's focus on a well-defined issue, coalition formation in politics, and within a well-defined framework, namely "cooperative game theory," may explain his distance from game theorists and, more generally, from those dealing with international politics. But the most crucial reason for his being an outsider was perhaps that Riker was advancing his theoretical agenda by advocating game theory. This agenda differed considerably from traditional approaches in political science as well as from the most recent "behavioral revolution." Meanwhile, it was not easy to be integrated into the actual developments of game theory due to Riker's mathematical difficulties.

In the aforementioned interview with Shepsle, when asked if he ever sent any of his ideas to contemporary game theorists, Riker remembered only Duncan MacRae, whose response filled with detailed criticisms is not, unfortunately, among Riker's papers stored at Rochester.(Riker and K. Shepsle 1979, p. 14) Another equally interesting proof of his being an outsider among the community of game theorists can be found in Oskar Morgenstern's papers at Duke University. (Morgenstern n.d. Box 83) Riker completed *The Theory of Political Coalitions* in 1961 and sent the manuscript to Princeton University Press and Yale University Press. At Princeton, Morgenstern was extremely critical, rejecting its publication. (Shubik n.d. Box 8) Indeed, in a letter sent to Gordon Hubel (who was press editor at PUP), Morgenstern wrote: "The basic attempt is very laudable, and nobody doubts that Game Theory will influence Political Science vary considerably, but the execution leaves much to be desired." (Morgenstern to Hubel, August 16, 1961, Morgenstern n.d. Box 83) He continued: "Even the outline of Game Theory itself is full of misunderstandings and gaps. A reader not acquainted with Game Theory would not understand the exposition, and one already familiar with it would quickly spot the error". Morgenstern attributed the poor mathematical quality of Riker's manuscript to his having worked by himself and advanced the suggestion to establish some cooperation with a real game theorist or spend some time obtaining a specific education in it. In fact, before writing his comment, he tried to detect who Riker was and what were his education, capabilities, and scientific research, but without obtaining any meaningful information. Finally, to remark on his point, he stated that "I am sure that anyone else who is at home in Game Theory and who would

see this manuscript, perhaps given to him by some other publisher, would come to the same conclusion." (Morgenstern to Hubel, cit.)

Despite Morgenstern's harsh criticism and last remark, another refereeing was more supportive. Yale University Press contacted Martin Shubik at the end of June 1961 with the proposal of reading and making a referee (anonymously) of Riker's manuscript. (Marian Neal Ash to Shubik, June 30, 1961, MSP, Box 8)[15] For many aspects, the request made by YUP was not surprising since Shubik, as seen, was one of the few people involved in elaborating n-person Game Theory. Furthermore, although he divided his research activities between RAND and his work as an applied economist for important corporations (especially IBM and General Electric), he was also associated with Yale University. Indeed in 1961, he was visiting there, and he would become a full professor in 1963, spending all his career in New Haven.

Shubik began his report praising Riker's manuscript: "This manuscript is well worth publishing. It will make a rather controversial book containing several imaginative ideas." Moreover, "its worth depends upon an imaginative insight concerning the application of the methodology of game theory to the subject matter of political science." (Shubik 1961, p. 1) This, in his own eyes, can prevent the "unfair criticism" that political scientists and game theorists would have likely levied on technical grounds.

This point is interesting and deserves attention: Shubik's appraisal of Riker's work and his defense from "unfair criticism" rests more upon methodological aspects, that is, the employment of Game Theory in political science, than on technical grounds. In fact, he judged Riker's argument about minimum winning coalition "too verbal for one part of his audience and probably may not be sufficiently verbal for the other part." Besides, it is not clear, Shubik stated, to which extent Riker's resting upon previous literature (like the work of Raiffa and the work of Vickrey about coalitions) suited political theory.

Focusing on the detailed comments on manuscripts, the only substantial analysis advanced by Shubik is about what seems to him too much emphasis on the zero-sum model. To his eyes, much of Riker's analysis would also hold in a non-zero-sum game, mainly because the most crucial feature of his game-theoretic analysis is the indeterminate temporal length. In contrast, zero-sum is well defined only for a small finite part of the game. Instead, the main feature is the opposition of interest, which is a prerogative of a zero-sum (and constant sum) game and in a "more or less strictly competitive non-constant sum game." (Shubik 1961, p. 2) Other comments are less substantial, although still interesting, like contesting that "Side Payments" has an underlying reference to money, as stated by Riker (although Shubik admitted that this is a common misconception of Game Theory). Still related to this is the remark that "to include threats as a form of side payments leads to a rather poor modeling of human affairs." Because "the result of a threat may be a rather low personal payoff to an individual. However, the threat itself is more properly a part of a strategy". (Shubik 1961, p. 4)

Shubik recommended Riker's manuscript for publishing, even with only editing corrections. Interestingly, he also advanced some forecasts about this work's reception: "The odds are that it will receive very mixed reviews, including several very favorable and several highly unfavorable comments.

[15] Riker never knew that his referee was Shubik, although he imagined it. See Riker and K. Shepsle 1979, p. 14

It is certainly not going to make any "best seller' list. However, it should make a worthwile (sic) contribution to the development of political science." (Shubik 1961, p. 4)

As stated above, Shubik's name was not attached to the referee report. Besides, there is no sign of correspondence between Shubik and Riker before and even after the refereeing process. Moreover, given the vagueness of the comments and the fact that they were about to the manuscript version, unaivalable in the archives, of Riker's book, it is not easy to detect if they were effectively accepted by Riker.[16] Notwithstanding, this report is interesting for many reasons. From the historian's point of view, it represents a convincing appraisal of Riker's work, made by one of the leading game theorists of his time. Shubik was perhaps the best choice to evaluate Riker's work. As he repeatedly stated, his leading talent rested more on being an economic model builder than a mathematical economist.(Shubik 1992) Then, maybe, some other author could have focused his attention on the poor mathematical nature of Riker's work rather than, as Shubik did, on its general structure. As a matter of example, another reviewer could have pointed out the fact that coalition theory was being tentatively employed to obtain existence as well as stability results in General Equilibrium Theory.[17] But probably, such criticism would have totally missed the point. Riker aimed not to offer a sophisticated mathematical theory of political coalitions or to elucidate their mathematical features. Instead, he wanted to provide new insights into political processes by modeling rational behavior following the methodological considerations explained in the first chapter of his work (see below). Although distant from Riker's concerns (from a disciplinary point of view), Shubik understood this perfectly, as his referee report demonstrates. An issue which, instead, apparently was not caught by Morgenstern.

Thus, whereas Princeton, the place where Game Theory was initially created and where there was a small but enthusiastic community of game theorists, rejected Riker's manuscript, Yale instead accepted it, and *The Theory of Political Coalitions* was finally published in 1962.

3 Riker's *The Theory of Political Coalitions* (1962)

Riker's *The Theory of Political Coalitions* represented the most significant accomplishment of the author's intense commitment to game theory and formal analysis. He published other such general works only in the 1980s, although his focus shifted from game theory to social choice analysis, political theory, and American History (see, for example, Riker 1982.) This effort was an extraordinarily ambitious enterprise that aimed to construct, using an "existing general theory of coalitions (the theory of n-person games)," a theory of coalitions useful in studying politics and that rested on exact and verifiable assumptions. (Riker 1962b, p. vii)

Riker was not a mathematician, and his work is not ("most emphatically not." ibidem) a book about mathematics. This is apparent to the reader acquainted with game theory by his discussion concerning some extremely formal points. Such weaknesses notwithstanding, his attempt deserves much

[16] Except for that concerning the inclusion of threat as side-payment. In the published version of his work, "the threat of reprisal" is still maintained among the various kinds of side payments in politics. (Riker 1962b, pp. 109–10)

[17] A research program in which even Shubik himself was involved in these developments. Cogliano 2019

praise. His main thesis is that political actors will create coalitions just as large as they believe will ensure winning and no larger. This is the notion of "minimum winning coalitions," namely that winning coalitions will be constrained in their size.[18] This idea still occupies a central place in the formal study of political behavior and party formations, although the specificities of Riker's inquiry were disputed on theoretical and empirical grounds. (Butterworth 1971; Riker 1971; K. A. Shepsle 1974; McKelvey and Smith 1975) In the second half of his book, Riker slightly modified the n-person analysis of von Neumann and Morgenstern into a set partition of the voting members to describe the dynamics of coalition formations, that is, the strategy at the steps before a winning coalition is established. His theoretical ambitions in this part are even less fulfilled than in the first part, especially because the point to be addressed was far beyond the author's technical capabilities. However, one could find some useful insights on the effective working of political systems even there.

The first chapter of the book, which the author later defined to Shepsle as "the most important part of the book" (Riker and K. Shepsle 1979, p. 15), was devoted to presenting the assumptions of his model, as well a general analysis of the role of modeling in political science. In these pages, Riker defended a positivistic attitude concerning social science through a mixture of (at least) three arguments: the necessity for the social scientists of focusing only on phenomena whose size was easy to circumscribe; of providing an adequate notion of causal determinism; and finally of testing theories against facts in the real worlds, to enhance their predictive and explanatory power.[19] The theory of games fits all three, since, in Riker's view, it refers to well-defined events with clear relations among all the variables and entails both explanation and prediction.

In the same pages, Riker also discussed the two fundamental assumptions of his model, namely, individual rationality and the zero-sum property. The latter refers to those situations that entail pure conflict. Von Neumann showed that for the case of 2-players only, the solution was determined by the "minimax theorem" (a result corresponding to the Nash Equilibrium for the same games).[20]Zero-sum also enters in the determination of the n-players coalition game since the value of each coalition, its "characteristic function," is determined through a 2PZSG between each coalition and its opposite. However, not all political situations are of pure opposition. For instance, the most crucial feature of politics, as stated by James Buchanan and Gordon Tullock, requires that people are compelled to accept the decisions of the majorities, even if they are hostile to them. Therefore, it involves the need to balance between them.[21] Nevertheless, Riker defended this assumption

[18]The idea of the minimal winning size was not conceived originally by Riker but was instead developed by Shapley and Shubik. However, Riker employed this notion in a way different from theirs. He did not compute each player's value to play a given game (in a nutshell, Shapley's original idea) or the power that each member of an assembly entailed. Instead, the purpose of this notion was to constrain the coalition structure of a game, explore its equilibrium and stability, and offer a way to test these results.

[19]Riker had elaborated upon the first two elements in the philosophical papers written in the late 1950s, where he, in his words, "squabbled over methodology." (Riker 1957;Riker 1958a)

[20]Note that in this situation, the difference between cooperative and non-cooperative games, vanishes, since it is not possible, according to the zero-sum condition, to form a coalition. Instead, in the case of 2-person games without opposition of interests, the solution is the so-called "Nash's bargaining solution." Nash 2002b

[21]James Buchanan & Gordon Tullock's The Calculus of Consent was published in

by noting that the zero-sum condition is present in many political situations, for instance, elections and voting, where the loser cannot receive what the winner gains. Besides, he also observed that even non-zero-sum situations could be perceived by the people involved as pure conflict. Then, he did not discard this assumption.

The second fundamental assumption of the model was that of individual rationality. In particular, Riker criticized the "tautological" view adopted by economists, which purportedly does not describe behavior but only defines preferences.[22] To him, if any idea of rationality is helpful in modeling political behavior, one must adopt the "cruder and already somewhat discredited" idea of maximizing economic or political man. However, he rejected the idea of the maximization of power since it is difficult to define what power really meant (as seen in his empirical analysis of Shapley & Shubik's work. Riker 1959a). He assumed instead that political rationality encompasses the preference for winning over losing. So then, a rational political man does not act according to his chance of gaining more power by passing from a coalition or a party to another (as presumed in the 1959 paper) but instead according to his chance of becoming a member of a winning coalition. This embodies the notion of political behavior that rules the nature of political coalitions. Indeed, in pursuing this aim, each member must face the fact that the value of the coalition is determined not by size but instead by its ability to remain winning, as long as it reduces its size to its minimum winning level.

The bulk of Riker's analysis is centered around the "size principle," namely "the fundamental principle concerning the size of coalitions," that goes as follows: *"In n-person zero-sum game, where side-payments are permitted, where players are rational, and where they have perfect information, only minimum winning coalitions occur.* (Riker 1962b, p. 32 Italics in the text) A descriptive statement (or "sociological law") is related to this principle: "*In social situations similar to n-person, zero-sum games with side-payments, participants create coalitions just as large as they believe will ensure winning and no larger* (Riker 1962b, pp. 32–3)

As is apparent from the definition above, this principle requires rationality, side payments, and perfect information. All are standard assumptions in von Neumann and Morgenstern's analysis. The role of side payments is that each player decides to join a coalition over another based on what he is offered in them. Perfect information instead rules out the role of uncertainty.[23]In Riker's model, side payments have great importance in his discussion of the strategical aspects of coalition-building, viz., the dynamical part of his anal-

1962.(Buchanan and Tullock 1962) Riker had written a very favorable review in the same year, praising their theory. Furthermore, he eventually joined the Public Choice Society after it was established. (Riker 1962a)

[22]In particular, Riker criticized rationality as presented in Luce and Raiffa's work: to them, the theoretical issue to be solved does not entail demonstrating that real men want to maximize money, power, or something else. Instead, it is sufficient to represent the preferences' structure properly and, therefore, examine them empirically. Then Riker summed up their definition of rationality as follows: *Given a social situation in which exist two alternative courses of action leading to different outcomes and assuming that participants can order these outcomes on a subjective scale of preference, each participant will choose the alternative leading to the more preferred outcome"*(italics in the text.Riker 1962b, pp. 18–9)

[23]Both, in game-theoretical analysis, have a technical meaning. In particular, side payments are related to the notion of "transferable utility" among players. The first refers to the existence of a common medium that the players can exchange. If this is linear with the increment of the player's payoff, then it is said that utility is transferable.

ysis. Perfect information refers instead to each player knowing what moves the other players have made. This aspect is essential for what Riker defined as the "information effect," which, according to him, represented the proper way to appraise the effects of the size principle in real-world politics. (Riker 1962b, 77 et ss. See below)

As seen, Riker aimed to discuss political coalitions, namely coalitions involving winners and losers. Then, the framework of his analysis is cooperative and not competitive since it involves coalitions and, therefore, communication and agreements among players. Unfortunately, as he rightly noted, no unique solution exists for such games (contrary to the latter type, where a solution is a Nash Equilibrium). In his view, the cause is that too much emphasis has been put by game theorists on the properties of coalitions, like their existence, reasonableness, or fairness, overlooking instead the possibility of delimiting coalition structures directly. Since among the different solutions that followed von Neumann and Morgenstern's, none appeared to him significantly preferable to the latter, neither in terms of empirical testing nor generality, Riker based his analysis directly on the original solution concept developed by the creators of game theory. (Neumann and Morgenstern 2007)

The essential idea is that when the number of players is greater than 2 (naming N the set of all players), each coalition that forms in a game, from the coalition made up of a single-player only to the coalition of the whole set of players, has a value which is defined by its "Characteristic Function," $v(S)$ (where S is a set of players, $S \subseteq N$).[24]This value is the solution of a 2PZSG between this coalition and its opposite, that is, $v(S) = -v(S)$. Among the properties of each Characteristic Function (CF), the most intuitive is perhaps its additivity; that is, the value of each coalition is greater than the sum of each component. Each coalition splits its value between its members in the following way: none receives less than he would receive by forming a coalition alone, and only the value of the coalition can be split, no more and no less. The vector of the values for each coalition member is called "imputation." An imputation dominates another if all its components are greater than the other. Then, the solution of a n-person game is the set V of all imputations that do not dominate each other (inner stability) and dominate all the imputations outside the set (external stability).

Riker focused only on those coalitions that are winning, i.e., that are larger than some size, arbitrarily ruled (for instance, m, where $m \geq \frac{1}{2}$). In those cases, he tried to show that, other than the stability conditions entailed in the "stable set" solutions for n-person ZSG, a further principle was working, that is, the value of each winning coalition, $v(S)$ (its CF) reduced with the growth of its size. Put differently, a minimum winning coalition is worth more than a non-minimum winning coalition.[25]

Riker restated von Neumann and Morgenstern's original model, in a more straightforward and less mathematically sophisticated way, to describe in a statical way the coalitional structure of a political situation modeled as a n-person game. However, he also interpreted such a structure in terms of equilibrium and disequilibrium. Each $v(S)$ can assume a range of values,

[24]By coalition, in formal terms, it is intended the number of all the possible subsets of n, also comprising the empty set $\emptyset$ and the set of all players. This number equals 2^n.

[25]As seen, a minimum winning coalition is a coalition that is winning as long as it does not lose one of its members. For example, under simple majority rule, with two parties, a party with 51 members is an MWC. If the winning party has 70 members, it is a winning coalition, but not a minimum winning one.

which is a function of its size, and the size of the opponent blocking or losing coalition. Riker's definition of equilibrium and disequilibrium directly reflects each agent's strategy and rational decisions in the coalitions. In particular, he defined the notion of "equilibrium" in terms of each coalition's realizability or not. Then: "[i]f there are some values of $v(S)$ so unnecessarily disadvantageous for S as a whole that rational players reject S in favor of an immediately available alternative T, then these values of $v(S)$ will be said to be in *disequilibrium* and S will be said to be *unrealizable*. Conversely, those values of $v(S)$ which are not disadvantageous in comparison with an immediately available alternative will be said to be in *equilibrium*, and S will be said to be *realizable*." (Riker 1962b, p. 262 italics in the text)[26] He outlined three different types of winning coalitions:

1. Those whose value decreases monotonically with the growth of their size;

2. Those that gain by adding new members, at least until a certain point. After that point, the value decreases;

3. Those whose values are indifferent to their size[27]

Intuitively, the first states that the value of the coalition decreases with the growth of its size. The second case relates instead to a coalition that gains by adding new members, at least until a certain point. Finally, there are coalitions whose values are indifferent to their size. So then, the puzzle is the following: if having reached a majority a coalition deems its owns size disadvantageous and prefers to leave out some members, then this is not an equilibrium. Riker showed that, in the first type, the only equilibrium, that is, the only coalition that is realizable is represented by a minimum winning size. In the same way, in the second case, coalitions are realizable only until the peaked point is reached. Finally, the only equilibrium size in the third case is m, the arbitrarily chosen size. (Riker 1962b, 245 et ss.)

Riker was looking to develop a theory that could be used to make predictions and to be tested against the reality of political facts through the formal structure of his assumptions. As he wrote: "whether or not the just-stated conclusion is of any scientific value depends on whether or not an analogous statement about real-world can be verified." (Riker 1962b, p. 47). The "analogous statement" is the "size principle, which states that in social situations similar to n-person ZSG, only coalitions no larger than the minimum size occur.

However, to assess the principle empirically, one has to face the problem of "perfect information." Assuming that each member of the coalition can estimate its size subjectively, it could represent a possible getaway inside the model. But it would be clearly insufficient to fulfill Riker's positivistic approach. He recognized the possibility of arranging laboratory experiments

[26] Unfortunately, Riker was not sufficiently precise about what the criterion of realizability refers to. For instance, it could be obvious to interpret it as a sort of "domination" criterion for each member of the coalition (recall that 'domination' can be represented as if, $\vec{x} > \vec{y}$, for all $x_1, ..., x_n$, then $\vec{x}$ dominates $\vec{y}$.) However, this analysis is not outlined in the appropriate formal terms.

[27] Again, Riker's analysis is quite involute. He adopted a notion in von Neumann and Morgenstern's TGEB: the range of admissible values for each CF. Then, among these values, in the space of 'winning coalitions' CF, he identified three types: those with a negative slope (type 1), those with a positive slope in part (type 2), and finally, those with zero slopes.

to simulate and test different types of games, but also that, lacking a specific methodology and rules, the results would be partial. Therefore, he looked for such empirical assessment in American History. In particular, if one could show that political leaders made strenuous efforts to reduce their oversized coalition in the direction of a minimum winning one, he could verify the size principle. (Riker 1962b, p. 54) He picked up three examples from American History. Often an oversized coalition (for instance, a coalition of the whole that, since the assumptions regarding CF, is valued nil) was reduced by its leader through the expulsion of some of his members. This happened in the case of Andrew Jackson's Democratic Party, which emerged from the fragmentation Democratic-Republican Party[28], the only party that remained in the US after the disappearance of the Federalist Party as a consequence of the War of 1812.[29] These pages make apparent that Riker was adopting an explicit "instrumentalist" position: "I do not suggest, of course, that these nineteenth-century statesmen appreciated this principle as a law of rational behavior. What I do insist, however, is that it describes their behavior, even though they probably perceived their problems thus:' With our overwhelming majority, there are so many and so conflicting interests in the party that none can be satisfied. As long as two conflicting interests remain in the party, neither can be satisfied [which, I add, is why a grand coalition is valueless]. For the sake of action for the interest we approve, we shall therefore decide to satisfy one interest, and if others are offended, they may leave the coalition."[...] (Riker 1962b, pp. 65–6)

In a similar spirit, he integrated the role of information in his model by outlining a relationship between the level of information of each player in a coalition and the size of the winning coalitions. Namely, the greater the degree of incompleteness of information, the larger the size of coalitions. This rule, which Riker named "the information effect," could explain why MWCs are often not found in the real world. But also why some elections in US history have been "critical." The American political scientist VO Key developed the notion of "critical election" to define those elections where the voters' involvement is high and new electoral groupings are created. (Riker 1962b, 90 et ss.) But, Riker stated, one could interpret this notion as a period where the amount of information in the system declined and the uncertainty grew about the size of the winning coalition.

Riker also elaborated upon some statements from Anthony Downs' economic analysis of democracy. According to the latter, in a 2-parties model, where voters want to maximize their utility and parties want to maximize their share of votes, it could be convenient for both parties to be as ambiguous as possible. However, this would contradict the assumptions about voters' utility maximization because this makes it more difficult to vote rationally for each citizen, thereby encouraging voters to make their decision on some different basis from issues (viz, the candidates' personality). This paradox represented a blow to any attempt to use rationality to define voters' and parties' behavior. However, Riker was convinced that his model offered a getaway from this situation. If parties seek to maximize the share of votes only up to the size needed to become a minimum winning coalition,

[28]Which he labeled only "Republican Party"

[29]The other two examples from American History are: the rise of the Republican Party as a consequence of the destruction of the Whig Party in the 1850s and the fragmentation of the Democratic Party in different Blocking Coalitions; the end of the Reconstruction, when the Republican Party, again a grand coalition' de-facto divided in different coalitions, at state and local levels. (Riker 1962b, pp. 59–65)

therefore, it is not any more convenient for them to becloud their positions in any situation, but only about those issues that are of concern to voters about whom they have imperfect information. So the principle that parties and voters are rational (i.e., utility maximizers) is preserved.

The "size principle" represented an ideal standard to which any reasonable attempt to form a coalition should conform. In this sense, although resting on von Neumann and Morgenstern's solution for n-persons games, it was not a solution for such games but a "sociological principle." In the second part of his work, Riker investigated how political leaders set forth coalition-building and reached a stable arrangement if any. This analysis aimed to fully exploit what the author later recalled as the main feature of game theory, namely the choice of strategies. (Riker 1992)

In a nutshell, Riker discussed how coalitions are formed, maintain their structure, or add new members across a multi-stage game whose last stage represents the outcome of the process. For example, in voting, where a weighted majority is required, different coalitions are present (or "proto-coalitions" in Riker's terminology). Then, the leader of each proto-coalition tries to add new members, if necessary, by offering side payments. If, in the final stage, the coalition structure is such that a minimum winning coalition occurs, then this represents an equilibrium outcome.

Riker outlined his model as follows: assume a decision-making body, I, made up of n-members (i.e., a n-PZSG with side payments). In this body, there are different roles, but each member can assume any role. Each member's power (i.e., the weight) is assumed to vary. The decision rule is that a coalition with weight m, where m is greater than half the sum of the weight of each player, can act as a whole. The ZSC imposes a limit; no decision can be taken, so losers would prefer to resign from the body rather than acquiesce. In this model, coalition building begins when a leader that is a member of the decision-making body undertakes the task of forming a coalition on a particular issue. To this aim, the leader needs to attract followers among the other participants of the decision-making body. Given the focus on the dynamic process, Riker distinguished between coalitions and "proto-coalitions." In brief, the first are end products of coalition-building and can be "winning," "losing," or "blocking." Followers join instead in a "proto-coalition," a subset of I when this has at least three subsets, and none has weight m. These proto-coalitions change their size due to moves made by each member of I, and each move has the effect of changing the body's internal structure. Thus, in the first stage of such a game, there are n-single member coalitions. In the second stage, there are $n-1$-single-member proto-coalitions, one 2-members proto-coalition, and so on, up to the last stage, where either a winning coalition or different blocking coalitions exist.

Since any attempts to build a coalition generate opposition, the effect of the leader's first step toward building a proto-coalition is that others follow him and try to build their coalitions. The growth of proto-coalitions depends on the leaders' ability to attract followers by offering side payments. (Riker 1962b, 122 et ss.) These side payments can vary, but Riker listed some examples. These are payments in promises on particular policies, or subsequent decisions, up to the threat of reprisal. Besides, these side payments also have costs, which the coalition leader pays, and must consider. (Riker 1962b, pp. 109–20) Most importantly, Riker assumed that side payments were scarce and finite, subject to considerations regarding their economic value.

The study of dynamic coalition building is important because it involves strategic considerations about the behavior of political actors and the equilibrium outcomes and, therefore, their inner stability. To discuss these features, Riker introduced a notion "in some respect stronger, and in some weaker" than von Neumann and Morgenstern's set-valued solution because the latter did not specify if some coalition in the V-set was winning. He proposed the notion of a "uniquely preferable winning coalition," which involves the specification of a determined winning coalition. A uniquely preferable winning coalition is a coalition that has a greater value than any other one possible and in which all the participants can satisfy their initial expectations. An "initial expectation" for a proto-coalition equals the best it can do in joining alternative non-minimal winning coalitions.

From these considerations, any proto-coalition has some advantages (and disadvantages) in different stages of the game. An equilibrium solution is that when a "uniquely preferred winning coalition" occurs, none of the other proto-coalitions can neither join it or form a new winning coalition. Since this entails the "Size Principle," then an equilibrium corresponds to the presence of a Minimum winning coalition.

However, the most crucial problem with this kind of analysis is that the equilibrium in coalition-building cannot be maintained (i.e., it is not stable), but it seems to depend on the size and the relative strength of the minimum winning coalition. (Riker 1962b, 147 et ss.) The effects of this lacking of equilibrium for political analysis could be very serious: "equilibrium in society is a kind of stability despite the change. And to say that this model lacks equilibrium is to say that the social processes it purports to describe are so unstable- that the political society itself is in fact unstable." (Riker 1962b, pp. 147–8) Therefore, the last three chapters of TPC contained a purely verbal discussion about the components of this disequilibrium and its consequences.

Once more, Riker rested on History to assess this theory. In particular, the event he tried to explain was the so-called "corrupt bargain of 1825," when Andrew Jackson won a majority in the electoral college but lost the vote at the House in favor of John Quincy Adams, who became the sixth President of the United States. (Riker 1962b, pp. 149–59)

Suppose this event is analyzed in terms of the dynamics of coalition building. In that case, one can assume that the four presidential candidates were leaders of four different "proto-coalition": Jackson, Adams, William H. Crawford, and Henry Clay (who entered the election, but since there was a maximum of three candidates for a vote in the House, he was forced to transfer his votes.) The values of the proto-coalitions were the following: $w(P) = 11$ (Jackson), $w(Q) = 7$ (Adams), $w(R) = 3$ (Crawford) and finally $w(S) = 3$ (Clay)[30]. Since Clay, Adams, and Crawford were hostile to Jackson, the latter's proto-coalition was "strategically weak," whereas they Q, R, S formed a "uniquely preferred winning coalition." Jackson's coalition shrinks, whereas Adams' now became the preferred one. The new values were: Adams with 9 states, Jackson with 7, Crawford and Clay with 4 each. To obtain a majority, Adams could ally with Crawford or Clay, but not with both, to maintain an MWC. Eventually, an alliance between Clay and Adams occurred and was the dominant strategy from the latter's point of

[30]The value of each coalition corresponds to the number of states where each candidate had the majority. In the House, each state had one vote, which was decided by a plurality of its representatives

view. Indeed, if Clay allied with Jackson (despite their ideological differences), Crawford would have allied with Adams, forming an MWC. If Clay joined Crawford, now they had the same values as Jackson. Jackson and Clay-Crawford would have won the election, but the total value would have been split three ways over two. Given these strategic possibilities, Clay joined Adams, a majority was reached, and Adams was elected as the sixth president of the United States. Clay became his Secretary of State.

Then, the historical argument again serves Riker to show the explaining power of his model and provide the basis for a new understanding of historical and contemporary political facts. Again, furthermore, Riker stressed the fact that his example did not show that Adams, Clay, or Jackson were employing game theory or rational choice, but that their behavior can be explained *as if* they did.

4 Political Science and economic modeling: Riker's unsolvable dilemma?

Riker's work was reviewed in the *American Political Science Review* and other social sciences journals (Fagen 1963; Matthews 1963; Hotz 1963; Kaplan 1963; Flanigan 1965). All the reviewers highlighted Riker's methodological originality and the importance of his non-trivial generalizations about politics (although with some reservations about the notion of rationality). Yet, none stressed its formal features. Indeed, none of the reviewers was a real expert in game theory. As to economics, the book went completely unnoticed in economic journals.

As Riker lamented in Shepsle's interview, not many people had a sense of his work's theoretical features. (Riker and K. Shepsle 1979, p. 24) Political scientists indeed focused only on some aspects of his model, especially the last three verbal chapters. Furthermore, as it is apparent from many reviews, Riker's idea came to be framed and discussed mainly in international politics. Riker's last chapter, where he tried to detect some considerations about the developing rivalry between U.S.A. and U.S.S.R., caught the readers' attention more than the formal characteristics of the model. Besides, Riker's adoption of von Neumann and Morgenstern's solution went mainly unnoticed by game theorists. Consequently, the importance of Riker's work on political coalitions rests upon its being the first attempt to elaborate a model of political behavior explicitly based on a game-theoretical approach, even if this was far from reaching the levels of contemporary formal analyses in economics. Although Riker's work did not offer anything new to the development of game theory, it still represented, in the light of what formal political theory became from the 1960s onwards, the first step of one of the most successful translations of theoretical ideas from one domain to another.

A further episode can make apparent how economists and political scientists remained disjointed communities in those years. In a review of a comprehensive collection of essays about the different approaches to the study of politics, Riker complained about the lack of an economic approach (Riker 1959b). We believe that this complaint can be paralleled with the publication of a strictly formalized volume about the use of mathematics in the social sciences, with several highly mathematical essays about economics, management, and psychology, but no reference to political science (Kenneth J. Arrow, Samuel, and Suppes 1959). Unsurprisingly this volume went un-

noticed in political science journals. Indeed, only one reviewer (the future Nobelist Reinhard Selten) noticed, in *Econometrica*, the absence of an essay on political science. (Selten 1962)

His theoretical shortcomings notwithstanding, *The Theory of Political Coalitions*, alongside other works, like that of Buchanan & Tullock (Buchanan and Tullock 1962), Anthony Downs (Downs 1957), or Duncan Black (Black 1958) has marked the birth of a formal political theory. (Ordeshook 1986; Persson and Tabellini 2002) Besides, among these authors, Riker was the only one who used game theory extensively and, overall, to be a political scientist with a definite theoretical agenda.

In the remnant part of this section, we discuss Riker's use of economic theory concerning his agenda of transforming political theory and the development of postwar mathematical economics. In particular, Riker showed a sheer interest in some methodological issues, which can be confirmed by his two philosophical papers, the first chapter of his work on political coalitions, and other works we will briefly address. (Riker 1957; Riker 1958a; Riker 1962b; Riker 1977) In the meanwhile, however, he did not adopt deep methodological arguments. So, the main feature of his philosophy of science is represented by the robust faith in the idea of science as a positive discipline. In order to be a "genuine science," a theory must display those elements that make it possible to explain the phenomena and predict their outcomes, eventually by discovering laws that relate different pieces of reality under a single theory. Riker's approach is "nomological-deductive." However, he paralleled such a view with a position entailing a more instrumentalist approach. Here lies what can be defined as "Riker's dilemma." He advocated the development of positive political science, resting heavily on formal assumptions, but the tools and the kind of analyses he was trying to adopt were far from being able to fulfill his aspirations.

Following E. Roy Weintraub's pivotal studies, historians of economics have interpreted the development of economics as a mathematical discipline in relation to the parallel development of mathematics as a formalist program. In this sense, postwar mathematical economics was not limited to employing math but mirrored the changes that occurred in this discipline, that is, "the changed standards for accepting proofs, changed ideas about rigor, and changed ideas about the nature of the mathematical enterprise." (Weintraub 2002, p. 2) Similarly, Giocoli summed up the radical transformations in economics between the 1930s and the 1950s as two distinct visions of economics. (Giocoli 2003b) The first is the idea of economics as a "system of forces," which entails the idea that its main subject is the analysis of the processes generated by market and non-market forces, including - but not exclusively - the processes leading the system to an equilibrium. Economists paralleled this idea and replaced it with economics as a "system of relations. According to the latter, "economics is a discipline whose main subject is the investigation of the existence and properties of economic equilibria in terms of the validation and mutual consistency of given formal conditions, but that has little if anything to say about the meaningfulness of these equilibria for the analysis of real economic systems." (Giocoli 2009, p. 24)

Such a transformation was far from being accepted without resistance among economists and gave new fuel to the everlasting debates concerning the philosophy and methodology of economics.In particular, the spirit of new axiomatic economics challenged the most traditional views of the scientific enterprise as an explanation and, to a certain extent, a prediction of

phenomena. For instance, it is pretty obvious that a mathematical model such as the "General Economic Equilibrium" model lacks both explanatory and predictive power. Indeed, it is explanatory *only* in the sense that a mathematical theorem explains something, and it is predictive in the same way.Then, as Pierluigi Barrotta rightly put it, Gerard Debreu's statement, at the very beginning of his *The Theory of Value*, according to which his theory explains how the prices are determined from agents' private ownership through markets and their role in an optimal state of an economy, could be seen as meaningless if one assumes that to explain something one has to find general laws relating different phenomena and their appearance.(Barrotta and Raffaelli 1998, p. 46; Debreu 1959) This does not mean that economists are no longer trying to explain phenomena or advance their predictions about possible outcomes, but new languages and practices have been developed to fulfill this aim.

For the case of economics, Milton Friedman presented a strictly positivistic view contending, among the others, the development of the discipline as pure mathematical modeling. Friedman's view is famous for advocating a radical instrumentalist approach and his seemingly unique belief that the realism of assumptions of theories does not matter.[31] Still recognizing the extremely important place of "purely formal or tautological analyses" like those blazing the trail to the Postwar mathematical economics, given the extreme difficulty of testing substantive economic theories, Friedman, however, held that "economic theory must be more than a structure of tautologies if it is to be able to predict and not merely describe the consequences of action; if it is to be something different from disguised mathematics." (Friedman 1954, pp. 11–2) Since the most important feature of a theory is to predict something, mathematical modeling is not sufficient, and realism is not essential at all.

That Riker was seemingly close to such an idea of economics is apparent by how he defined the discipline in his work on political coalitions. To his readers, indeed, most likely political scientists, he defined economics as a "coherent theory and verified generalizations," the product of "150 years of empirical investigation and refinement of theory." (Riker 1962b, p. 6)Furthermore, even his treatment of rationality and the attempts to assess his model empirically show an ostensibly "instrumentalist" influence. (Riker 1962b)[32]

Starting with rationality, it is well-known that this notion went in the Postwar years under a dramatic transformation in a mathematical sense. Game theorist and philosopher Ken Binmore defined this as the "consistency view" of rationality. (Binmore 2015) In a nutshell, the core of this idea is that rationality does not describe behavior but only defines preferences in a purely tautological way, and the properties of the utility functions assume great importance in modeling individual rational behavior. However, Riker criticized this view, as represented by Duncan Luce & Howard Raiffa in their pivotal textbook *Games and Decisions*. (Luce and Raiffa 1957)[33]

[31] "Instrumentalists claim that theories are best viewed as *nothing more* than instruments." (Caldwell 1994, p. 178) In this sense, such an approach is contrasted with realism, whereas the latter claims that theories should make real references. Note that Friedman did not explicitly characterize his position as "instrumentalist." Lawrence Boland did it.

[32] Riker did not explicitly refer to Friedman in his work on political coalitions. But he did in the textbook he wrote about the "Positive Political Theory that he wrote with Peter C. Ordeshook (Riker and Ordeshook 1973, p. 30), where this position is made explicit.

[33] Note, however, that Luce and Raiffa's work was by far less mathematical than the

In his eyes, if rationality is to help model political behavior, a simple mathematical tautology is not sufficient. On the contrary, as seen, one must adopt the more meaningful idea of a maximizing economic or political man. As a consequence, the problem of rational action and empirical validation should become: "how can the rationality condition be stated in such a way that it is more than a tautology but not subject to the criticisms implied in those experiments which show that the scale of individual utility is not the same as a scale of money." (Riker 1962b, p. 20) And the proper definition of rationality simply is: *"Given social situations within certain kinds of decision-making institutions (of which parlor games, the market, elections, and warfare are notable examples) and in which exist two alternative courses of action with differing outcomes in money or power or success, some participants will choose the alternative leading to the larger payoff. Such choice is rational behavior and it will be accepted as definitive while the behavior of participants who do not so choose will not necessarily be so accepted.*" (Riker 1962b, p. 23. Italics in the text). Furthermore, Riker also stated that not all behavior needs to be rational, but rational behavior is crucial for managing any economic or political institution. Thus, like the markets are controlled by those economic agents who adopt a maximizing behavior, the same holds for the working of political institutions that "select and reward with success behavior which is apparently motivated by the intention to maximize power." (Riker 1962b, p. 21)

One can find even more strong references to a sort of instrumentalist position in the pages where Riker attempted to assess his "size principle" historically. (Riker 1962b, pp. 65–6, 149–59. Then, as seen above, what matters to him is not that politicians closely employ game theory but that the model offers an accurate explanation of their behavior, thereby making possible a fuller understanding of it.

Further details, although not developed systematically, concerning the nature and the scope he attributed to social sciences can be found in a review essay significantly titled *The Future of a Science of Politics*. (Riker 1977) Riker again rejected the particularistic study of political events, proper of the more traditional approaches of political studies, emphasizing instead a "positivistic view of science" whose central tenets are sets of scientific laws, i.e., "well-verified generalizations." Besides, by scientific laws, Riker defined not only those discovered by observation but also theorems derived from axioms. He seemingly attributed the same scientific status and explanatory power to both: "Law and axioms thus reinforce each other. The necessity of the inference makes the law seem reasonable, and the empirical validity of the law makes the axioms seem true. Thus, with a theory there is a much stronger reason than mere observation to accept a scientific law." (Riker 1977, p. 15)

Riker's model for political theory is now explicitly Price Theory, that "satisfies, in structure and outcome, [his] notion of what a science is just as well as, perhaps, physics." (p. 22) This theory, he argues, states that price is determined by equalizing supply and demand in a competitive market. The law of demand can be derived by an empirical validation of how the quantity demanded varies when the prices rise or fall. This empirical validation can be extended to axiomatic theory, developing the theory of consumer choice.

vast part of contributions to Rational Choice Theory. Furthermore, still presenting the axiomatic idea of rationality, following especially von Neumann & Morgenstern's treatment of "expected utility," they were also concerned with the attempts to assess it empirically.

Despite the supply side of the price theory being less satisfying and intuitive from the empirical point of view than the demand side, economists generalized both demand and supply into a theory of competitive equilibrium. In Riker's words, Price Theory: "contains all the elements in our previous description of a science. It starts with an empirical law, which is presumably universal when properly restricted. This law is then imbedded [sic] in a theory of choice. In turn, this initial theory is elegantly elaborated to produce a nonobvious and far from trivial inference about market clearing, which is in turn strongly supported by empirical evidence." (Riker 1977, pp. 21–2)

If in this work, Riker's position shows a "nomological-deductive" approach, assuming the importance, for the science, of discovering general laws in order to explain phenomena, in another paper, he returned to the issue of predictability. (Riker 1980) There, he explicitly stated that economics is a theory that admits predictions of an equilibrium. Furthermore, its prestige among other social sciences is due to the "actual occurrence of numerous predicted equilibria." (Riker 1980, p. 434) Note that Riker generally refers to economic theory and not to a part of it (like, say, game theory).

However, his close identification between equilibrium and predictions raises several problems for political theory. Riker linked the main advantage of equilibrium analysis to its ruling out interpretive and psychological motives behind people's actions. As he wrote about one of the most important results of the theory of voting: "ideological extremists who vote eagerly for the party closest to themselves certainly intend to bring about extreme results; yet one social consequence, as McKelvey (1975) has elegantly shown, is that they drive parties to converge at the ideological center. No amount of sympathetic interpretation of individuals' motives and actions can explain this unanticipated social result. [...] Even when individuals accomplish exactly what they intend, social outcomes are not explained by interpreting motives." (Riker 1977, p. 28) The failure to produce general results concerning the theory of voting (the most significant example in economics of such a general result being the General Economic Equilibrium), and, consequently, the flowering of many "impossibility results" (see especially those discovered by Richard McKelvey and Norman Schofield) scattered Riker's faith in his positivistic idea of science linking equilibrium, individual rationality and predictions so that he famously argued that not economics but "politics is the *dismal* science because we have learned from it that there are no fundamental equilibria to predict. In the absence of such equilibria, we cannot know much about the future at all, whether it is likely to be palatable or unpalatable, and in that sense our future is subject to the tricks and accidents of the way in which questions are posed and alternatives are offered and eliminated." (Riker 1980, p. 443)

Therefore Riker, although not rejecting his positivistic program *in toto*, spent the remnant part of his career (he died in 1993) investigating further the institutional arrangements surrounding the persistence of disequilibrium and the occurrence of many, very fragile, temporary equilibria in choice settings.

Not surprisingly, Riker's strong arguments concerning the *dismal* character of political science called for a reply in the community of formal political scientists. Peter Ordeshook did one, and his response makes apparent the gap between Riker's view and that of other advocates of formal modeling more in touch with economic analysis.[34]Ordeshook claimed that Riker at-

[34]Ordeshook was among the first students to enroll in the Ph.D. Program in Rochester,

tributed too much significance to the idea of equilibrium as stability and, therefore, to the idea that price theory itself referred to market stability. As it turned out, "the presumed stability of markets is an abstract fiction that most economists recognize as a *a theoretical* impossibility." (Ordeshook 1980, p. 447 Italics in the text). It was something existing in the formal model but not necessarily in reality. The same holds for the theory's predictive power: abstract descriptions of markets might predict how a change in the general settings of the model affected prices and consumption but could not anticipate these changes. For instance, in a market with one seller and two buyers, the value of the coalition of buyers is zero. The core (i.e., the set of all undominated imputations) comprises only those imputations that attribute value to the seller and nothing to the buyers. Namely, the single seller is a monopolist and will extract all the added value from any exchange. Nonetheless, it could be reasonable to assume that buyers could form a cartel and negotiate as a team, and a new equilibrium would arise. For example, this now becomes a bargaining problem, where the monopolist tries to obtain a distribution as advantageous as possible on the Pareto frontier. However, the core now consists of all the points in the Pareto frontier, and therefore, the existence of equilibria is not a sufficient condition for predicting outcomes. (Ordeshook 1980, p. 448)

Ordeshook's central point is thus that equilibria are elements of the formal model and therefore display features like existence, uniqueness, and stability but do not necessarily entail predictive power. This argument does not exhaust the question of the impossibility of collective choices but instead points to new ways of analyzing the problem. In particular, he called for political scientists' genuine contribution to developing new formal models and not only to employ notions and ideas taken away from economics.

Looking at the differences between Riker's argument and Ordeshook's allows a final appraisal of how Riker used game theory and economic theory and how this differed from postwar neoclassical economics and other attempts to extend economic reasoning across domains different from economics. Returning to Giocoli's distinction between "economics as a system of forces" and "economics as a system of relations," Riker's analysis looks closer to the former vision. Indeed, to him, equilibrium is not an analytical framework within which formal analysis can be conducted (and neither the solution of a game) but instead a relationship of forces. Take, for instance, how, in his work on political coalitions, Riker defined this notion: "[T]he notion of equilibrium is that of a relationship of forces arranged so that the deviation from some point of balance results in a (possibly automatic) correction back to balance." (Riker 1962b, p. 147) Only in this way equilibrium can be related to prediction. Besides, this can also explain why he was so concerned with the issue of disequilibrium. Indeed, in a "system of forces" framework, disequilibrium and equilibrium have the same importance. Since it is clear that reality, especially social reality, hardly shows anything similar to "physical" equilibrium, disequilibrium sometimes has a stronger appeal to the researcher. On the contrary, in a purely axiomatic model and within the notion of equilibrium widely employed in game theory (that is, Nash Equilibrium), equilibrium is simply the necessary outcome of the model, i.e., the

established by Riker in the 1960s and focused on formal theory and game theory. Ordeshook had co-authored, with Riker, the first textbook devoted to Positive Political Theory, in 1973, besides having offered pivotal contributions to voting theory and political game theory. (Riker and Ordeshook 1973.)

solution of a problem modeled like a mathematical problem. Naturally, then, the main issue in such models is the existence or non-existence of equilibrium itself (and, possibly, its multiplicity).

The point is that in economics, game theory was conceived within the "system of relations" framework. This simple historical fact shows how the entry of game theory into political science differed substantially from the way it entered economics. It also makes it apparent how the task Riker tried to accomplish, especially in his analysis of political coalitions, was arduous to achieve: not only because Riker lacked the advanced game-theoretic skills that could have made his research more mathematically robust, but also because he considered game theory from a viewpoint that was too different from his actual development in mathematics and, later, in economics.

Finally, some hypotheses can be advanced for explaining Riker's resting on the old idea of "economics as a system of forces." Riker was an outsider among the community of game theorists and economists as well, so the most immediate hypothesis is that he simply lacked adequate knowledge of economics to fully assess its most recent developments. True as it could be, such an explanation is not sufficient. A suitable answer lies, in our view, in the fact that Riker was trying to make sense of the idea of political theory based on rational choice for political scientists, an audience much different from economists. In this sense, one could explain, for instance, his rejection of a preferences-based tautological argument regarding rationality. (Riker 1962b)

He preferred a more meaningful definition over a purely mathematical approach because the earlier's meaning could be easily grasped by an audience not comfortable with mathematical sophistication. But this raises, as seen, the problem of the wedge between what the theory can do and Riker's positivistic aspirations. Then, the refusal of the preference-based approach pushed him toward a significantly weaker argument to defend rational choice, namely that, even if not all agents are rational, the most important agents are (a summation argument). In doing so, the author seems to discard what the rationality assumption in economics is, viz., one way to constrain the beliefs and desires people are allowed to have in order to make their actions explainable. However, he did not offer a valid alternative to explain individuals' actions *and* properly model them. Indeed, restraining the set of individuals assumed to be rational does not preclude the fact that modeling rational behavior requires strong assumptions about people's beliefs, preferences, and formal structure.[35]

Compare Riker's idea with von Neumann's theory characterizing rational behavior with prudence. Indeed, in his 1928 paper and his 1944 work, rational behavior is represented by the "security payoff" given by the minimax strategy. Namely, a player does not need to know what his opponent's plan will be in reality, but if he plays the minimax strategy, he will maximize his expected payoff in the worst possible case. (Neumann 1928; Giocoli 2003a) This idea solves the paradox of perfect knowledge discussed in the 1930s by authors like Morgenstern and Friedrich von Hayek, albeit in different forms. (Morgenstern 1976; Hayek 1937. See also: Giocoli 2003b) Like von Neu-

[35]Besides, Riker implicitly adopted a preference-based idea of rationality when using the Vsolution of von Neumann and Morgenstern. Indeed the fundamental properties in the notion of "imputation" on which the two authors built upon their V-solution and Riker extensively discussed (although not in mathematical terms), encapsulated the notion of individual and collective rationality in preference terms. (Luce and Raiffa 1957, pp. 192–3)

mann, Riker focuses only on one feature of human behavior: the preference for winning outcomes. Avoiding a too narrow idea of rationality (i.e., that "individuals' scales of utility [...] are isomorphic with the scale of some objective measure such as money or even power"), he expected that "rationality as winning" could establish the foundation of a theory of political coalitions based on von Neumann and Morgenstern's formal analysis. If one interprets Riker's argument that way, it could be possible to understand why he chose it. Nevertheless, there is no contradiction between a formal argument and an idea of rationality that encapsulates concrete human behavior features. Von Neumann's solution was criticized for equalizing rationality and prudence. However, it was not this aspect that undermined its acceptance, but the mathematical difficulty in proving a similar result for those situations (non-cooperative and cooperative) different from 2PZSG (a problem that the newborn idea of Nash Equilibrium overcame).

To conclude, one could explain Riker as resting on an old idea of economic analysis as a "system of forces" that, attractive as it might be, was increasingly being displaced in the very area of economic analysis that he had selected as his theoretical reference (i.e., game theory) by new ideas concerning axiomatization through a view of economics as a 'system of relations.' An area, it should be added, that until very recently has always been the most distant from the possibility of empirical validation. This, in a nutshell, was Riker's unsolvable dilemma.

Conclusion

This paper showed how game theory entered Political Science and what was the role of William H. Riker.

I focused on how Riker's analysis differed from economics and formal game theory, primarily for what concerns the structure of the argument and the different scope pursued. I showed that Riker's relationship with economics and game theory was, in a certain sense, fuzzy. He referred to economics as a role model, but his idea of economic theory was distant from that of economists at his time. His treatment of rationality lacks the generality for establishing a full-breadth theory and, indeed, will be discarded when 'Positive Political Theory' aligns itself with economics. Besides, he understood what was missing in game theory but was an outcast in the community of game theorists.

Riker certainly had a limited influence on the development of game theory *qua* theory. Instead, emphasizing the consistency of his theory with the explanation of real-world (or even historical) political phenomena served better his task of showing how functional game theory could be in political analysis. To this aim, more than a mathematical sophistication that few political scientists were barely able to grasp, he showed how political behavior was inherently game-theoretical and, therefore how fertile game theory was for allowing "political science to rise above the level of wisdom literature and indeed to join economics and psychology in the creation of a genuine science of human behavior." (Riker 1962b, p. viii)

With the passing of the years, the difference between formal political science and economic theory narrowed, up to the point that important contributions were also published in economic journals like *Econometrica* or *The Journal of Economic Theory*. This process paralleled and was the effect of establishing a community of scholars entirely devoted to the formal analysis

of political issues. At the same time, it opened a new stream of methodological issues, namely the attempts to reconcile positive aspirations with the ambition to develop a genuinely mathematical analysis. (Austen-Smith 1999) In the end, this was Riker's most significant legacy. Such a story, which starts with Riker's creation of the Political Science Graduate program at Rochester University, is the necessary complement to this paper.

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