

# 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 at offering a historical reconstruction of the early cross-fertilization between economics and political science in the 1950s, focusing especially on the role of American political scientist William H. Riker (1920-1993). In particular, I want to show how the formal revolution in economics has influenced the developments of Rational Choice and Game Theory in Political Science. I will begin with a reconstruction of Riker's training as a political scientist, and his early works, by which also a very concise summary of the disciplinary state of Political Science, will be provided. Later I will focus my attention on some pivotal theoretical contributions regarding the economic approach to politics (e.g. Duncan Black's and Kenneth Arrow's) and some applications of Game Theory (e.g. Shapley's and Shubik's works). Finally, I will highlight Riker's crucial role in this process, through an analysis of his most ambitious and important theoretical work, *The Theory of Political Coalitions* (1962) and its relation with contemporary formal analysis in economics.

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

**JEL Classification:** B16, B21, B29, B31

## 1 Introduction

In the first chapter of his most ambitious theoretical work, *The Theory of Political Coalitions* (1962) American political scientist William H. Riker was adamant about his methodological concerns. To him “the main hope for a genuine science of politics lies in the discovery and use of an adequate model of political behavior.” (Riker, 1962, p. 9). Such a model was, in his view, eminently game theoretical. The game theory Riker adopted was the initial analysis pursued by John von Neumann and Oskar Morgenstern in their 1944 work, namely the study of  $n$ -person zero-sum games, involving coalitions formation and stable set solutions (von Neumann and Morgenstern, 1944). Despite some inaccuracies and some mathematical difficulties in his original model, Riker's work determined an entire class of scholarly researches, which modeled political decisions, domestic politics, and international relations, through an economic approach, where game theory occupies a central place. Then, Riker's main accomplishment was the establishment of the subfield of 'Positive Political Theory'. (Riker and Ordeshook, 1974)

This paper aims to present Riker's early commitment to formal political analysis by describing his early life and works, other than the development of formal political

models in the 1950s. This intellectual story is framed on the one side by the incipient mathematization of Postwar economics, quickened by the publication of von Neumann and Morgenstern's *Theory of Games and Economic Behaviour*, in 1944.<sup>1</sup> On the other by the pursuit of a similar 'going scientific' path in other social sciences, including Political Science. The 1950s coincided with the so-called 'Behavioral Revolution' in political science. A young generation of political scientists (most notably, people like Robert Dahl, Gabriel Almond, David Easton, David Truman, among others) rejected the traditional approaches to political studies (history and law) to establish a more empirical and quantitative analysis. (Somit and Tanenhaus, 1967; Adcock, Bevir and Stimson, 2007)

Riker indeed joined the 'protest' movement, but not the revolution, focusing on extending such a plea for 'objectivity' and political theory. In doing so, his path crossed that of other scholars who worked on related themes. Indeed, different attempts to apply formal modeling to address political issues were being carried forward, and the so-called 'formalist revolution' in economics also transformed other social sciences. For example, Kenneth Arrow's 'impossibility theorem' (1951) gave birth to the new sub-field of Social Choice, leading political scientists to rediscover the problems of voting paradoxes and majority cycles in a more rigorous setting.

Game Theory occupies a crucial role in this story. Its impact on microeconomics was somehow slowed down by some inherent weaknesses of early game-theoretic models (and the related development of other powerful, more ready-made mathematical tools). 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 von Neumann and Morgenstern was immediately perceived, by some scholars, as the ideal tool-box of methods, notions, and techniques. Then tools like the characteristic function and solution concepts like the 'Shapley Value' were applied in that period to the study of politics, showing the fertility of this approach while economists seemingly rejected it.

I will divide the paper as follows: the first section is devoted to Riker's biography until 1962. This section also presents the disciplinary status of American political science briefly in the 1940s and 1950s, which Riker radically criticized. The second section will discuss a brief treatment of formal political theories in the 1950s, namely Social Choice theory and early game theoretical results employed in political science. Riker's *The Theory of Political Coalitions* will be addressed in the third section. Finally, the last section will discuss some features of the formal modelling in economics and Riker's political science.

## 2 William H. Riker's life: from Harvard to Rochester

William H. 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. 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). Despite his bachelor's in economics, Riker did not attribute

<sup>1</sup> The History of the development of mathematical economics has been the subject of different works. Among the most important: Ingrao and Israel, 1987; Weintraub and Düppe, 2014 (General Economic Equilibrium); Morgan, 1990 (Econometrics); Weintraub, 1991 (Economic Dynamics); Weintraub, 2002 (Mathematical Economics); Giocoli, 2003 (Game Theory and Rationality) Leonard, 2010 (Creation of Game Theory); Moscati, 2017 (Utility Theories)

particular influence to his undergraduate studies in economics. Indeed, in his interview devoted to them only some scant references.<sup>2</sup>

Although he considered poorly scholarly research in political science during his undergraduate studies, he decided to apply for Graduate School. Riker recollected that his set of possible choices comprised Harvard University, Columbia, and Chicago. These were “[t]he three schools that were producing substantial numbers of political scientists at the time” (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. His political science professor at DePauw, Harold Zink, advised him to apply for Chicago. 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 both in the development of ‘Social Sciences Research Council’, in the late 1940s and 1950s, and in its ‘Committee on Political Behavior’. (Pendleton Herring, 1947; Dahl, 1961)

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

<sup>2</sup> 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 (Shepsle and Bueno de Mesquita, 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 (e.g. Riker, 1992). 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. (Interview to William Harrison Riker by Kenneth Shepsle, 1979, Box 85, William Harrison Riker papers, D.262, Rare Books, Special Collections, and Preservation, River Campus Libraries, University of Rochester) This 150 typed pages interview spanned from reminiscences on graduate and undergraduate education to theoretical and methodological issues.

approach to politics.” (p. 45) To Riker's eyes, its main representative at Harvard was the famous German scholar Carl J. Friedrich, about which he spent some harsh words in the interview with Shepsle. 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) In general, Graduate School experience did not satisfy him, which 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, people like Friedrich, who defended the case for a theoretical discipline, but at the expense, at least to Riker, of any practical purpose.

The 'behavioral revolution', which occurred in American political science from the late 1940s onward, 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 afore-mentioned Easton, whose work will be pivotal in the development of it. (Adcock, 2007) 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 et al., 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 to Shepsle, 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." (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 passable of scientific validation. Among them, 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'" (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)

He certainly shared such 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 by far less advanced by incipient econometrics), to his eyes, political theory was still grounded on flawed and unspecified ways of reasoning. This flawness 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 to Shepsle, pp. 50 et ss.) 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”. (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” (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.

The issue at stake, in his mind, 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 philosophy of science, especially 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, around the mid-fifties. (von Neumann and Morgenstern, 1944).

In a paper written for reconstructing the history of the entry of game theory in political science, Riker offered a slightly different account of how he became acquainted with Game Theory. (Riker, 1992) This started with him reading Lloyd Shapley and Martin Shubik's pivotal paper, in the *American Political Science Review* about the distribution of power in a committee system. (Shapley and Shubik, 1954) Together with this, he also read Kenneth Arrow's work on social choice (1951) and these two works led him back to von Neumann's and Morgenstern's outstanding book. “There I discovered what I thought that political science needed for constructing theory” (Riker, 1992, pp. 207-9)

Riker devoted the second half of the 1950s expanding his knowledge of game theory, especially cooperative game theory, and investigating further social choice theory. To these, he also added two philosophical papers, published in the *Journal of Philosophy* where he dealt with the issues of how to circumscribe the events to provide descriptive generalizations of political events. (Riker, 1957; Riker, 1958)

Riker spent the entire academic year 1960-1 as a fellow at the “Center for the Advanced Study in the Behavioural Science”, at the University of Stanford. The main outcome of that experience was his theoretical analysis of political coalitions, published in 1962. In the same year, Riker was hired by the University of Rochester, with very generous conditions for establishing a graduate program in Political Science. His move to Rochester marked the institutional birth of ‘Positive Political Theory’. But the roots were well embedded in the developments of formal theories in the 1950s and the debates surrounding them.

### 3 Modeling politics in the 1950s

In the 1950s, alongside the ‘going mathematical’ process in economics, some attempts to extend the same reasoning to other social sciences were carried forward. For political science, these include social choice theory and some applications of game theory. This section aims to frame these developments historically.

First, I have to address some terminological questions. What I label as ‘formal political theory’ has been defined, yet at the end of the 1950s, by Riker as ‘Positive Political Theory’ or ‘Formal, Positive Political Theory’ (Riker, 1962 p. 33). Being ‘formal’ was the main feature of this approach, but no precise definition of what ‘formal’ meant was explicitly discussed by people who contributed to it, like Duncan Black, James Buchanan and Gordon Tullock, or Riker himself, up to suggest to the reader that perhaps the real significance of ‘formal’ was ‘what economists do’.

Additionally, before ‘Positive Political Theory’ spread in American political science (and ‘Public Choice’ in American economics) some other definitions were adopted. Black spoke of a ‘Pure Science of Politics’ (Black, 1950; 1958). Tullock instead referred to a ‘Strict Theory of Politics’ (Buchanan and Tullock, 1962) To these can be added the ‘Genuine Science of Politics’ which Riker mentioned in his 1962 work. (Riker, 1962) All three seem to suggest that in the mind of their proponents, their analysis aimed to make political science really ‘scientific’. Consequently, economics, the social science where the formal modeling had peaked, appeared to offer the example to follow. But this relationship with economics is more problematic, especially given the clear-cut references to the ‘positive’ character of analysis made by Riker. I will discuss some aspects in the last paragraph.

#### 3.1 Social Choice and Voting

The 1950s opened with one of the most important and comprehensive contributions to the formal treatment of theoretical problems in social science, Kenneth Arrow’s *Social Choice and Individual Values* (1951) a revised edition of the Ph.D. thesis he defended in 1949. (Arrow, 1963; Arrow, 2014; Igersheim, 2017) Arrow’s short work opened a new subfield of economics, i.e. the modern rediscovery of Social Choice Theory. The core of Arrow’s main argument, the impossibility of collective choices that preserve all individuals’ ranking of preferences (Arrow’s impossibility theorem), was discovered in the middle ages concerning voting procedures. Still, Arrow opened a new way to address such issues. (McLean, 2015). His essay occupies a central place in the history of XXth theoretical social sciences because his formal approach to theoretical social problems paved the way for many similar analyses. (Sen, 2017)

Arrow’s work received some attention outside the economists’ community given its theoretical importance. For instance, a brief review on the American Sociological Review appeared (Feb. 1953) (Goodman, 1953) and in other minor social science reviews. But apparently, no real feedback can be found in the American Political Science Review pages, starting from the fact that no review was published. One exception is Riker’s extensive bibliography review of social choice literature published in the 1950s, devoid of excessive mathematical sophistication, and aimed to make political scientists aware of this recent field of research. (Riker, 1961)

Arrow explicitly paralleled the voting and the market mechanism as examples of collective choices where the ‘impossibility theorem’ occurs. Nevertheless, his work

certainly did not belong entirely to the domain of Political Science, given that the author explicitly addressed the issue of SWF (but also given its quite mathematical structure). Then, for instance, into a famous work written by a cutting-edge political scientist, Robert Dahl, and an economist, Charles Lindblom, with an approach explicitly compared to that of Classical Welfare Economics, they define the paradox of voting as “a minor difficulty in voting that people with a mathematical turn in mind enjoy toying with.” (Dahl and Lindblom, p. 422)

The Scottish Economist Duncan Black pursued a similar strand of research. He started to work on these themes independently, and he published his first papers in the same period Arrow was working on his essay. A comprehensive collection of his results was published only in 1958, *The Theory of Committees and Elections* (Black, 1958) Arrow was aware of some of Black's contributes and focused on them in the final chapter of the first edition of his work. Unlike the American economist, Black did not focus on the issue of SWF but instead only on committee decisions. (Arrow, 1963, p. 80) Most importantly, he did not limit himself to highlight the analogy, in terms of social choice, between market decision and voting, but advanced a positive program to develop a ‘pure science of politics’. His starting point, addressed in a paper written in 1950, entails the ‘unity between politics and economics. He explicitly stated that political and economic choices refer to the same superset, namely the ‘general theory of choices’. (Black, 1950)

Again, Black did not develop a formal model like Arrow and therefore did not analyze social choices in terms of consistency between fair properties of the latter and the individual rationality. His analysis is less advanced than Arrow's one in terms of mathematical sophistication. He presented the relationship between different motions and preferences as sets of discrete points (or continuous curves) for each committee member (or voters). For each of them, therefore, preference can be represented as an ordered schedule. Given such premises, Black demonstrated a first important result: if each voter has a single-peaked preference, the value associated with the median voter's preference can get at least a simple majority against every other, and it is the only one value to do so. (Black, 1958, pp. 14 et ss.)<sup>3</sup>

I need to highlight some crucial points. First, to Black, his analysis presents a striking analogy with price determination in economics. But at the same time, Black firmly remarked that exists a fundamental difference between prices and committee decisions, namely, no automatic adjustment to the opinion of median voter will emerge. (Black, cit., p. 19) Besides, whereas Arrow and social choice theorists after him, were more interested in the formal properties of their model, Black instead focussed on the positive features of his theories. Then, a central role is occupied by cyclical majorities and their recurrences (Black, cit., pp. 51-5).

The positive importance of Black's analysis was discussed extensively by Riker in his 1961 bibliographical essay. (Riker, 1961) Then, the failure to agree on standards (i.e., single-peakedness) implies the presence of social cleavages difficult to reconcile. Consequently, this also entails the problem of social norms and the agreement on them.

<sup>3</sup> Despite the different analytical treatments, Arrow showed that Black's theorem could be easily reproduced using his approach and notation. Therefore, what Black defined as single-peakedness, Arrow treated as a ‘betweenness’ property, characterized by a ‘strong ordering’, that is a relation implying only ‘more or less’. This means that given a set of alternatives, at least one is not the less preferred in all individual orderings. This condition restricts the domain of possible choice preferences, and Arrow showed that a Social Welfare Function satisfying this condition, along with the others, can be construed.

Black also tried to compute the random a-priori expectation of cyclical majorities without obtaining a general rule. Riker used his results to infer that an a-priori expectation of the existence of a majority decision, for  $n \geq 5$  (where  $n$  is the number of alternatives) is extremely small (Riker, 1961 pp. 908-910). But, apart from the low chance to obtain a majority decision, “the surprising fact is that majority decisions occur at all” (Riker, cit, p. 909). This result deserves to be examined as a central problem of political science, according to Riker, through the development of new theories as well as by experiments. Then, to him, the practical question of how majorities are ever arrived, or not, is of central importance.

The importance for the development of the political theory of the kind of formal works, like those of Arrow and Black, has been summed up by Riker as follows:

”[...] If students of political behavior were to discover and explain the range of mechanisms and social conditions leading to that agreement on norms sufficient for a set of single-peaked curves, then political theorists might be able to evaluate such notions as the public interest and the general will based on empirical knowledge, a kind of procedure which is, I regret, almost unprecedented in the study of politics. Even if such a happy outcome is not possible, many spheres of political life can, I am certain, be more perceptively explained than they have been by the use of the theory here reviewed.” (Riker, cit. p. 911)

Finally, in the decade, there was a third pivotal contribution to the literature concerning voting and formal modeling. I am referring to Anthony Downs’ *An Economic Theory of Democracy* (Downs, 1957), his Ph.D. thesis, revised for publication. Downs was an economist by training and applied the economic way of reasoning concerning the maximization of individual utility to the behavior of voters and party. Unlike Arrow (who supervised his Ph.D. thesis), Downs’ is writing for a political scientists audience and did not stress the formal properties of his model, which is considerably less mathematical than Arrow’s and even Black’s. However, the most famous intuition in Downs’ work concerns the relationship between the rational behavior of the parties and the uni-modal preference distribution of the voters. This argument has a striking resemblance with Black’s single-peakedness.<sup>4</sup>

Downs’, like Black, extended his argument also in other directions, although less fruitful. However, from some of his intuitions, an enormous amount of scientific literature spread, from the 1960s onward, especially thanks to scholars like Otto Davis, Melvin J. Hinich, Peter Ordeshook, Kenneth Shepsle, and others. Many of them had strong connections with Rochester University, and the Ph.D. program Riker established there. Furthermore, from the late 1960s onward, it was easily shown that the main results above could be addressed using game theoretical arguments. However, game theory entered political science in another way, and yet in the 1950s was partially discussed among the practitioners of the discipline. The next subsection is devoted to this.

<sup>4</sup> Note that they are different: one is referring to voters in a committee and the problem of cyclical majority (Black); the other instead is about parties, or political members, seeking for election. However, the result is similar. If preferences are single-peaked, Black showed, the winning majority ever contain the median voter preference; therefore, to win, a group of voters must adopt it, or adopt a position that is much closer as possible to it. In Downs’ argument, if voters’ preferences are unimodal, it is convenient for political parties to adopt the position of the median voter to win the election. If the preferences are also normally distributed, parties will adopt a centrist position.



### 3.2 Game theory and Political Analysis

The history of the creation of Game Theory and its spreading across domains different from economics has been the subject of different analyses (Weintraub, 1992; Giocoli, 2003; Leonard, 2010; Erickson, 2015). Besides, as showed, for instance by Erickson, Amadae, and others, game theory research, both theoretical and applied, occupied a central role in such places like RAND and from this in the development of the so-called 'Cold War Rationality' (Amadae, 2003; Erickson et al. 2015).

Less attention has been devoted to the fact that Game Theory, in the 1950s, was also applied to political science issues other than military strategy or international politics. This section aims to present how political scientists used GT in the 1950s and introduce the difference with Riker's 'Positive Political Theory'.

The first difference seems to pertain to the type of theory adopted. Indeed, whereas in International Relations, the opposition of interests and non-binding agreements represented the main facets of the analysis, pushing researchers toward so-called 'non-cooperative GT' models, Riker instead adopted 'Cooperative GT'.

Mathematician John Nash introduced the distinction between 'cooperative' and 'non-cooperative' games. He, contrary to what von Neumann and Morgenstern had done in TGEb, treated a case where it is "assumed that each participant acts independently, without the collaboration of communication with any of the others." (Nash, 2002, p. 85) Viz. where coalitions are not permitted. Nash famously defined such games as 'Non-Cooperative Games', and the earlier, studied in TGEb came to be labeled as 'Cooperative Games'.<sup>5</sup>

In a nutshell, cooperative game theory refers to the presence of coalitions, where communication, and therefore binding agreements are possible. On the other hand, 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.

In the first case, the cooperative one, the fundamental issue is that of choosing to which coalition join, given each player's expectations regarding the payoffs, what he will obtain by selecting a coalition over another (the role of 'side payments'), and finally which partition of the game represent the solution to it. Besides, in this case, there is not only one idea of the solution, but different, each with axiomatic and substantive properties which make them acceptable, both from a mathematical but also, rational point of view.

This is the original idea of von Neumann and Morgenstern for  $n$ -person ZSG. To them, what each coalition is worth can be represented by a 'Characteristic Function', and what each coalition gives to its members, by a vector, defined as 'imputation'. Since 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'). But 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). Besides,

<sup>5</sup> 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, 1953. From this point, 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. On this point, I will say something more below

von Neumann and Morgenstern focused only on two types of games: the 2PZSG, to which they presented the minimax solution, and the  $n$ -person ZSG, whose solution is the 'stable set'. At Princeton, John Nash instead carried game theory analysis many steps further. He provided a cooperative solution for 2-person games that were cooperative because players' interests are not opposite (Nash, 1950).

Nash also provided the new equilibrium solution for those games for which communication and agreements were ruled out. This is the fundamental idea of 'Nash Equilibrium'. (Nash, 1951) From this originated all the research field of Non-cooperative game theory, which after the 1980s came to prominence in Economics Theory, fostering the so-called Game-theoretical revolution in economics.

The creation of Game Theory, as showed by Giocoli, was deeply embedded in the debates surrounding the development of economic theory in the 1930s. However, von Neumann and Morgenstern presented their analysis in such a way that it could also be extended to any kind of social situation, other than economics. Indeed, their solution for  $n$ -person games should be interpreted as any accepted 'standard of behavior' within a social group, or a social organization. (von Neumann and Morgenstern, cit., p. 41; Giocoli, 2003) Besides, Leonard convincingly reconstructed the political difficulties which pushed von Neumann's analysis to reprise the study of 'Theorie der Gesellschaftspiele', after his 1928 paper, namely the collapse of the International political order in the Interwar years. (Leonard, 2010)

Then, game theory, yet in its constitutive structure, i.e. the notion of rational players, the opposition of interests, and coalitions, seemed apt to address political analysis.

In 1954, Martin Shubik, a young economist who studied under Morgenstern at Princeton, and was perhaps the only economist at the time deeply interested in Game Theory, edited a brief collection of essays where the theory of games in political behavior was explored. (Shubik, ed. by, 1954; Shubik, 1992)<sup>6</sup> The same year, Shubik, together with Lloyd Shapley, a Princeton and RAND mathematician<sup>7</sup> 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. (Shapley and Shubik, 1954)

In a different way from a long tradition of studies in political theory, where power is defined as the ability to compel someone to do something, eventually against the latter's will, Shapley and Shubik addressed the issue by means of game theory. They used in particular a solution concept developed by Shapley for  $n$ -person games, where binding agreements and transferable utility are permitted. Contrary to von Neumann and Morgenstern's 'stable set', this value is not based on stability considerations but entails the players' "reasonable expectation of reward" based on an a-priori evaluation of the entire game. In other words, the 'value added' to every coalition by a player, times the a-priori probability that the coalition will form. The players' SV is the

<sup>6</sup> This brief volume contained the pages where von Neumann and Morgenstern discussed verbally the meaning of the notion of solution; 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 (1951), where he presented voting as a problem of collective choice, and Black's 1950 about the 'unity between economics and political science'. Shubik, 1954

<sup>7</sup> 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/>

unique payoff vector that is the game's solution.

In their analysis, the two authors focused on a voting body and defined power as the chance each member of it has of being critical to the success of a winning coalition. (Shapley and Shubik, p. 787) Then, their index represents the a-priori chance for each committee member to be pivotal for a minimal winning coalition.

Despite resting on SV, a strictly axiomatic result, their analysis was not formal, since it was written for an audience likely lacking knowledge of the axiomatic method. However, they advanced some considerations regarding the fair properties of their result and how a researcher could empirically test it. Indeed, Shapley and Shubik conceived their method as a first step to address such problems as designing the size and type of legislative bodies, protecting minority interests, and even finding a criterion for 'fair representation'.

The novelty of Shubik and Shapley's formal approach, in Riker's own words, was the following:

"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 their result empirically. (Riker, 1959) 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'. Note that in the original paper by Shapley and Shubik, no such strong maximization hypothesis was advanced. However, as Riker rightly pointed out, even if it could be demonstrated that each member raised his power by moving from a coalition to another, it could be impossible to prove that this was the leading motive behind his decision. Besides, Riker's empirical result was partially disappointing. Indeed, he used the roll-call of the French Legislative Assembly and computed how the power of each member who switched his side, changed after his migration, but the final result was uncertain at most. Nevertheless, Riker was satisfied because he felt that even the missing of 'rational behavior' (or, in this case, 'power maximization') in such places as large legislative bodies, with many parties, as was the French Assembly, could be an interesting analytical result.

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 'minimum winning coalition'. Instead, he rested on the original analysis of von Neumann and Morgenstern. Moreover, he explicitly rejected, as I will show, any definition of rationality in terms of 'maximization of power'.

Concepts like Shapley and Shubik's power index never became mainstream in political science. As a matter of example, when the most important popular manual

about Game Theory in the 1950s appeared, Duncan Luce and Howard Raiffa's *Games and Decisions* where much place is devoted to these problems (and also to voting paradox), it went practically unnoticed in the main political science reviews. (Luce and Raiffa, 1957) Again, no review appeared in the APSR.

Those works which employ game theory in International Relations represented the other strand of game-theoretical applications in Political Science. Probably the most famous work in this group is Thomas Schelling's *The Strategy of Conflict* (Schelling, 1961). Schelling was an economist, and despite being awarded the Nobel Prize in 2005, his main contributes to game theory did not entail new formal statements or solving techniques. Instead, he provided many insights into how many advanced theoretical problems can be addressed in reality. In particular, he focused on the coordination problem in non-cooperative game theory, namely the problem of multiple NE and how to choose among them. In international politics, this regards mainly the resolution of conflicts and nuclear deterrence. (Schelling, 2014)

However, Schelling was an economist and not a political scientist. Then, as Riker did instead, he did not join the methodological debates in the 1950s or advanced a 'reformist agenda' within the discipline.

Rather, Morton Kaplan was a political scientist and a scholar of international relations. He published a work in 1957, titled *System and Process in International Politics* (Kaplan, 1957) where he devoted an entire chapter to discussing theory of games. To him, GT has a lot to say concerning the strategic analysis of conflicts, but it is not a substitute for all other kinds of social and political theories. Instead, it must be complemented with the analysis of the system as a whole. However, Kaplan focused only on 2PZSG and their minimax solution.

The cases above of Shapley and Shubik, Schelling, Kaplan, and finally, as I will show, Riker, are different. Shapley and Shubik's creation process was more than a translation one. They based their result on a strict axiomatic notion, to which they added some features to make it workable for political analysis. Kaplan's use of game theory involved anything original, and he embedded it in a more general theoretical framework than system analysis. Moreover, his contributions to the development of game theory *qua* theory cannot be compared to those of real game theorists. Schelling was instead a sort of middle-ground. Indeed, his use of game theory was much sophisticated than Kaplan's (he also based much of his knowledge of it on Luce and Raiffa's comprehensive textbook, a handy source for those scholars lacking previous knowledge of the theory. Schelling, 2014), but his game theoretical analysis is barely formal and not axiomatic.

The main difference with Riker did not entail their use of GT. As a game theorist, Riker was (especially in the late 1950s) more sophisticated than Kaplan, perhaps at the level of Schelling, but not comparable neither to Shubik nor Shapley, on any other RAND Theorist.<sup>8</sup> Instead, it was Riker's commitment to formal theory that represents the trademark of his approach. Indeed, to him, game theory is not only a useful tool to explain political issues but the "adequate model of political behavior". Naturally, then, he devoted many pages of his work on Political coalitions to defend the theory's assumptions, especially that of rationality.

As it is written in the preface of a collection of essays devoted to game theory

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<sup>8</sup> Although Shubik was highly dismissive about his mathematical abilities when compared with those of other Princeton game theorists, like Shapley, who was Shubik's roommate (during the graduate school years) and Nash, who was their room neighbor. Shubik, 1992

and political science, edited by Peter Ordeshook (who was among the first cohorts of graduate students at Rochester Political Science Ph.D. program), political science "has moved far from using game-theoretic models simply as analogs of political phenomena of the type 'let player 1 be China, with the two strategies'..." and instead has progressed toward sophisticated applications of the cooperative and non-cooperative theory. (Ordeshook, ed. by 1978) This statement marks the essence of Riker's research program and 'Positive Political Theory'. These words were written in the late 1970s, less than twenty years after the works aforementioned, but showed a fundamental departure from these.

In the early 1970s, the future Nobel laureate Robert Wilson provided a game-theoretical formulation of Arrow's theorem in a cooperative game theory model (Wilson, 1972). Instead, the British scholar Robin Farquharson presented an analysis of strategic voting, employing some notions from non-cooperative game theory. (Farquharson, 1969; Dummett, 2005) These studies represented the final merging between social choice theory and game theory in political science in a broad sense. Such works, especially the first, were completely formal in their methodology and represented a fundamental extension of game theory as a complete model (i.e. axiomatic treatment) to formal political issues. Again, these contributions predate of different years the definitive explosion of Non-cooperative game theory in economics, which dates back to the early 1980s, with the pivotal works of Wilson, David Kreps, and the extension of NE to extensive games and games with incomplete information. (Selten, 1965; Harsanyi, 1967; Kreps and Wilson, 1982)

Eventually, 'Positive Political Theory' also joined the game-theoretical revolution, along lines similar to those of economics (namely, cooperative models were marginalized and non-cooperative theory came to prominence). But its path toward it started well before economics, and Riker's theory of political coalitions was one of the milestones.

#### 4 Theory of Games and Political Coalitions: Riker's 1962 work

Riker's *The Theory of Political Coalitions*, published in 1962 by Yale University Press, was the most significant accomplishment of the author's intense commitment toward game theory and formal analysis. Riker 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.

This work 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, 1962, p. vii)

Riker was not a mathematician, and his work is not ("most emphatically not". Riker, cit., p. vii) 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 praise. For example, in the first half of his book, he argues 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" (from which the "size principle" is obtained), namely that winning coalitions will be constrained in their size. 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.

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 now far beyond the author's technical capabilities. However, one could find some useful insights on the effective working of political systems even there. Besides, the first chapter of the work is entirely devoted to exploring some methodological implications of his analysis and presenting to the reader the main features of his model.

Since his argument was not mathematical nor axiomatic, TPC is very different from the high theoretical game theory development in the 1950s (think of Shapley or other RAND theorists). Consequently, it does not occupy a central place in the history of the development of game theory *qua* theory. However, it has an important role in how game theoretical ideas crossed domains different from economics. Therefore, before discussing its content, I will devote the next subsection to reconstructing the relationship between Riker and the community of Game Theorists in the 1950s.

#### 4.1 Riker and the community of Game Theorists in the 1950s

According to Sonja Amadae and Bruce Bueno de Mesquita, "[...] It was Riker who bestowed on the game theory the promise of a new life after RAND defense strategists concluded it had little merit for studying warfare, and before economists grasped its promise for grounding a new mathematics of the market." (Amadae and Bueno de Mesquita, 1999, p. 278) Perhaps the latter statement is too generous because game theory, both in a cooperative and non-cooperative fashion, never ceased to be studied in places like RAND even in the 'glory days' of the General Economic Equilibrium Theory (in the 1960s and 1970s). But it is not far from being correct as Riker, by narrowing its attention mainly to such a situation that can be represented as a cooperative game, really exploited some of the ideas in von Neumann and Morgenstern's original view.

However, if we examine the relationship between Riker and game theorists closely, in the 1950s, there are no clues that suggest Riker was much in touch with them.

In general, with very few exceptions (see above), the intellectual communities of economists, political scientists, and game theorists remained disjoint communities, with almost no element in common among all of them. The game theorists' community was composed, in the 1950s, by young mathematicians interested in the most abstract developments of it, more than in its practical employment. Despite in places like RAND Corporation the emphasis was allegedly put on the applied strand of game theory, e.g., its use in strategic and international political issues, even there, in reality, pure theoretical researches were easily funded.

In that community, not surprisingly, Riker was an outsider, lacking the necessary advanced mathematical capabilities needed to produce new real theoretical developments. Riker was not a visiting member at RAND, where political scientists 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 Schelling spent research time at RAND. Amadae, 2003). Among the institutions that shaped the

so-called (and heavily debated) 'Cold-War Rationality', Riker was only a fellow of Stanford's CASBS, a place with a 'less hawkish' attitude toward formal analysis and game theory than RAND. (Erickson et al., 2013, p. 14)

The distance of Riker from game theorists could be explained by his focus mainly on a well-defined issue, coalition formation in politics, and within a well-defined framework, namely 'cooperative game theory' instead of 'non-cooperative', which was adopted especially in international relations. But perhaps the most crucial reason for his being an outsider within this community was that Riker, was advancing his theoretical agenda in advocating game theory. This agenda differed considerably from traditional approaches in political science and from the most recent 'behavioral revolution', and in the meanwhile it was not easy to be integrated into the actual developments of game theory due to Riker's mathematical difficulties.

His reminiscences in the afore-mentioned interview with Shepsle can offer insights into his relationship with game theorists' scholar community. Thus, asked if he ever sent any of his ideas to contemporaries game theorists, Riker remembered only Duncan MacRae, whose response filled with detailed criticisms is not unfortunately among Riker's papers, stored at Rochester.<sup>9</sup>

However, 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.<sup>10</sup> Riker, although in the interview with Shepsle, did not mention it, sent his manuscript of the 'Theory of Political Coalitions' both to Princeton University Press and to Yale University Press. While YUP accepted it and sent it for anonymous refereeing to Martin Shubik, Morgenstern was extremely critical at Princeton, rejecting his publication.<sup>11</sup>

In a letter he 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 very considerably, but the execution leaves much to be desired" (Morgenstern to Hubel, 16th August 1961, OMP, 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". Furthermore, 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 to spend some time to obtain a specific education in it. In fact, before writing his comment, he tried to detect who Riker was and what his education, capabilities, and scientific research were about, but without obtaining any meaningful information. (OMP, Box 83) Finally, to remark 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 his last remark, Shubik's refereeing was more supportive, and in the end, YUP published the manuscript. Although, he too levied some criticisms to Riker's discussion on mathematical concepts of Game Theory.

<sup>9</sup> <https://rbscp.lib.rochester.edu/finding-aids/D262>

<sup>10</sup> Oskar Morgenstern Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University, Box 83

<sup>11</sup> Martin Shubik Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University, Box 8

## 4.2 Riker's model: the fundamental assumptions

As seen above, Riker's analysis is twofold: firstly, he tried to refashion von Neumann and Morgenstern's analysis to make it suitable for political analysis. That is, to demonstrate the working of a 'sociological principle', the size principle, that rules how political coalitions behave. In the second part, his analysis explores how coalitions form. Instead, the first chapter of the book, that the author later remarked to Shepsle as "the most important part of the book" (Riker to Shepsle, cit. p. 15), was devoted to the assumptions of his model, as well to a general analysis of the role of modeling in political science.

Here 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; that of providing an adequate notion of causal determinism; and finally that of testing theories against facts in the real worlds, to enhance their predictive and explanatory power. The first two elements refer to the philosophical thesis Riker advanced in two papers written in the late 1950s, where he, in his words, 'squabbled over methodology'. (Riker, 1957; Riker, 1958)

As it is apparent, the theory of games fits all three. It refers to well-defined events, with clear relations among all the variables, and entailed explanation and prediction. Within the different types of game theory (see above) Riker focused on von Neumann and Morgenstern's analysis of  $n$ -person games, "which is essentially a theory of coalitions". (Riker, cit. p. 12) He chose it over the other solutions ideas for 'cooperative games' because none of them seemed significantly better than von Neumann and Morgenstern's original solution. Indeed, in his view, too much emphasis was put by game theorists on the properties of solutions, like for instance their existence, their 'reasonableness', or their fairness, overlooking the possibility of delimiting the structure of the permitted coalition directly. Instead, he made this by focusing only on those winning coalitions, viz., larger than some size arbitrarily ruled.

In the first chapter, he 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 that corresponds to the Nash Equilibrium for the same games.<sup>12</sup> 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 his opposite. However, not all political situations are 'zero-sum'. 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 opposition of interests and the need to balance between them. (Buchanan and Tullock, 1962) Riker was aware of this, but he defended this assumption by noting that zero-sum condition is present in many political situations, for instance, election and voting, where the loser cannot receive what the winner gains. Besides, he also noted that even non-zero-sum situations could be perceived by the people involved as pure conflict. Then,

<sup>12</sup> 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, 1950



he did not discarded this assumption.

The second fundamental assumption of the model was that of individual rationality. Riker criticized the 'tautological' view adopted by economists, which purportedly does not describe behavior but only defines preferences.<sup>13</sup> To him, if any idea of rationality is useful 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 (and also after his empirical investigation in the 1959 paper. See above) He assumed instead that political rationality encompasses the preference for winning over losing. Then, rational political man does not act accordingly to his chance of becoming more 'powerful' by passing from a coalition or a party to another (as presumed in the 1959 paper) but instead accordingly to becoming a member of a winning coalition. 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 until its minimum winning level.

Riker's treatment of rationality often seems flawed, and I will discuss some of his weakest points in the conclusive section of this paper. Nevertheless, this point is the most important of all his analysis because it embodies the very notion of political behavior that, according to the author, rules the nature of political coalitions.

### 4.3 Riker's model: the 'Size Principle' and strategy in coalition building

In Riker's analysis, "the fundamental principle concerning the size of coalitions" is the following:

*" 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, 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, pp. 32-33) Riker labeled this as the 'Size Principle'.*

As is apparent from the quotation above, this principle requires three features: rationality, defined in the previous subsection; the others are side-payments and perfect information. Both are standard assumptions in von Neumann and Morgenstern's analysis. The role of side payments is that each player makes his decision to join a coalition over another, based on what he is offered in them. Perfect information instead rules out the role of uncertainty. 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

<sup>13</sup> 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, pp. 18-9)

payoff, then it is said that utility is transferable. In Riker's model, side payments assume great importance where he discussed strategy in coalition-building, viz. the 'dynamical' part of his analysis.

Perfect information refers instead to each player knowing what moves the other players have done. 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, pp. 77 et ss.)

As seen, Riker aimed to discuss political coalitions, namely coalitions in those situations which involve winners and losers. Therefore, concerning von Neumann and Morgenstern, he 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 'characteristic function') reduced with the growth of its size. Put differently, a minimum winning coalition is worth more than a non-minimum winning coalition.<sup>14</sup>

Riker's reasoning is quite abstract. Its math is unnecessarily involute and often flawed and his analysis does not rest on axioms. However, his claims are easy to understand. First of all, he identified three possible types of the winning coalition; besides, he investigated if each type could be identified with an 'equilibrium' or a 'disequilibrium'. 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, p. 262, italics in the text)<sup>15</sup>

The three different types of winning coalitions are:

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<sup>16</sup>

Riker showed that, in the first type, the only equilibrium, that is the only coalition that is realizable, is represented by minimum winning size. In the same way, in the second case, coalitions are realizable only until the peaked point is reached. Finally,

<sup>14</sup> 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 a MWC. If the winning party has 70 members, it is a winning coalition, but it is not a minimum winning one.

<sup>15</sup> Unfortunately, Riker was not sufficiently precise 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  $x_1, \dots, x_n$ , then  $\vec{x}$  dominates  $\vec{y}$ ). But this analysis is not outlined in the appropriate formal terms.

<sup>16</sup> 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.

the only equilibrium size in the third case is  $m$ , the arbitrarily chosen size. (Riker, pp. 245 et ss.)

However, as stated in the first chapter, the author's aim was to establish a theory capable of being tested against real-world facts and foster useful predictions. In other words, to show that in social situations similar to  $n$ -person ZSG, only coalitions no larger than the minimum size occur.

In order to adequately address this issue, Riker recognized that the role of information was pivotal. But, then, he stated, in many events, it is apparent that no minimum winning coalitions will be reached because of the lacking of perfect information. To overcome this difficulty, he introduced the notion of 'subjectively estimated minimum winning coalition'.

He rested on the reconstruction of historical events to appraise the size principle. In his view, if one could show that the 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, cit. p. 54 et ss.) 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<sup>17</sup>, the only party that remained in the U.S. after the disappearing of the Federalist Party, as a consequence of the War of 1812.<sup>18</sup>

Besides, these pages make apparent the Riker was adopting an explicit 'instrumentalist' position, as the following quote suggests:

"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, cit, pp. 65-6)

Riker explored the role of information in a second way. Since perfect information was unattainable in real-world situations, which could explain why MWC are often not to be found, he outlined a relationship between the level of information at disposal to each player and the size of winning coalitions. Therefore, the greater the degree of imperfection or incompleteness of information, the larger the size of coalitions.

Then, according to Riker, this 'information effect' could explain why there were some 'critical elections', in U.S. electoral history. The notion of 'critical election' was developed by the American political scientist V.O.Key, to define those elections where the voters' involvement is high and new electoral groupings are created. (Riker, cit., pp. 90 et ss.) But, Riker stated, one could interpret this notion as a period where

<sup>17</sup> Which he labeled only 'Republican Party'

<sup>18</sup> 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, pp. 59-65)

the amount of information in the system declined and grew the uncertainty about the size of the winning coalition.

However, most interestingly, Riker elaborated upon some statements from 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. But this would be in contradiction with the assumptions about voters' utility maximization, because, in Downs' own words "this makes it more difficult for each citizen to vote rationally [...] As a result, voters are encouraged to make decisions on some basis other than issues, i.e. on the personality of the candidates. But only parties' decisions on issues are relevant to voters' utility incomes from government, so making decisions on any other basis is irrational." (Downs, cit. in Riker, p. 98) This paradox indeed 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 to this situation. If parties seek to maximize the share of votes only up to the size needed to become a minimum winning coalition, therefore it is not any more convenient for them to becloud their positions in any situations, but only about those issues only that are of concern to voters about whom they have imperfect information.

The 'size principle' represented an ideal standard to which any rational attempt to form a coalition should conform. But, then, it was not a solution for  $n$ -person ZSG, but a 'sociological principle' derived from some characteristic of the latter. In the second part of TPC, Riker investigated how political leaders set forth coalition-building and reach 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 conversely add new members, across a multi-stage game whose last stage represents the outcome of the process. Think for example to a voting, where a weighted majority is required and different coalitions, or in Riker's terminology 'proto-coalitions' are present. 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,  $I$ , composed 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 that 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, 1962, pp. 122 et ss.)

The nature of 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 have also costs, which the leader of the coalition itself pays, and that must be taken into account. (Riker, cit., 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 equilibria 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 that did not specify if some coalition in the  $V$ -set was winning. He introduced the notion of '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 is equal to 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 nor form a new winning coalition. Since this entails the 'Size Principle', then an equilibrium corresponds to the presence of an MWC. However, the most crucial problem with this kind of analysis is that the equilibrium in the 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, pp. 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." (pp. 147-8) Therefore, the last three chapters of TPC contained a purely verbal discussion about the components of this disequilibrium and its consequences.

## 5 Political Science and economic modeling

Riker's work was reviewed in the APSR and other social sciences reviews (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. The reason can be due to its subject and because his work did not feature real mathematical analysis.

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, 1959). I believe that this complaint can be paralleled with the publication of a strict formal 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 (Arrow, Carlin and Suppe, 1960). Again, this critical volume went unsurprisingly unnoticed 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)

This final section aims to explore some aspects of Riker's analysis, particularly regarding the author's employment of economic and game-theoretic reasoning. Previously I showed from a historical point of view how Riker was an outsider in the community of game theorists in the late 1950s. In the following pages, I will explore some theoretical issues, viz. the treatment of rationality, game theory employment, and Riker's discussion of the notion of equilibrium.

Starting with the latter issue, Riker explicitly assumed economics (alongside psychology) as his 'role model'. He interpreted 'economics' as a "coherent theory and verified generalizations", the product of "150 years of empirical investigation and refinement of theory". (Riker, cit. p. 6)

It seems, from these quotations, that he referred to economics, not as an axiomatic analysis, in the way, for instance, General Equilibrium Theorists, and also game theorists, from the 1950s onward, did, but rather as a 'positive discipline', which looked for empirical validation, using statistical arrangements, historical analysis and finally even laboratory experiments.

Following 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 (after David Hilbert). (Weintraub, 2002) Giocoli summed up the radical transformations which occurred in economics between the 1930s and the 1950s as two distinct visions of economics. (Giocoli, 2003) 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, 2005, p. 24)

In this distinction, the concept of equilibrium occupies a central place. Suppose economics is intended as a 'system of forces'. In that case, equilibrium is a 'state' of an economic process where other kinds of issues, like perfect foresight, stability, perfect knowledge, are also present and need to be addressed. Compare this with the formalist approach, i.e. the 'system of relation' approach, where equilibrium is

‘simply’ the necessary outcome, i.e., the solution, of an economic problem modeled like a mathematical problem. The difference is easy to be assessed.

In my view, Riker’s formal analysis is closer to the ‘system of forces’ vision. Indeed, 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, in a way not different from that of partial equilibrium analysis in economic models. This is apparent when he wrote: “The 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, 1962, p. 147)

This can also explain why he focused, in the last chapters, on the analysis of the components of the disequilibrium. Indeed, in a ‘system of forces’ framework, disequilibrium and equilibrium have the same importance. Besides, 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.

Instead, in a purely axiomatic model and the notion of equilibrium widely employed in game theory (that is NE), equilibrium is simply the necessary outcome of the model, i.e. the solution of a problem modeled like a mathematical problem. Naturally, then, the main issue in such models is their existence or non-existence.

In the textbook Riker wrote together with Peter Ordeshook, *An Introduction to Positive Political Theory*, the two authors presented a threefold categorization of equilibria (‘social equilibria’) which can be interpreted as evidence to confirm my view: (Riker and Ordeshook, 1974, pp. 150-1)

1. “Strong, Unique, Equilibrium”: this is the product of interactions so precise (mathematically speaking) and goals so specific that society will certainly arrive at it. If some circumstances displace this, society will return to it as soon as possible. The standard example is price formation in a competitive market (i.e. General Equilibrium Theory) In political science, one can find this type of equilibrium in Social Choice Theory.
2. “Weak, unique equilibrium”: A social outcome that is the product of (usually) more complicated interactions toward more complicated goals. Riker’s instance is that of monetary Macroeconomics.
3. “Non-unique equilibrium (unstable equilibrium)”: A social outcome that is part of a set of outcomes, where the set is such that the interaction of goal-seeking persons will lead them to some unspecified outcome in the set, not necessarily that one toward which the society originally began to move.

Perhaps not surprisingly, Riker associates his analysis of Political Coalitions with a type II equilibrium and not with substantially mathematical framed type I and type III equilibria. (Riker and Ordeshook, 1974, p. 177) No surprise, given that he aimed to elaborate a positive theory addressed to the description, explanation, and foreseeing real-world phenomena.

Let now turn to Riker’s analysis of rationality. As seen, he criticized the notion adopted by economists, at least from the late 1930s onward, that is the tautological idea that modeling rationality can be disjointed, through mathematical formalism, to the substantive content of it.<sup>19</sup> Instead, he presented an idea based on the principle of the preference for winning over losing.

<sup>19</sup> In the 1930s, it was elaborated the idea of ‘revealed preferences, that is, to infer the pattern of

His rejection of a preference-ordering argument could be explained by the attempt to make sense, for political scientists, of the idea of political rationality. Therefore, an audience who is not comfortable with mathematical sophistication could easily grasp a more meaningful definition. However, it seems that Riker's argument is extremely weak and does not fit well in the discussion he advanced about modeling in social science.

Indeed, Riker defended the assumption of rational choice adopting what he defined as a 'summation argument': even if not all agents are rational, the most important agents are. But rationality in economics has another and more important meaning. It is a way to constrain the beliefs and desires people are allowed to have for their actions to make them explainable. In this sense, even if certain ideas regarding rational behavior can be interpreted as a way of describing actual people's behavior (think for instance to the minimax solution, the rational way of solving a 2PZSG, as 'prudence'), this does not preclude the fact that modeling rational behavior requires strong assumptions concerning not only beliefs and preferences, but also their formal structure. In other words, there is no contradiction between a purely tautological argument and a positive analysis of concrete human behavior.

Resting only on one of the two, as Riker seems to do, undermines the generality of his argument. But, again, this can be attributed to his resting on an idea of economic analysis that was increasingly being displaced by new ideas concerning axiomatization.

Finally, I will give some insights on Riker's employment of game theory. In particular, I want to focus on what he said regarding the dynamics of coalition-building. Whereas in the first part of TPC, he tried to show how political coalitions aiming to win should conform to the 'size principle', in the second part, he discussed, from the strategic point of view, how coalitions were formed. Strategic reasoning has a pivotal role in these pages, and von Neumann and Morgenstern's analysis, which is eminently static is no longer sufficient. Instead, an analysis of how the process occurred through time and how leaders and followers interacted (for instance, by making a bargain) was necessary.

As noted, John Nash, at Princeton, to overcome the perceived weaknesses of von Neumann and Morgenstern's analysis, made two pivotal contributions to the theory of games. One entailed the idea of 'non-cooperative' games and the 'Nash Equilibrium' solution. The other encompassed an attempt to bridge the gap between cooperative and non-cooperative games, reducing the first to the second. (Nash, 1951; Nash, 1953) In particular, Nash presented a model where a bargain among two players (a cooperative game for which he had provided axiomatically the solution in 1950) reduced to a two-stage non-cooperative game concerning the determination of what is to be bargained (the 'status quo' in the original model) as well as how to set forth the bargain.

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choices of different individuals, one must rest only upon their effective choices, and nothing more. After von Neumann and Morgenstern axiomatized the idea of utility function under uncertainty, axiomatic decision theory became part of the theoretical corpus of economics. (Arrow, 1951; Debreu, 1959; Moscati, 2017) Furthermore, assumed prominence what Game Theorist and philosopher Ken Binmore defined as 'the consistency view' of rational action. This meant to frame in a mathematical and logical fashion those features which characterized individual action in classical and modern philosophy. "[A]n agent's strength of body becomes his feasible set [...]. His passions become his preferences. His experience is summarized by his beliefs. His reason becomes the set of rationality principles that guide his choice of an optimal action from his feasible set, given his preferences over the possible consequences and his beliefs about matters over which he has no control." (Binmore, 2014, p. 4)



Due to the state of game theory in the 1950s,<sup>20</sup> and especially its place within mathematical economics, this 'Nash Program', despite some attempts of addressing it, remained largely not fulfilled, at least until the game theory revolution in economics occurred, after the 1980s. (Binmore and Dasgupta, 1987; Serrano, 2005)

It is not unlikely that Riker was aware of Nash's result, even if he never referred to it in TPC.<sup>21</sup> Nevertheless, it is interesting that Riker noted a problem that has a close resemblance with the 'Nash Program'. In particular, he said how it could be extremely difficult to infer anything concerning strategies in  $n$ -person games, especially in von Neumann and Morgenstern's theory. This seemed to reduce von Neumann and Morgenstern's result to a simple algorithmic procedure, fostering no discussion about strategy in a dynamic setting and no prediction about the best course of action.

Therefore, in his analysis of how coalitions were set forth, Riker tried to develop an ambitious analysis of a multi-stage game, where leaders and followers could behave as players in non-cooperative situations. But, unfortunately, what he necessitated to make sense of his model (at least from a game-theoretical point of view) was simply beyond his reach. Too much, in terms of game theory notions and solving techniques, was still to be created. Besides, what was yet existent, as the notion of the extended form of the games, or that of 'information set' (both discussed by von Neumann and Morgenstern in the second chapter of TGE), was not sufficient.

Indeed, Nash's idea of bridging cooperative and non-cooperative games required the extension of NE to extensive games. This idea was fully developed only from the second half of the 1960s onward, starting with the works of Reinhardt Selten. (Selten, 1965)<sup>22</sup> Riker instead foresaw the possible development of GT but was unable to pursue it.

## 6 Conclusion

This paper showed how the game theory was employed in the late 1950s to address political issues. As seen, Riker was not the only social scientist to advance a similar research agenda. But he was alone, among the political scientists, to consider game theory the most adequate technique to advance the scientific understanding of politics.

Riker's main aim was that of uttering true sentences about political phenomena. In doing this, he found that game theory and economic theory could be useful to this scope. This approach raised and still does, many objections, but these came mainly after 'Positive Political Theory' started to occupy a prominent role in the American Political Science scholar community. In the 1960s, until the mid-1970s, it still maintained a peripheral role.

I focused on how Riker's analysis differed from economics and formal game theory,

<sup>20</sup> Comprised the fact that Nash did not return upon this idea, focusing instead on pure mathematics problems for the rest of the decade, before his personal difficulties in the 1960s and 1970s.

<sup>21</sup> In Luce and Raiffa's book, that, as seen, was the most comprehensive source for game theory in the late 1950s, this result is discussed in a section of the chapter devoted to 2-person games. However, they were pretty critical of it, sensing that it would have hardly any relevance to players since it was an entirely artificial mathematical device. (Luce and Raiffa, cit., p. 140)

<sup>22</sup> Selten developed the idea of 'perfect subgame Nash Equilibrium', which was a stronger idea than NE. Together with Bayesian Nash Equilibrium, this idea represented the most decisive development of Non-Cooperative GT and paved the way to the game-theoretical revolution in the 1980s. In a nutshell, a non-cooperative game in extensive form can be divided into different sub-games, concerning the kind of information disposable to each player. Namely, if the information is perfect, then each node of a game tree, letting apart from the terminal nodes, can represent the subgame's initial node. A perfect sub-game Nash equilibrium is the strategy profile which is a NE in every sub-game. Gibbons, 1992

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 perhaps 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' aligned 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. This reconnects with what he stated about the importance of an adequate model of political behavior to address why, after all, political events like majority elections occur. (Riker, 1961) To this aim, more than a mathematical sophistication which 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, cit. p. viii)

With the passing of the years, the difference between formal political science and economic theory narrowed, up to the point that important contributes 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 and Banks, 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 of this paper.

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