Looking for a Science of Politics

William H. Riker and the adoption of Game Theory in Political Science

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INTRODUCTION

IN THE 1980S and in the 1990s, Game Theory became the most lively field of economic theory. Its astounding success can also be appraised by thinking of the widespread diffusion of such notions, now common usage words, like "zero-sum," "games," or even "prisoner's dilemma."

In the words of one of his early historians, E. Roy Weintraub, the simplistic view of the history of Game Theory in Economics goes as follows: "[...] von Neumann wrote a paper in the late 1920s on two-person games and minimax. Borel claimed priority but this claim was rejected as mistaken. Then von Neumann and Morgenstern got together in Princeton, wrote their book in 1944, and the word went forth. The story goes on to tell us that, unfortunately, economists were slow to see the importance of the theory. Thus although two-person theory was solved early on, the interesting issues became those of n-person cooperative theory. These problems took a long time to solve, but finally the core emerged as a good solution idea for economics and helped to unify topics in general equilibrium theory. Subsequently information problems emerged in microeconomic theory which could be studied by attention to the nature of the Nash equilibrium theory of non-cooperative games, and this is the area of current work in game theory in economics." (E. Roy Weintraub 1992, p. 7) However, more than thirty years of historical work have demonstrated that this story is superficial and misleading and that Game Theory struggled to enter the economists' toolbox.

This dissertation aims to further contribute to the history of Game Theory by showing how the Theory of Games crossed the disciplinary domain of Economics and entered into Political Science. This process was mainly due to American political scientist William H. Riker. Albeit trained as a traditional political scientist, in the second half of the 1950s, Riker became really committed to game theory and formal (viz. mathematical) political science. His activities were pivotal in devising that subfield of contemporary political science, known as "Positive Political Theory."

Furthermore, Riker's activities were not limited to advocating the use of Game Theory. Instead, he was also an exceptional "intellectual entrepreneur," whose efforts spanned from providing game-theoretic analyses of political issues to advancing his research program within Political Science as an established discipline. The latter was made possible by Riker's role in setting up a department, especially a Ph.D. program, at the University of Rochester (NY), mainly devoted to developing analytical models of politics other than recruiting and training scholars to do so.

Riker is the main character of the following research, and I will dwell extensively on his life and his place within the Postwar American Political Science, the latter being a field of research that experienced a series of disciplinary transformations in its scopes and methods. However, I will overlook Riker's early analysis as a "traditional" political scientist and even his important works on federalism. (Riker 1953; for a overview of Riker's theory of federalism see: Filippov 2005) Besides, I will only briefly mention Riker's perhaps most-known accomplishments among political scientists: his political theory of liberalism vs. populism and his

theory of "herestetics" (namely, the 'rhetoric' manipulation of voting outcomes). (Riker 1982)

In a nutshell, my thesis is neither an intellectual biography of Riker nor a comprehensive overview of his scientific activities.[1] Furthermore, it is not a review of the numerous epistemological, methodological, and philosophical critiques levied against any attempt to provide a Rational-Choice explanation of politics. Instead, it is a history of contemporary Economics/Political Science centered around a precise topic: the entry of Game Theory into the toolbox of Political Science.

In the last thirty years, no one has probably devoted more effort than E. Roy Weintraub to investigating how the history of economics can be studied. He outlined several ways of writing the history of economics. (E. Roy Weintraub 2002, 256 et ss.) The first assumes that Economics, like other sciences accumulates instances to elaborate a rational reconstruction of how ideas progressed through centuries, from the most primitive analyses to the most advanced ones. Evidently, this is a naive position, and this naivete is only partially adjusted by the most comprehensive view that Weintraub defines as "critical rationalism." This position assumes that it is not entirely true that the history of science is the cumulative history of scientific knowledge. Still, when discussing science and its history, one must look at "exemplars of good science." "Good science" can be falsified (à la Popper), or it encompasses "progressive research programs" (after the Hungary-born philosopher of science Imre Lakatos). Similarly, "bad science" is a "regressive research program." Finally, there is also the Kuhnian idea of "scientific revolutions." Both produce histories of progress and decline within science. In the case of economics, as Weintraub pointed out, they provide "a sense of vitality of economic science." (E. Roy Weintraub 2002, p. 260). However, they still give centrality to the idea of science as a cumulative enterprise. Consequently, a most comprehensive view, at least from the historical perspective, is provided by the so-called "science studies approach." There, the aim is to develop "a perspective based not on asking of science how it should be done, but rather how it was and is done." (E. Roy Weintraub 2002, p. 267)

Weintraub's taxonomy can be paralleled with the most "traditional" historiographical discussions, especially Joseph Schumpeter's famous dichotomy between "analysis" and "vision" and Lakatos' one between an "internalist" and an "externalist" science history. (Schumpeter 1987; Lakatos 1978) In a nutshell, Schumpeter's history of analysis and "internalist" histories point to reconstructing the content of a definite theory or idea. Externalist history instead reconstructs the intellectual framework, institutions, journals, places, et cetera, which contribute in many ways to an author's work. Schumpeter's "vision" entails instead those "preanalytic cognitive act that supplies the raw material for the analytic effort." (Schumpeter 1987, p. 39)

In the same years (the 1950s-1970s), when Game Theory struggled to obtain recognition among economists, it caught the attention of other social scientists, even political scientists. Part of this story is well-known, especially the application of game theory in international relations and military strategy fields. Furthermore, much attention has been paid to the role of game theory in the so-called "Cold

1 A definite, although introductory, appraise of Riker's academic career and impact on the study of Political Science is Maske and Durden 2003. They also provide a citation analysis for his works and commentaries from colleagues and students.

War Rationality" and the institutions which shaped it, like the notorious RAND Corporation. (Amadae 2003; Erickson et al. 2015) However, less attention has been devoted to how the same diffusion took place in Political Science and how it has affected the discipline's methodological canon in its interaction with the rise of novel sub-disciplines such as Public Choice, Political Economy, and "Positive Political Theory."

This latter story displays interesting features that I will explore in detail.

The label "Positive Political Theory" was coined by Riker in the late 1950s and refers to the alleged superior descriptive power, with respect to other kinds of political theories, achieved by adopting Rational Choice Theory and Game Theory. In his view, thanks to these methods, Political Science was finally able to pass "from the purely inductive to include the deductive as well." (Riker and Peter C. Ordeshook 1973, p. xi) Riker exhibited a positivistic vision of Political Science, intended as a progressive enterprise to elaborate a "genuine science of politics." In his view, deductive theory of an axiomatic kind made it possible to define the phenomena to be explained more precisely, and therefore, their empirical investigation in order to obtain valuable predictions, along the lines of what postwar Economics has started to do.

However, it will also be shown that such a bold statement was somehow beset by many problems, both theoretical and epistemological. Therefore, a more contemporary and less heroic definition of "Positive Political Theory" is the following: "Positive political theory is concerned with understanding political phenomena through analytical models which, it is hoped, yield insight into why political outcomes look the way they do and not some other way." (Austen-Smith and Banks 1999, p. xi)

This dissertation aims to discuss why Riker's original idea was problematic or perhaps too "optimistic" regarding the actual explanatory and predictive power of Game Theory. On the contrary, as the long-lasting debates in the philosophy of economics demonstrate, the actual power of Game Theory rests in providing an essential tool for modeling phenomena, although not necessarily with a strong normative stance or, even less, with strong predictive power. (see Reiss 2013; Rubinstein 2007) This view is definitely entailed in the second definition of "Positive Political Theory" outlined above.

This also encompasses a discussion of the formal aspects of Riker's theory. Indeed, he adopted "cooperative game theory," not the "non-cooperative game theory," thus exploiting game theory's creators John von Neumann & Oskar Morgenstern's original ideas. My research will argue that, as far as Positive Political Theory is concerned, a much more relevant connection existed, both theoretically and institutionally speaking, with von Neumann/Morgenstern's original view of game theory. The non-cooperative, Nash-driven revolution would significantly affect the study of politics only much later, starting with the late Seventies and early Eighties, i.e., at about the same time it finally conquered Economics.

However, Riker's analysis was flawed from a mathematical point of view. Therefore, letting aside biographical elements (the fact that Riker lacked the in-depth training in mathematics necessary to fulfill his high theoretical ambitions), I will also provide an epistemological explanation of how Riker enacted Game Theory and how this differed from how economists employed it.

Finally, strictly related to these points is the controversial issue of "economic imperialism." Indeed, if any extension of economic analysis outside its domains

is a justified intellectual activity or not, such an issue is often used to assess the epistemological consistency of Rational Choice and Game Theory in those disciplines like Sociology, Political Science, and History.

As for the methodology of this work, this thesis is a historical reconstruction of Riker's early works, primarily *The Theory of Political Coalitions*, published in 1962. It displays both "internalist" and "externalist" elements without fully committing to only one of these approaches. It is an 'internalist' work since I discuss the specific use Riker made of Game Theory, providing some criticisms to part of his work, starting with the issue of rationality. At the same time, I will rest heavily on historical research, like archival material and interviews, and I will try to reconstruct also the external forces that bolstered Riker's efforts to establish "Positive Political Theory."

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2

THE PARALLEL PATHS OF ECONOMICS AND POLITICAL SCIENCE: FROM THE EARLY XXTH CENTURY TO THE POSTWAR

THE INTERWAR AND SECOND POSTWAR years saw dramatic changes in the history of social sciences. The following pages aim to reconstruct such history for Political Science and Economics cases briefly.

For economics, the main development consisted in its becoming a mathematical discipline. The development of economics, up to its contemporary mathematical fashion, has been explored by many scholars, starting from Weintraub's pivotal studies (e.g., E. Roy Weintraub 1983; E. Roy Weintraub 2002; Ingrao and Israel 1987; Mirowski 1992; Mirowski 2002; Giocoli 2003b; Giocoli 2003a and others). A central role in all these analyses is occupied by increasing abstract mathematical formalism, turning Economics into a mathematical science. The outcome was that the ways economic theory was conceived and taught were radically modified within thirty years (1930-1960). Such a change entailed the shifting from, following Giocoli, the traditional image of Economics as a "system of forces" toward the new image of economics as a "system of relations." (Giocoli 2003b; Giocoli 2009a) According to the first view, economics analyzes the economic processes generated by market forces (including, but not exclusively, economic equilibria). According to the second view instead: "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 2009a, p. 24) For instance, the Nobelist Robert Fogel reported a statement made to him by his colleague Lionel W. McKenzie, one of the most important postwar mathematical economists, in the 1960s, when both were part of the faculty of Rochester University Department of Economics, about the existence and uniqueness of General Economic Equilibrium: "We know that equilibria exist because markets produce them every day. The problem is that we ran into difficulties in demonstrating their existence in our models". (Fogel et al. 2013, p. 84)[1] Thus, Giocoli's distinction points out the existence of radically different questions raised by the progress of the discipline.

From a historical point of view, the steady progress of Mathematical Economics also affects how the history of the discipline is seen by its practitioners. It implicitly legitimizes a view of the development of economic theory as a march onward through the progressive refinement (and creation) of new mathematical tools to address new problems or offer new insights into old ones. Gerard Debreu (Nobel Prize winner in 1983), and perhaps the most "extreme" among mathematical economists (see below), defended the employment of Mathematics in Economics (axiomatic method) based on four advantages he repeatedly referred to: generality, weakness of assumptions, clarity of expression, and freedom from ideology.

1 McKenzie's role in chairing the department of economics in Rochester from the late 1950s will be explored in a separate chapter of this dissertation since Rochester was also the institution where Riker established his formal approach to political science, and there was certain proximity between the two departments.

(Düppe 2010). Of these, the latter, in particular, can assume importance in the case of economics. It can be associated with the "whig perspective" of ongoing development and refinement of theories. Thus, in reconstructing their history, it could be possible to separate them from the institutional and social contest which framed them. As a consequence, it should hold what Debreu stated about mathematical economics: "[...] even though a mathematical economist may write a great deal, it usually remains impossible to make from his works, a reliable conjecture about his personality." (Düppe and E Roy Weintraub 2014a, p. xiii).

Eventually, key transformations took place in the XXth century in political science. However, the result differed from economics because a substantial narrowing of methodological pluralism did not accompany the path toward increasing systematization. This development can be summarized as the "epistemic shift" from what some authors define as "developmental historicism" to "modernist empiricism." By the first, Robert Adcock and Mark Bevir encompass some XIXth century social sciences trends, like philosophical idealism, positivism, Whiggism, and early evolutionary theories. Instead, "modernist empiricism" is widely intended as an approach "[...] based on a rigorous accumulation of facts coupled with the modernist view of science and reality as parts of a probabilistic world and of various new ways of ascertaining reality."(M. Smith 2009, p. 118; Adcock and Bevir 2006) Finally, in the 1950s, the efforts to establish systematic political science culminated in the Behavioralist movement, which became the dominant approach in political studies for that decade and part of the next. These efforts entail discussions about the proper methods and scope for a scientific analysis of politics.

Thus, creating a "real science of politics" meant also dealing with the appropriate relationship between this and political philosophy, political theory, history, and constitutional law, namely the disciplines traditionally devoted to exploring the issues of political order. The exact nature of this relationship has been one of the main starting points for the development of autonomous political science. Therefore, the history of Political Science is the history of the different answers elaborated to illuminate the inner nature of political phenomena. As in the history of other disciplines, Political Science can be seen as a sequence of attempts to cope with different problems by developing different theories and methodological approaches.

In the following chapter, I will offer a general reconstruction (although not exhaustive) of some prevalent features of the development of economic theory from the interwar period up to the 1950s. These parts will be devoted to historical reconstruction, grounded exclusively on secondary sources, with no pretense of originality. Indeed, such discussion aims to provide the historical framework to better address the development of game theory and formal theories outside the boundaries of economics from the 1950s onward.

I will also present a "linear narrative" of the development of Political Science, focusing mainly (albeit not exclusively) on the American case. In doing this, I am accepting, implicitly, two diffused interpretations: that of 'Behavioralism' as the appropriate lens for reading the development of political science; and the narrative of "Americanization" of the discipline. (Somit and Tanenhaus 1967) Therefore I will conclude this introduction by outlining a few points to explain my choice.

Given the pivotal role of the behavioral movement in shaping the disciplinary development of political science in the second postwar, a common framework for the history of political science is the periodization of "Pre-Behavioralism, Behavioralism, and Post-Behavioralism."[2]Consequently, the "Behavioral Revolution" is central to how contemporary political scientists envision their discipline's past. Instead, the narrative of "Americanization" means that American scholars pioneered the institution of political science as an autonomous discipline. (Adcock and Bevir 2006, p. 71) American scholarship was undoubtedly influenced by the transnational migration of ideas and individuals. Still, for almost half a century, the existence of an autonomous discipline of political science, as well as independent scholars' association, was a North American anomaly, while in other countries, the development of political science as an autonomous discipline is mainly a second postwar phenomenon. (Easton, Graziano, and J. Gunnell 2002) As an example, the "American Political Science Association" was established in 1903, and in 1906 the first issue of the association journal, the "American Political Science Review," was published. However, in that period, the "American Economic Association" was already in existence (founded in 1885), and the "Royal Economic Society" in England (from 1890). Instead, the English "Political Studies Association" was formed only in 1950 due to the UNESCO Symposium on 'Contemporary Political Science." (Kenny 2009).

However, both these interpretations have been contended by many authors and historians. For instance, Adcock has shown convincingly that the belief in Behavioralism's revolutionary role must be tempered. (Adcock 2009) Behavioralism was undoubtedly innovative in its character and impact, but some changes were neither immediate nor radical, contrary to what is often implied. Moreover, Behaviouralism lacks a specific definition, and even interpreting it as a scientific revolution is highly problematic (Robert A Dahl 1961). John Dryzek reconstructed the history of American political science through five different "revolutions without enemies." (Dryzek 2006) In his narrative, the employment of the concept of revolution, intended as scientific revolutions, is only apparent because none of these can be qualified as a "paradigm shift" in a Kuhnian sense. In fact, to him, only two revolutions were successful. Then, the main feature by which engaged scholarship could have transformed the discipline in a revolutionary fashion is that there are no enemies prepared to resist. (Dryzek 2006, p. 487) That behavioralism was one of the two successful revolutions (the other was the Statism (see below)) does not suffice to interpret the history of political science exclusively in terms of it. Behavioralism did not radically modify the "image" of the discipline like, for instance, mathematical formalism did for economics.

Finally, it is also incorrect to treat political science in the 1950s and 1960s as exclusively dominated by Behavioralism (indeed, as I will show, Riker's formal approach was conceived during that period, also in opposition to Behavioralism, although not exclusively). (Adcock 2009). However, other scholars have shown that the historical development of Political Science followed similar lines, at least up to the 1950s, in the United States and Great Britain (Ross 2009). Then, this narrative could legitimize the attempt to write the history of political science exclusively in the function of the development of the American discipline. Alongside the functionalist interpretation of Behaviouralism as the unique and narrow

2 Despite the lacking of a precise definition of Behaviouralism, its features as well as its meaning will be discussed in the second section.

path by which the contemporary discipline has emerged, the main risk is Political Science history's "whig" narrative.

Despite my agreement with all these criticisms, I maintain the necessity of an incremental view of the development of political science exclusively because the aim of these pages is not that of presenting a systematic history of Political Science but instead, a simplified, albeit exhaustive, framework of the institutional and disciplinary status of political science up to 1950s.

2.1 THE DEVELOPMENT OF ECONOMICS

2.1.1 *The rise of Mathematical Economics*

THIS PARAGRAPH AIMS at present some features of the dramatic changes that occurred in economic theory in that period, leading up to the creation of the Theory of Games and the mathematization of economics. However, issues such as business cycles, utility theories, and cost theories will not be discussed. Instead, I will focus on some aspects of the mathematical transformation in economics and its role in reshaping economic theory in the axiomatic Neo-classical fashion of the second postwar.[3]

In the 1930s, despite the so-called marginal revolution, in the last quarter of the 19th century, having introduced increasing abstractness to treat counter-intuitive concepts, like marginal utility or marginal productivity, there were still some methodological differences between different "schools." These differences were about the employment of mathematical reasoning, statistics, and economics's inner meaning and scope. In the "years of high theory" (Shackle 1967), the issues at stake were consumer theory, business cycles, production, and capital theory, and from a methodological point of view, whether economics was an apriori discipline or could adopt a Neo-Positive perspective. An important issue, missing in Shackle's narrative but filled firstly by Weintraub and Ingrao and Israel (E. Roy Weintraub 1983; Ingrao and Israel 1987), was the adoption of the axiomatic method to solve fundamental economic problems like modeling individual rationality, and, from this, the early determination of General Economic Equilibrium. Moreover, these scientific and theoretical efforts were deeply embedded in analogous scientific transformations in Mathematics and theoretical physics.

Some authors have labeled the radical mathematization in the years following the Second World War as a "formalist revolution" (Ward 1972; Blaug 2003). In Mark Blaug's words, its central tenet is "not just a preference, but an absolute preference for the form of an economic argument over its content." Moreover, "the Formalist Revolution was much more than applying mathematical techniques to

3 Usually, the comprehensive histories of economic thought end with Keynesian economics, offering only a glance at most contemporary theories or Mathematical Economics. On the contrary, the (often) extremely historically detailed works devoted to such topics usually do not comprehend the development of economic theories. It often seems challenging to read them properly without the earlier. A classical general work that is highly detailed about the development of economics between 1870 and 1930 (whose knowledge helps understand more specific results), in my view, remains Blaug 1997. Another classical piece, although much less general than Blaug, is Shackle 1967. Particular issues are instead treated in Ingrao and Israel 1987; Düppe and E Roy Weintraub 2014a (General Economic Equilibrium); Morgan 1990 (Econometrics); E. Roy Weintraub 1991 (Economic Dynamics); E. Roy Weintraub 2002 (Mathematical Economics); Giocoli 2003b (Game Theory and Rationality); R. Leonard 2010 (Creation of Game Theory); Moscati 2018 (Utility Theories)

economics. It was, rather, reveling in mathematical modeling as an end in itself and treating the equilibrium solution of the economic model as the final answer to the question that prompted the investigation in the first place." (Blaug 2003, p. 396)

Instead, other scholars have preferred to avoid this label, given the double ambiguities of the concept of "revolution" and that of "formalist." (Giocoli 2003b, p. 6) The validity of such criticism notwithstanding, the change in Economics theory and practice between the 1930s and the 1950s is a matter of fact.

Debates about the proper adoption of mathematics in economics date back to the mid-XIXth century, to authors like Augustine Cournot, William Whewell, Hermann Heinrich Gossen, Jules Dupuit, and Johann Heinrich von Thunen, among the others. The marginal revolution after the 1870s strengthened the relationship between Mathematics and Economics, also introducing a new element. The standard justification of mathematical Economics was based on the intrinsic quantitative nature of economic phenomena (this was still, generally speaking, the position of authors like William Stanley Jevons and Leon Walras). However, some authors, more mathematically trained, like the American Irving Fisher, explicitly defended the adoption of Mathematics in Economics based on some features of mathematical reasoning, especially as a way to judge the inner consistency of a theory. (I. Fisher 1892) Fisher's view anticipates those that will become customary decades later.

In 1909, Irish-born Oxford-based Francis Ysidro Edgeworth offered a synthesis of both positions. In fact, to him, economics possessed the essential condition for applying mathematics, namely the constancy of quantitative relations (his example is the law of diminishing returns). (Edgeworth 2008) However, at the same time, such use is not limited to these quantitative relations. In fact, to him: "[...] the mere statement of an economic problem in a mathematical form may correct fallacies. Attention is directed to the data which should be required for a scientific solution to the problem [...] The mathematical method is useful in clearing away the rubbish which obstructs the foundations of economic science, as well as in affording a plan for the more regular part of the structure." (Edgeworth 2008, p. 461) Following Alfred Marshall's prominent position, Edgeworth also highlighted the risks of abuse of mathematical reasoning, especially its being often overrated. Consequently, he maintained that mathematical treatment is not helpful if a problem has not been economically studied and analyzed.

Starting from the 1930s, Mathematics assumed a new role in economic theory. Historians, philosophers of science, and discipline practitioners have given different interpretations of such a radical change over the years. The common problem is pointing out how changes in economics affected its relationship with mathematics, up to integrating some aspects of the latter. (E. Roy Weintraub 2002) In this sense, the radical transformations of late XIXth-early XXth-century mathematics, namely its axiomatization and David Hilbert's formalist program, played a role. Consequently, the problem is how axiomatic mathematical theory entered economics and the differences with previous employment of mathematics (like Calculus). In a more general way, this is also deeply connected with the debates about the philosophy of science, and the nature of scientific knowledge, especially in the German-speaking intellectual, scientific and philosophical world (like Neo-positivism).

Related to that, is also the issue of how "mathematical economics became only economics" (Orozco Espinel 2020), namely how such mathematical methods became dominant in the discipline, up to marginalizing practically any kind of theoretical approach but the mathematical ones.

Thus, a historian can present the following (rational) outline of the interwar debates about Economics. Proper definitions and concepts are needed to provide valuable knowledge of economic phenomena. One possible helpful concept is equilibrium, which paved the way to new issues: reaching it and defining it properly. It also involves specific discussions about rationality and human economic actions. Such problems also comprise more general questions about the nature of Economics as a science. Then, psychological and hedonistic explanations must be set aside since they cannot be appropriately demonstrated. The same holds for any theory based on perfect foresight. For some authors, this disputes the general validity of any equilibrium approach (a route followed by Friedrich August von Hayek. Hayek 1937). Others, although raising concerns similar to that of Hayek (see for example Morgenstern 1976a), pursued, as a possible getaway, the path offered by the increasing mathematization of economic analysis. Philosophical discussions about the nature of economic knowledge and the status of Economics as science (and scientific knowledge in general) provided a positive evaluation of the mathematical approach, other than that discussed concerning quantitative analysis.

Besides, also contingent concerns about political and social affairs played a role. As shown by Robert Leonard about the case of John von Neumann, real political matters like the disruption of the European political system after the Nazis' ascension to power occupied a vital role in his further discussions about the concept (and the meaning) of equilibrium, and more in general, the concept of solution in his Theory of Games. (R. Leonard 2010).[4] The same also holds for people like the Austrian mathematician Karl Menger, whose role in connecting Mathematics and Economics and the different communities of their practitioners was pivotal.

To properly appraise these developments, historians of Economics have explored the evolution of economic theory, mathematics, and the Philosophy of Science and the personal and institutional connection among scholars in different places. For instance, according to Weintraub: "[...] The history of economics involves a history of not only the development of economic knowledge but the development and changes in the image of economic knowledge [...] Consequently, a discussion of the interconnection of mathematics and economics requires not just attention to the interconnection of the bodies of knowledge, as is reflected in the historical discussion of mathematical economics, but a historical discussion of the interconnection of their respective images of knowledge." (E. Roy Weintraub 2008, p. 462)

Giocoli, as seen, summarizes this process as transforming the image of Economics from a "system of forces" to a "system of relations," where General Equilibrium and its "existence theorem" occupy a central role. More specifically, Giocoli and Weintraub have applied the historian of mathematics Leo Corry's dichotomy between the "body of knowledge" and the "image of knowledge" to XXth century Neo-Classical Economics. (Corry 2003; E. Roy Weintraub 2002; Giocoli 2009a) In Corry's view, the "body of knowledge" generally represents a discipline's

4 See next chapter

theories, facts, methods, and open problems. Instead, the "image of knowledge" represents attitudes concerning issues like the determination of the open problems of the discipline, the relevant arguments, disagreements, and methodology. It also comprises the emergence of institutions devoted to evaluating intellectual contributions, new academic curricula, and so on. (Giocoli 2009a, p. 23)

Then, according to Giocoli, a radical change in economics' image happened after the Second World War, affecting its body. The two poles of this transformation were a traditional view of economics as a discipline dealing with a system of forces (this image corresponded to the conventional view of Economics and was embraced by the first marginalist authors) and the new idea of a discipline dealing with a system of relations. Such a difference in the discipline's image also explains the modern concept of rationality in economics, i.e., rational choice theory.

The process toward the "system of relations" image was influenced by the new philosophical concerns about the epistemological foundations of science and the logical foundations of mathematics. Equally important, however, it was also the perceived stalemate of Neo-classical economic theory in the interwar years. This stalemate was due to the different, unsatisfactory attempts to explain how equilibrium might arise from adjusting the individual agents' expectations and plans. The issues Neo-classical economists were dealing with were, on the one hand, how to explain equilibrium as a balance of forces, namely "as the rest point of the system's dynamics, and on the other, how to incorporate in their model mental variables, i.e., the actual forces which determine the process of adjustment toward an equilibrium." (Giocoli 2003b, pp. 368–9)

Neo-classical economic theory has followed two different, although not strictly parallel paths, to approach these theoretical and empirical problems. Again in Giocoli's terms, these were an "escape from psychology" and an "escape from perfect foresight." The first term refers to freeing economic analysis from any issue regarding hedonistic valuation. Strictly related to this is the adoption of the Ordinal Utility concept instead of Cardinal Utility (see the famous works of Vilfredo Pareto, John Hicks, and Roy Allen). Beyond that, psychology-free analysis by authors such as Paul Samuelson has also permitted a more empirical foundation of economic theory, following the operationalist approach. (Samuelson 1947. On Samuelson's early life (1930-1940s) see Backhouse 2017) The second term refers to the attempts to elaborate a more comprehensive, purely theoretical analysis, i.e., "a pure logic of choice," where economic agents face informational problems without perfect knowledge.

These discussions were also profoundly influenced by the debates in the Philosophy of Science. From this point of view, the spreading of Neo-positivism, his commitment to a scientific conception of the world, namely the possibility of unified science, and the emphasis on the logical-mathematical deduction had a profound effect on the way economic theory was thought. It is undoubtedly not casual that such debates took place was Vienna. Here, the discussions about mathematical social science interweaved the discussions about the logical foundations of mathematics and the developments of new forms of pure mathematics.[5]

5 In the case of economic theory, such debates involved people like Karl Menger, Abraham Wald, Morgenstern, and Karl Schlesinger, and also, although briefly, von Neumann. They addressed overall Walras' General Economic Equilibrium Theory, which was reshaped in an axiomatic fashion, in order, according to some interpretations, to establish a 'meta-theory' of economics, analogous to the attempts to establish the logical foundations of mathematics. (Punzo 1991). The postwar

Neo-positivism was concerned with such kinds of mathematical propositions and the empirical verifiability of scientific statements to provide the actual knowledge of external words. This empirical verifiability, along with the emphasis on the logical consistency of ideas, was the basis of the main feature of logical positivism: the discrimination between science and metaphysical assertions. In the debates about economic methodology, these issues can be summed up by the divergence between a deductive, aprioristic approach founded on deductive reasoning and an empirical approach. "Pure economics" propositions, derived from generalizing human behavior to understand basic principles (like diminishing utility or maximizing behavior), belong to the first. The position which put a strong emphasis on verification of economic statements and conclusions was opposed to this view.[6]

As stated before, the development of mathematics between the XIXth and XXth centuries deeply affected its employment in Economics. In the last decades of the XIXth century, mathematics underwent a deep process of rigorous foundation of its inner structure, which profoundly changed how it was understood, elaborated, taught, and employed. By 1900 the images of mathematics changed, as well as its practice. For instance, new problems emerged. They were associated with the foundations of mathematics, and new concepts like Infinity and the Continuum of Real numbers were discovered. Also, physics went through a crisis, with new problems like quanta and relativity. It means that modeling the concerns of the new physics required new mathematics.

The new mathematics was also needed to bypass the logical weaknesses of foundational problems, and its main result was the building of the axiomatic method. Now, a rigorous argument is built on solid foundations and axioms. Such a logical foundation permitted the development of new branches of mathematics and provided new logical and theoretical tools to create mathematical knowledge. The notion of "rigor" has acquired fundamental importance for affecting the new explorations in mathematical logic but also for allowing the establishment of an autonomous notion of progress for mathematics. This process radically changed the relationship between mathematics, physics, other natural sciences, and social sciences. (E. Roy Weintraub 2002. See also E. Roy Weintraub 2008)

The main feature of this transformation is the development of the axiomatic method, mainly due to Hilbert and his so-called "formalist program" for mathematics. Since Hilbert's commitment to proof theory or "meta-mathematics," all XXth-century mathematics has changed its face.[7] Historians of mathematics have offered different interpretations of such a "formalist program," mainly concern-

debates about the existence and properties of General Economic Equilibrium started from Viennese results.

6 The champions of these different positions are usually considered two English economists: Lionel Robbins (for the abstract-deductive view) and Terence Hutchinson (Robbins 1932; Hutchison 1938). From a history of ideas perspective, such contrast is not far from problematic. It seems clear that mathematical Economics, namely its reductionism about economic action, overall by postulating mathematically convenient postulates about economic behavior, is closer to the pure economics view. Indeed, some scholars have emphasized the role of Robbins' methodological views and his famous definition of economics in having influenced postwar axiomatic economics (Backhouse and Steven G Medema 2009). However, as the same authors have also recognized, Robbins, albeit not adverse in principle to mathematical economics, was not an advocate.

7 Note, however, Hilbert's profound conviction that all mathematics can be reduced to axiomatic form has been smashed by Kurt Godel's incompleteness theorems. (E. Roy Weintraub 2002)

ing Hilbert's aims and aspirations for developing the discipline. In any case, it involves the proper relationship between mathematical knowledge and reality and how the first can successfully understand the second. In the development of XXth century mathematics, these issues generated intense philosophical debates among mathematicians, but they also affected how mathematics was to be employed eventually in economic analysis. In Debreu's own words, a pivotal role was occupied by von Neumann & Morgenstern's *Theory of Games and Economic Behaviour* (1944), which paved the way for the development of axiomatic economic theory. (Debreu 2008; Neumann and Morgenstern 1944)[8]

If in the late 1930s and early 1940s, "classical" mathematical Economics reached its peak, with the employment of differential calculus and linear algebra to address and solve economic problems framed as an optimization problems (see, for instance, the works of John Hicks, Paul Samuelson and, Maurice Allais), "The Theory of Games" opened a new course by introducing logical rigor in economic reasoning and, at the same time, new mathematical tools, primary convex analysis, and algebraic topology. Convex analysis complemented the real vector analysis as a standard technique, by separation theorems, for obtaining implicit prices. Besides, perhaps the most famous example of Algebraic topology in economic theory is von Neumann's 1937 generalization of Brouwer's fixed point theorem. (Neumann 1945 Letting aside the effective adoption of Game Theory in the economists' community, von Neumann & Morgenstern's work occupies a pivotal role in the history of post-war economic theory. Indeed the 1940s theoretical economists found in it not only an axiomatic treatment of expected utility but also "a clear, almost textbook-style presentation of topics such as the geometry of n-dimensional spaces, vector operations, hyperplanes and half-spaces, convex spaces." (Giocoli 2003a, p. 9) Through von Neumann & Morgenstern's work, the postwar generation of young economists from all around the world, but almost exclusively in U.S. economics departments and research foundations, assimilated new mathematical tools and discovered a new approach to mathematical economics and economic theory. [9]

Game Theory, also because of role occupied by von Neumann (a mathematical genius, deeply fascinated, at least in his earlier years, by Hilbert's formalist program) is the most important example of how the development of mathematics affected that of economic theory. (Israel and Gasca 2009) Indeed, one of the trademarks of the "system of relations" image of economics is the emphasis on the criterion of consistency at the expense of empirical truth. Such consistency can be demonstrated only through proof of the absence of any internal inconsistency, exactly as mathematicians do.[10]

8 From now on, this work will be sometimes labeled as TGEB

9 The exception was represented by the development of mathematical economics in the USSR, which followed a path not dissimilar to that of the United States, even if, not surprisingly, the focus was not on competitive markets but decentralized production and planning problems. Nevertheless, from a mathematical point of view, there was no technical difference.

10 A constructive proof is a demonstration that outlines a procedure leading to the mathematical object whose existence is asserted (namely, calculability refers to the object under scrutiny). A non-constructive proof is a demonstration that works by contradiction, i.e., by showing that assuming the converse of what we are trying to prove leads to a contradiction. This difference is at the origin of the contrast between the so-called "formalists," who accepted both the kind of mathematical proof and the "intuitionist," who instead rejected the "non-constructive" proofs. In economics, there has been a progressive abandonment of the first kind of proof in favor of the second. (Giocoli 2003b; R. Leonard 2010)

Finally, the radical Bourbakist approach offered a further step forward. "Bourbaki" was the pseudonym used by a group of young french mathematicians, beginning in the 1930s, whose aim was that of the complete re-foundation of mathematics on a strict axiomatic basis. According to this collective, mathematics was not only a discipline that deals with formal axiomatic systems but, above all, was an autonomous subject, totally separated from the outside world and its applications. In this sense, Bourbakism was a radicalization of Hilbert's attempts to understand the actual truth of mathematical ideas. Bourbakists, in particular, aimed to elaborate multivalent theories, achieving unity inside mathematics by exploiting the full power of axiomatization. Besides, from a philosophical point of view, Bourbaki's mathematics avoided the so-called "foundational issues," escaping the dismission of Hilbert's formalist program after Kurt Godel's critiques. (E. Roy Weintraub and Mirowski 1994)

So, Bourbakist viewed mathematics as a storehouse of abstract forms, and this view, primarily through Debreu, profoundly influenced mathematical economics during the second half of the XXth century. Debreu's *Theory of Value* (1959) is the most outstanding example of Bourbakism in Economics. In this short work, the French economist (who had personal connections with "Bourbaki") explicitly aimed to treat the theory of value "with the standards of rigor," stating that such allegiance "dictates the axiomatic form of the analysis, where the theory, in the strict sense, is *logically entirely disconnected* from its interpretations." (Debreu 1959, p. x, my italics; for Debreu's connections to Bourbaki group, see E. Roy Weintraub 2002; Düppe 2012)

Debreu also offered one of the most precise summaries of the postwar mathematical approach to economic theory (Debreu 2008). A rich mathematical structure can be elaborated and used to develop a well-based economic theory. For example, the action of an economic agent can be described by an input vector and an output vector for each commodity. Therefore, a point in the commodity space (i.e., a finite-dimensional real vector space). A social system is divided into different states. Each state can be described by listing actions for each agent. Each agent selects an optimal action for him, given the efforts of all the others. Listing those reactions yields a new state, and thereby a transformation of the set of states of the social system into itself is defined. Thus, a state of the system is an equilibrium if, and only if, it is a fixed point of that transformation. The process of elaborating a formal model for an economic theory can be seen as "an inexorable process in which rigor, generality, and simplicity are relentlessly pursued." Debreu summed up it as follows:

> "An axiomatized theory first selects its primitive concepts and represents each one of them by a mathematical object. [...]The economic interpretation of the theorems so obtained is the last step of the analysis. According to this schema, an axiomatized theory has a mathematical form that is completely separated from its economic content. If one removes the economic interpretation of the primitive concepts, of the assumptions and the conclusions of the model, its bare mathematical structure must still stand." (Debreu 2008, p. 457)

Such a radical image was soon challenged, after its 1950s appearance, by a more applied view of mathematical economics. (E. Roy Weintraub 2008) These challenges notwithstanding, it has occupied for the following decades a central position in how economic theory has been produced. Economists

socialize themselves through it (for instance, in economists' advanced training, like Ph.D. courses), offering the most outstanding example of the difference between economics and other social sciences.

2.1.2 *Institutions and Communities*

THE INTERWAR AND POST-WAR CHANGES affected in-depth also Economics' institutional and social history. At the same time, these changes were also favored by some institutional arrangements. In a certain sense, the mathematization of economics required the development of special institutions, courses, and curricula in order to become effective. In the meantime, their development strengthened the position that Mathematical Economics came to occupy within the discipline.

As the end of the XIXth century saw the definitive institutionalization of Economics through the creation of new faculties, economics' departments, reviews, and national associations, the years from the 1930s onward saw the rise of the United States as the most important place where to do economic research. This "Americanization" of Economics was certainly favored by the rise of Nazism in Germany (and the annexation of Austria in 1938), after which a great number of scholars (not exclusively Jews) sought refuge in the U.S. (R. Leonard 2010; Hagemann 2011) For instance, in Ingrao and Israel's view, the development of the modern theory of General Economic Equilibrium represents perhaps the best example of the dramatic effect of Nazism in impoverishing scientific culture in Europe up to the unchallenging supremacy of the United States. (Ingrao and Israel 1987, p. 245) Then, after WWII, the United States took the role previously occupied by Great Britain but with a significant difference. The increasing mathematization of economics radically diminished the peculiarity of different traditions (the "national schools"), replacing them with a unified methodological approach: Neo-Classical economic theory, based on modeling individual behavior through concepts like concepts preferences, and endowments.[11]

To this must be added the development of extremely advanced statistical analyses, namely Econometrics. So then, despite the persistence of inner differences in terms of policies or even technical aspects [12], economics, starting from the second postwar, began to represent itself in a compact fashion, where no room remained for verbal debates like those, say, involving the meaning of 'utility.

Such a process of radical transformation of the discipline occurred in (at least) two different steps. One involved the shaping of usual economic problems in formal terms and the elaboration (often the creation) of new theories (or the reshaping of old ones) or new methods to address them (to make some examples: General Economic Equilibrium theory and its framing as a topological problem, or the creation of linear programming). The other, which followed, involved training these new theories or methods.

The second process occurred mainly from the late 1950s, primarily through projects advanced by institutions like the Social Science Research Council (SSRC. See below)

11 The construction of Economics as a discipline in Great Britain, between 1850-1950, including the differences with the developments in the United States, has been reconstructed in Tribe 2022

12 For instance, among econometricians, the proper relationship between theories and measurement. Dimand 2019. A point briefly addressed below.

The development of mathematical Economics in the interwar years, mainly in the 1930s, thus was favored by the establishment of institutions and associations, often animated, at least at the beginning, only by a few scholars, devoted quite exclusively to these scientific efforts, and funded privately. So, one cannot understand the history of economics' "going scientific" path without paying attention to such institutions as the Cowles Commission, RAND Corporation, Econometric Society, National Bureau for Economic Research (NBER), or such places as Princeton's Institute for Advanced Study. However, iron curtains did not separate all the institutions above. On the contrary, they were often profoundly intertwined, be it for the people affiliated or for the funding sources.

The Cowles Commission for Research in Economics was founded in 1932, thanks to the funding of Alfred Cowles III, an extremely wealthy investor and businessman interested in stock market forecasts. But despite this practical inclination, with the help of mathematician Harold T. Davis, Cowles was able to involve many academic and professional economists. Indeed, he widened his research center's scope and range of interest, including any discussion about employment and the development of mathematical methods and quantitative analysis in Economics (not strictly related to business forecasting). Since its creation, the Commission has been very close to the "Econometric Society" (still the most prestigious association of mathematical economists within the discipline). Indeed, among its first academic supporters there were such people as the yet-mentioned Fisher, president of the Society, and Charles Roos, a mathematician acting as secretary-treasury of the Society. (Dimand 2019; Christ 1952)

The "Econometric Society," established in December 1930 during the joint annual meetings of American scholarly associations in Cleveland (Ohio), was the first international disciplinary association in Economics. (Bjerkholt 2015) Its establishment was promoted mainly by Roos, Fisher, and the Norwegian (and future 1969 Nobelist) Ragnar Frisch.[13] Fisher was its first president, while, at the Council of the Society were elected nine members, American and European (mathematicians and economists).[14] In the Constitution of Econometric Society, adopted in the first meeting (1930) and published in the first issue of the Society's review, *Econometrica* (1933), the new association was defined as "an international society for the advancement of economic theory in its relation to statistics and mathematics [...] completely disinterested, scientific organization without political, social, financial, or nationalistic basis" whose "main object shall to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences." (Roos 1933, p. 106) In this charter, it was also explicitly stated that any activity aimed to promote such unification of theoretical and quantitative studies in economics should be within the sphere of interest of the Society. (ibidem) Thus, the Cowles Commission and the Econometric Society flourished together, and although they remained separated, their subsequent

13 Frisch was also the first to coin and to use the term "econometrics," in 1926, to define the new discipline intermediate between mathematics, statistics, and economics.

14 These were: Luigi Amoroso (University of Rome), Ladizlaus von Bortkiewicz (University of Berlin), Arthur Bowley (London School of Economics), Francois Divisia (École Nationale des Pontes et Chaussées, Paris), Frisch (University of Oslo), Roos (Smithsonian Institute), Joseph Schumpeter (University of Bonn), Edwin B. Wilson (Harvard University) and Wladimir Zawadzki (University of Wilno).

paths proceeded parallel, often intertwining. Alfred Cowles also financed the publication of *Econometrica* and acted as treasurer and secretary of the association in the 1930s. As recognized by Robert Dimand, the role of Cowles as the private benefactor of research in economic theory, mathematical economics, and econometrics in the 1930s in the United States is comparable to the definitely most known role of Alfred Loomis in American physics. (Dimand 2019) Finally, then, in the 1940s and 1950s, the Commission also benefitted from other funding sources, adding to those established initially by Cowles, like the Rockefeller Foundation, RAND Corporation, and the Office for Naval Research.

Initially, the Cowles commission was established in Colorado Springs (CO), where it remained up to 1940, before moving to Chicago (1940-1955) and later to Yale University, where it is still active, with the name of Cowles Foundation for Research in Economics at Yale University. During the Colorado years, the principal research interest was the analysis of economic fluctuations and business cycles using advanced statistical analysis. Instead, when the Commission moved to Chicago, affiliated with the Department of Economics of Chicago University, the focus was broadened to the foundations of economic theory. Such a shift was mainly due to the influence and pivotal role of Russian Empire-born economist Jakob Marshak, the director of Cowles from 1940 to 1948.[15] Indeed, in a statement made to sketch the Commission's activities, Marshak pointed out the importance of developing new statistical tools different from those usually adopted in other empirical disciplines, and new mathematical methods: "the available results of mathematical analysis are currently applied and tried out in econometric investigations; conversely, new situations arising in the course of practical work present new problems to the mathematician." (Christ 1952) Thus, alongside the development of Econometric techniques, a strand of strictly mathematical treatments of theoretical economic problems emerged in the late 1940s and early 1950s. To this aim, Marshak defended the switch from exclusively empirical and quantitative analysis instead of the focus on "the general theory of mathematical development." Among the theoretical strands that flourished at Cowles in that period were the new formal approaches to General Economic Equilibrium, Social Choice Theory, and Rational Choice Theory.

In 1949 the Commission organized the famous "Conference on Activity Analysis of Production and Allocation," whose importance, also from a historical and network analysis point of view cannot be undermined. As pointed out by Düppe & Weintraub, "that conference defined more than any single event, the emergence of a new kind of economic theory growing from game theory, operation research, and linear programming and the related mathematical techniques of convex sets, separating hyperplanes, and fixed point theory." Moreover, "[t]he conference was the 'coming out party' of the community that would transform the practices of academic economists for decades to come," establishing "the historical conditions for economics to become a modeling science." (Düppe and E Roy Weintraub 2014b, p. 454)

Finally, in the 1950s, the vast part of progress in the Theory of Games was made by mathematicians, mainly at Princeton University, yet, Marshak was among the

15 Marshak (1898-1977) obtained his Doctoral Degree in Germany, in 1922, after escaping the Soviet Union (where, during the Civil War, he was a member of the Menshevik Party and, for a while, labor secretary of the Soviet Republic of Terek). From Germany, he moved to Great Britain after the nazis' rise to power and later to the USA. For an intellectual and biographical sketch, see Kenneth J Arrow 1991; Hagemann 2011

first to offer a comprehensive review of von Neumann and Morgenstern's theory, up to offer critical discussion on expected utility theory and Rational Choice in the 1950s. (Marschak 1946; Herfeld 2018).[16]

The Cowles Commission was not the only institution committed to quantitative analysis in Economics. In 1920 a group of scholars, most notably Wesley C. Mitchell, established, again with private funding, the National Bureau of Economic Research (NBER), whose most noted accomplishment was the first estimate of U.S. national income in 1934, and later the establishment of other indexes of economic activity (among them, the GDP index). (Fogel et al. 2013). Although devoted to similar lines of research, NBER and Cowles's approach diverged quite radically, at least in the 1940s and 1950s. This difference was due to the prominence given by the latter to the role of economic theory, up that in a famous exchange with NBER's members, Arthur Burns and Rutledge Vining, Cowles' affiliate (and future 1975 Nobelist) Tjalling Koopmans famously defined the earlier's approach as "measurement without theory." The controversy was also the supposed policy preferences of the different groups, with NBER associated with more conservative stances and Cowles more open to debates on such topics as economic planning. (Dimand 2019; Levy and Peart 2020)

If Cowles Commission remained a strictly academic institution (overall after its affiliation at Chicago and later, Yale), other research centers whose role was pivotal in shaping postwar formal approaches to economics (and social sciences) were not. The most famous and influential is certainly the notorious RAND Corporation, "the Think Tank Icon of Cold War America." (Amadae 2003) So then, RAND's deeply connections with United States Military apparatus have been used (often with derogatory nuances) to highlight the intimate relations between mathematical social sciences and Cold War politics. (Mirowski 2002; Erickson et al. 2015). RAND was founded in 1946 as a private research enterprise, partially funded by U.S. Army Air Force and by Douglas Aircraft Company. It aimed to conduct research and development for the military as well as civilian use (a comprehensive, albeit controversial history of RAND's first years, can be found in Amadae 2003). For the scope of this work, two points have to be highlighted: first, despite its practical inclinations RAND also funded theoretical researches; then, among the list of people affiliated with it, for different periods, we found also pure mathematicians like John Nash or Lloyd Shapley. Also, Arrow's Ph.D. research on Social Choice benefitted from RAND's funding and the connection between the Cowles Commission and RAND. (Kenneth J. Arrow 1951b, p. ix; Kenneth J Arrow 1983) Second, among RAND researchers, more than at Cowles (and differently from Princeton Mathematics Department), applied implications of Game Theory were further elaborated, mainly about the problem of optimal strategic behavior and its application in military and international political problems. (R. Leonard 2010)

Finally, some space must be devoted to the role of some specific academic departments. Concerning Game Theory, the central location for its development was certainly Princeton's department of Mathematics and, to a lesser extent, Princeton's "Institute for Advanced Studies." [17] Indeed, John von Neumann obtained his U.S. academic affiliation at IAS when he moved permanently to

16 For enjoyable (although necessarily historically incomplete) internalist histories of the mathematical economics at Cowles, see: Kenneth J Arrow 1983; Debreu 1983

17 Given the scope of this work, I will focus on the intellectual development of game theory, and then no emphasis will be given to such otherwise essential places for the development of mathe-

the U.S. in 1933 (despite spending a relatively small amount of time in New Jersey, especially after the start of WW2). The Institute was established in the same year through the efforts of the progressive educator Abraham Flexner. Despite the projected interdisciplinarity, it remains most famous for his School of Mathematics (to which, other than Neumann, internationally and nationally renowned people joined like Albert Einstein, Hermann Weyl, Oswald Veblen, and Kurt Gödel).[18] Yet, well before the establishment of the Institute, Princeton University had a nation-leading mathematics department, with a strong interest in recent developments of pure mathematics like topology and strong connections with continental Europe's mathematicians and scientific institutions, especially Göttingen, Hilbert's University.

Similarly, Oskar Morgenstern, after the Anschluss, obtained a faculty appointment at the Princeton Economics Department in 1938, where he spent the vast part of his career. (R. Leonard 2010) Thus, despite the clear previous influences in French and German-speaking mathematical tradition, Game Theory was a product of Princeton. Some applications of it were extensively studied at RAND, and at the Cowles, too, some attention was given to it, even if the latter's members rapidly moved away to the wider approach represented by General Economic Equilibrium. However, it was at Princeton, in the 1950s, after Cowles' theoreticians lost interest, and paralleling RAND activities, that some of the most critical Game Theory results, and the backbones of subsequent developments, were created.

The most outstanding example is the concept of Nash Equilibrium, the topic of John Nash's Ph.D. Dissertation in Mathematics (1950). Among the people affiliated with Princeton's mathematics department and who offered pivotal contributions to Game Theory (and not only), we found such scholars as Albert Tucker, Harold Kuhn, Lloyd Shapley, Nash, Sam Karlin, and David Gale.

Martin Shubik offered a first-person account of how Princeton was Game Theory's birthplace in the 1950s. Despite his enrolment as a graduate student in Economics, Shubik grew very close to Nash and Shapley. (Shubik 1992) Thus, as I will mention again, he was one of the few economists in the 1950s who showed a sheer interest in the Theory of Games and were keen to employ it (see below). In his account, Shubik highlighted the differences he perceived between the department of Economics, to which he, as a graduate student, was affiliated, and that of mathematics. In the former, despite the presence of renowned theorists like William J. Baumol and Jacob Viner and a favorable attitude toward mathematical Economics (especially Baumol), Game Theory was of extremely little interest. As remarked by Shubik:

> "[...] [G]ame theory apparently had little impact on the economics department. William Baumol raised questions about the value of the measurable utility assumption used in much game theory work at that time; outside of Princeton Karl Kaysen had questioned the worth of game theory in economics. The view was that in spite of favorable reviews of Leonid Hurwicz

matical Economics like Paul Samuelson's MIT Economics Department (Backhouse 2017; citeweintraub2014introduction).

18 This statement is not an undermining of the importance of the Institute for historical studies and social sciences. For instance, among the members in these fields, we find, since the second postwar, such people like the medieval historian Ernst Kantorowicz, the art historian Erwin Panofsky, the economist, and historian Albert O. Hirschmann, the international relations scholar and former ambassador George Kennan, among the others.

> and others, this new mathematical bag of tricks was of little relevance to economics.[19] This view was put forward in particular by Jacob Viner, whose favorite comment on the subject was that if game theory could not even solve the game of chess, how could it be of use in the study of economic life, which is considerably more complex than chess." (Shubik 1992, p. 152)

In general, the attitude of people like Viner toward Game Theory was similar to that of other aging scholars (Viner was born in 1892), even if not necessarily hostile toward the mathematization of Economics. However, in Shubik's account, the difference was not limited to Game Theory but embedded in a more general attitude the two departments displayed. Thus:

> "The contrast of attitude between the economics department and the mathematics department was stamped on my mind soon after arriving at Princeton.[...] The contrast [...] at that time has some lessons to teach. Besides Morgenstern, there were some fine scholars in economics such as Viner and Baumol, but there was no challenge or apparent interest in the frontier of the science. Morgenstern was to some extent an inconvenience. To me, the striking thing at that time was not that the mathematics department welcomed game theory with open arms - but that it was open to new ideas and new talent from any source, and it could convey to all a sense of challenge and a belief that much new and worthwhile was happening." (Shubik 1992, pp. 61–2)

To conclude, those mentioned above (namely, the Cowles Commission, RAND Corporation, and Princeton Mathematics Department) were the most critical sites (although not the only ones) within the dramatic changes in economic theory briefly presented in the first paragraph, took place. However, as seen, such developments were not the final steps but the first in the disciplinary transformations of Economics. A further move was needed: the effective conquest of the discipline and the broad adoption by every or (at least) the vast majority of scholars, students, and instructors.

It happened successfully in economics. Different was the case of other social sciences. There, the entry of quantitative analysis and statistical methods was quite successful and followed a path similar to that of Economics, although in different terms, at least in the second stage, thanks to the role of SSRC and the board for mathematical social sciences. However, a "revolution" even in the theory-building comparable to that offered by Postwar Neo-classical Microeconomics (namely General Economic Equilibrium in axiomatic fashion) and Game Theory did not occur. Perhaps the closest attempt, but far from being so successful as economic theory, was that of Riker.

19 An example is Schumpeter's attitude, as reported in Giocoli, 2003, p. 355. It must be recalled that Schumpeter was among the founders of the Econometric Society, and his view toward mathematical economics (letting a part his effective managing of it) was extremely positive, up to the point that he exercised a great influence on young Samuelson, where the latter was Harvard Graduate student in economics, in the late 1930s. (Backhouse, 2018; Swedberg, 1991)

2.2 THE DEVELOPMENT OF POLITICAL SCIENCE

2.2.1 *Early debates on Political Science*

POLITICAL SCIENCE EMERGED in the late XIXth and early XXth century as a specialized field of study related to, but distinct from, disciplines like constitutional law, History, and political philosophy. In particular, the link with the study of History was intense from a disciplinary point of view and from an institutional one. For example, the "American Political Science Association" emerged from the "American Historical Association." Besides, until the 1930s, the affiliation between History and Political Science teaching was intense. This affiliation lasted up to the second postwar in countries other than the United States. (Farr 2009)

Methodologically, History was considered, up to the 1930s and 1940s, the principal field for observing and testing political conceptions. The obvious consequence was that these were inseparable from normative and prescriptive implications. Although anecdotal, proof is to be found in the wide success (both in America and England) of the oxonian historian Edward A. Freeman's famous aphorism: "History is past Politics, and Politics is present History." This sentence became the motto for the Johns Hopkins University Studies in Historical and Political Science. (Farr 2009, p. 68; Ross 2009).[20] This intertwining of political and historical analysis reflected the fact that the earlier was deeply connected with liberal politics, given its link with the narratives of the historical development of modern polities, often in terms of evolutionary theories, where "whiggism" (yet in the political and historical sense) was the central tenet. (Ross 2009) It also means that the discipline's institutional trajectory influenced its intellectual elaboration, showing the tension between normative and positive theory. (Adcock and Bevir 2006)

In this section's purposely simplified narrative, some (very general and often intertwined) dichotomies can be advanced to better delineate the development of "pre-behavioralist" political science. These are the (apparent) opposition between historical and empirical research, normative and positive theory, and finally, the proper object of investigation, Statism or Pluralism. All of them are very general and often profoundly intertwined.

The first dichotomy is not an opposition. In fact, through empiricism, one does not have to think (exclusively) of sophisticated discussions about the philosophy of science but instead of a more general plea for the "scientific method." Despite the dominant historicism, political science was not devoid of methodological discussion about its proper scientific structure, namely the meaning of "science" concerning political issues. The formation of Political Science was indeed perceived by its early proponents (like Lord Bryce, Lawrence Lowell, and others), both in the United States and Great Britain, as a "scientific" project, where the new model was that of natural sciences. Accordingly, among the main issues, one finds such things as the impartiality of the scholar, at least in a significantly broader sense (which does not contradict the normative implication of the majority of works) and "method." The latter entailed the application of the empirical

20 Another anecdote is in the reminiscences of the political scientist Charles Hyneman, whose mentor at Indiana University in the 1920s, Frank G. Bates, used to say that a political scientist is a middle ground between a poor lawyer and a poor historian. (Baer, Jewell, and Sigelman 1991, p. 9

method elaborated by John Stuart Mill in his *System of Logic* (1843) to moral sciences and the Rankean historical method based on archival research.

The role occupied by more sophisticated versions of positivism was minor, although not insignificant. There were discussions about the proper way to obtain impartiality and objectivity through quantitative methods and statistical tools. At the beginning of the XXth century, the rising employment of these techniques, albeit still extremely elementary, became the distinctive mark of American political science. However, there were some significant exceptions, like that of the British scholar Graham Wallas, a professor at the London School of Economics and Political Science.[21]

Discussing the method of political reasoning, Wallas explicitly opposed the apriori deductive methods based on "personifications and uniformities" of political abstractions and species and "large and untried generalizations" to the employment of quantitative analysis. (Wallas 1920, pp. 138–9) The case of Wallas is interesting. In his most important work, *Human Nature in Politics* (1908), he developed an argument against the "intellectualist fallacy" of adopting apriori deductive methods, which overstated the power of human reason and downplayed psychological features like that of the "impulses and instincts." His argument rested on psychological considerations (mainly on William James and the theorists of the "Psychologie de la foule" like Gabriel Tarde and Gustave Le Bon) and evolutionary arguments (following Darwin).

Although the analysis is often anecdotal (also given the similar interest of Wallas for active political engagement), the bulk of his argument is quite systematic. To the long-lasting attitude in the political analysis of finding "[...] 'standard'... facts about man which should bear the same relation to politics which the fact that all things can be weighted bears to physics, and the fact that all things can be measured bears to geometry" (Wallas 1920, pp. 120–1) he opposed a different method, based on "finding as many relevant and measurable facts about human nature as possible," to make them serviceable in political reasoning.

This method, explicitly compared to that of the biologist, is threefold: One is the description of these facts; the second is their quantitative analysis; finally, there is the analysis, both descriptive and quantitative, of the environment into which men are born and its effect on their behavior. In Wallas' view, descriptive analysis is still inseparable from psychological considerations. For this reason, he criticized some works like that of Moises Ostrogorski and Lord Bryce because both the authors, according to him, tried to adapt their observations of political reality in the U.S. and G.B. to their preferred conception of "ideal political men." Up to the point that, in Ostrogorski's work, "one seems to be reading a series of conscientious observations of the Copernican heavens by a loyal but saddened believer in the Ptolemaic astronomy." (Wallas 1920, p. 125)

The role of quantitative methods is ascertained through association with some recent developments in "Mathematical Biology," namely Biometrics, following the contributions of Karl Pearson to statistical analysis (p. 132) and with the recent development of Economics. For the latter case, Wallas attributed fundamental importance "to the passage from the abstract deductions about ideal economic men" (which he associated with classical political economy, his reference being

21 Wallas (1858-1932) was an English scholar, member of the Fabian Society, and among the founders, together with Sidney and Beatrice Webb and the playwright George Bernard Shaw, of the LSE, in 1895. Qualter 1980; Howson 2011

Walter Bagehot), to the method based "upon the variety and not the uniformity of individual instances" namely that of William Stanley Jevons and Alfred Marshall. Jevons, in particular, to him "[...] arranged the hours of labour in a working day, or the units of satisfaction from spending money, on curves of increase and decrease, and employs mathematical methods to indicate the point where one curve, whether representing an imaginary estimate or a record of ascertained facts, would cut the others to the best advantage." (Wallas 1920, pp. 141–2) It is no more an abstract method because, in his view, it corresponds to the process by which real people arrive at practical results. Furthermore, this approach can be easily extended by employing statistical analysis.

Wallas' discussion about Economics could seem contradictory with respect to his previous argument about the psychological aspects of human nature. Indeed Neo-classical Economics is often identified with excessive apriorism, abstracticism, and deductivism, about individual behavior. However, Wallas emphasized the exact quantitative relationship among political issues of any different kind (from public spending to legislative politics). Therefore, although his discussion on these issues is framed in a clear marginalist fashion, it shows only, in my view, his definite adoption of a "psychological" interpretation of the latter, as common among his contemporary British economists.[22] For the same reasons, it would be misleading to see Wallas as a possible precursor of any "economic approach" in political science. Notwithstanding, his analysis entails many distinctive elements of the subsequent development of political science. Thus, despite being largely unsuccessful in Britain, his quantitative and "psychological approach" will be one of the trademarks of proto-behavioralist analysis (Merriam 1923; Adcock and Bevir 2006).

The discussions on methodological developments also embodied those about its proper positive or normative content. It shows that Political Science, intended as a scientific understanding of politics and society, was also seen as a fundamental tool for defending the current State of affairs (for instance, the XIXth century British liberal institutions) or advancing reform programs. It was common for many scholars to be also politically involved (this holds for people like Bryce, Ostrogorski, Wallas, and Merriam, among the others), and despite the classical Weberian analysis of the "vocational" approach to science arrived among the American scientific community only in the second postwar, such issues like the impartiality of the scholar were yet widely discussed, as well as the right balance between political engagement and political analysis.[23]

For example, in a review of the recent advances in political methods, Charles Merriam, the founder of the so-called "Chicago School of Political Science" (see below), explicitly distinguished between two different attitudes: "practical political wisdom or prudence exhibited by men of the type of Hamilton, Madison, Adams and Jefferson; and on the juristic side by such masters as Marshall, Story, Webster, and Calhoun" and "systematic study of government" which only began with the works of Francis Lieber. The first is practiced by political actors, and the

22 for a comprehensive historical discussion about the different interpretations of marginal utility theories, and their relations with psychology, Moscati 2018

23 James Bryce (1838-1922) was a member of the English parliament for the Liberal Party, Ambassador to the United States, and occupied important cabinet roles; Moises Ostrogorski (1854-1921) was elected in the first Duma of the Russian Empire, as a member of Constitutional Democrat (Cadets) party; Charles Merriam tried, unsuccessfully, to be elected as Major of Chicago for the Republican Party in 1911. On the reception of Weber scholarship among American Scholars, see: Scaff 2011)

second by scholars. Sometimes, the same person can enact these but must remain well separated (Merriam 1923).

An ulterior insight into the early State of the discipline, its methodological concerns, and the main issues it dealt with can be offered by Lawrence Lowell's presidential address to the "American Political Science Association" in 1910. (Lowell 1910)[24] To him, political studies suffered mainly from the absence of precision and the "imperfect development of the means of self-expansion." Then, although the different aspects of the study of politics and society can be ascertained precisely, their fundamental importance is often neglected. This aspect holds particularly for Political Science, which Lowell defined as the "physiology of politics," i.e., the study of the actual working of the government. Thus, discussions about "living topics as proportional representation, the referendum and initiative, and the reform of municipal government [...] are for the most part conducted in the air. They are theoretical treating mainly of what ought to happen, rather than what actually occurs; and even when they consented to deal with facts is usually on a limited scale with very superficial attention to the conditions under which the facts took place." (Lowell 1910, p. 3) However, Lowell does not exclude a normative purpose, too, namely supporting reform movements. Then, to him, only the scientific study of politics can offer the knowledge to be effective in political action.

> "[...] (t)he ultimate object of political science is moral, that is the improvement of government among men. But the investigator must study it as a science, as a series of phenomena of which he is seeking to discover the causes and effects. He must not set out with prejudice for or against a particular institutions, or, indeed, regard politics from an immediate moral standpoint; for if he does he will almost inevitably be subject to a bias likely to vitiate his observation.[...] It is our province to discover the principles that govern the political relations of mankind and to teach those principles to the men who will be in a position to give effect to them hereafter".(Lowell 1910, pp. 4–5)

Regarding the object of analysis, in the years before First World War and the interwar years, a central role in American Political Science was occupied by two different schools or "tendencies," namely "Statism" and "Pluralism." Even in this case, we do not face a rigid separation or an exclusive political analysis framing. Some topics, like the functioning of political parties, electoral mechanisms, voting procedures, and so on, were yet partially explored (an exhaustive list of problems to address in political science can be found in: Lowell 1910, pp. 11–3) But the difference between "Statism" and "Pluralism" reflects, on the one hand, the ascendancy of political science from Constitutional law, and on the other, the growing mingling with affine social sciences. In this sense, neither Statism nor Pluralism was truly intellectual movements, but mainly intellectual tendencies, as they had not a real manifesto but at most several different ones, although with shared aims and even a common political agenda.

Statism is strictly related to the concept of State derived from European judicial and political philosophy (mainly from German *Staatwissenschaft*). During the so-called "Progressive era," this concept had been explored in American political tradition, both by historical analysis and empirical classification, often with a

24 Lowell (1856-1943) was a scholar of comparative politics whose main famous work is *The Government of England* (1908). He also became president of Harvard University

radically normative aim of helping establish a reason-based State and political community. Occasionally, political science allied itself with the progressive movement on topics such as race, immigration, women's labor, eugenics, and others. (T. C. Leonard 2016).

In contrast to what was perceived as the monism of Statism, some authors (like Arthur Bentley and Charles Beard) started to recognize the importance of different groups and organizations, often with divergent interests. The presence of these groups rendered the State, in reality, a plural polity. With the emergence of pluralist theories, the ditch between normative and positive theories assumed new features. For instance, some authors like British Harold Laski (before his conversion to marxism in the 1930s) saw pluralist analysis as a way to defend and justify the existence of these differences against the unifying aspiration of the modern State.[25]

Besides, the development of such debates surrounding the pluralistic nature of contemporary societies stimulated some authors (despite maintaining a normative role for the State, namely that of being the locus of liberal democracy and the vehicle for the solution of social problems) to develop proper methods and analyses to study group politics.

The most influential role was that of the so-called "Chicago School of Political Science," which formed, from the 1920s up to the 1950s, around the pivotal role of Charles Merriam. The importance of Merriam in creating the "Chicago School" and shaping the disciplinary development of political science cannot be downplayed. In the 1920s, his contributions entailed a series of methodological as well as historical discussions on the State of the discipline, the new course impressed at Chicago Political Science Department, and finally, the establishment of institutions and scholars' associations, like the "Social Science Research Council," of which Merriam was the first chairman (1923), or the first National Conference of Science of Politics (Merriam 1923). In 1926, Merriam also became the American Political Science Association president.

At Chicago, a group of scholars, under the influence of Merriam, produced seminal studies about voting behavior, African American politics, political psychology, urban politics, comparative politics, political parties, methodology, and other topics. (Heaney and Hansen 2006) These researches employed advanced empirical methods, like survey experiments, content analysis, field experiments, factor analysis, and an innovative quantitative and qualitative analysis mixture. Chicago graduates were among the forerunners of the Behavioral Revolution after the Second World War (among them V.O. Key Jr, Harold D. Lasswell, Gabriel Almond, Herbert Simon, and David B. Truman).

The particularity of the Chicago approach to political science can be better ascertained by the presence of a recurring theme in the oral histories of many influential American political scientists, namely the contrast between Chicago and Harvard's different approaches to the scientific study of politics. (Baer, Jewell, and Sigelman 1991) These two were not the only political science Graduate Programs in the United States during the interwar period. However, in many scholars' subsequent perceptions of the History of the discipline, they became the two examples of different attitudes in the analysis of political phenomena.

25 On Laski's "normative pluralism" influence in American scholarship, see: (Dryzek 2006; J. G. Gunnell 1995)

On the one hand, the Chicago Social Science Department, with Merriam but also people like Harold Lasswell, Leonard White, and Harold Gosnell, all his academic pupils, on the other Harvard School of Government, identified with a more historical and theoretical commitment, represented by such scholars like the famous historian of political thought Charles McIlwain, and the German scholar Carl Joachim Friedrich. A lucid reminiscence, for instance, is that offered by David Easton, who obtained his Ph.D. at Harvard in the 1940s but later received his first appointment at Chicago. In his words, speaking of Harvard, "it is difficult today to appreciate fully how inimical the whole atmosphere in the Department of Government was, at least among many of the senior professors, to the scientific method for the study of politics and society." (Interview to D. Easton, in Baer, Jewell, and Sigelman 1991, p. 198) Then "you could not graduate from the program without being sensitive to the importance of theory in political research." (ibidem) Harvard years shaped Easton's attitude toward theory, albeit with solid empirical foundations, but at the same time, did not contribute, in his perception, to his training as a political scientist. Up to the point that Easton explicitly declared that "By the time I left Harvard, I just didn't know what political science was all about." (p. 199)

Turning to his Chicago experience, this was instead "a place where 'a tremendous emphasis was placed not only on the solid empirical ideas but on the procedures and means that were used to attain these ideals, or, in common parlance, upon methods [...] the whole atmosphere and rhetoric was one of interdisciplinary research, the sense that all the social science were indeed." (p. 201) Easton arrived in Chicago in the early 1950s, when, according to many accounts, the most original and exclusive features of the School were about to be lost after the death of Merriam and the departure of most of his students (and their replacement with scholars of totally different attitudes like Hans Morgenthau and Leo Strauss. (Heaney and Hansen 2006) Nevertheless, this loss coincides with their countrywide adoption and the Behavioral Revolution in political science, to which Merriam was undoubtedly one of the forerunners.

2.2.2 *The Behavioural Revolution*

In the 1930s and 1940s, American political scientists concluded the "epistemic shift" commenced in the late XIX-early XX century. A further step is represented in the 1950s by the genesis and the development of the "Behavioral Revolution." The growth of empirical work that distinguishes interwar American political science certainly provides a starting point for understanding the subsequent Behavioral Revolution. The transformative aspirations of Behavioralism lay in the departures it prescribed to make political science systematic. Then, political Scientist Robert Dahl defined Behaviouralism as a "protest movement" within the discipline, carried on by those political scientists who shared strong dissatisfaction with the historical and juridical approach, looking for instead more scientific analysis. (Robert A Dahl 1961) Such commitment was present yet in some authors like Wallas and the Chicago approach, and American Political Science began a dramatic shift in his disciplinary approach and methodological commitment in the 1940s. In this sense, Behaviouralism is only the final step of more complex development, as shown in the previous section. However, it for sure entails some innovative aspects, up to the point that some of the discipline's

practitioners described it as an example of "Kuhnian revolution." (Truman 1965; Gabriel A Almond 1966). Although this view has been contended by subsequent and coeval practitioners, other than historians of political science, it still remains the fact that the leading proponents of this new approach to political analysis were inclined to highlight the difference with others and to define themselves in revolutionary terms.[26]

In the following pages, I do not want to describe or contend with all these interpretations accurately but instead point out some of the main features of Behaviouralism, resting mainly on how its practitioners perceived this. The focus will be on the role of theory in behavioralism, even discussing the content of Easton's *The Political System* (1953) briefly, a pivotal work that attempted to bridge political theory with what was becoming political science. (Easton 1953)

In a famous paper about the genesis and characteristics of Behaviouralism, Dahl famously pointed out one of the most important features of it, namely the lacking a precise definition: "[t]he behavioral approach [...] is rather like the Loch Ness monster: one can say with considerable confidence what it is not, but it is difficult to say what it is." (Robert A Dahl 1961, p. 249) Furthermore, according to another behavioralist political scientist, David Truman: "[...] it's a mistake to over standardize the definition of what [behavioral revolution] was. It was a kind of multifaceted expression of dissatisfaction with the constraint and the formalities of the conventional political science, which we had inherited." (Baer, Jewell, and Sigelman 1991, p. 148) Thus, to Dahl, Behaviouralism can be seen simply as "[...] an attempt to improve our understanding of politics by seeking to explain the empirical aspects of political life using theories and criteria of proof that are acceptable according to the canons, conventions, and assumptions of modern empirical science." (Robert A Dahl 1961, p. 256)

It seems evident that this definition is too broad, comprising almost every attempt to understand politics using empirical theories and approaches. Besides, Dahl continues: "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." (Robert A Dahl 1961, p. 255) So, he concluded that " [...] 'the behavioral approach' might better be called the 'behavioural mood' or perhaps even the 'scientific outlook.'"(p. 258)

According to Adcock, what is now called the "behavioral movement" took shape as a loose grouping of scholars committed to disciplinary transformation and sharing, in broad outline, a shared vision of new political science. Their vision stood in contrast to an alternative vision of the discipline articulated by such scholars (often European by origin) as Hans Morgenthau, Leo Strauss, or Eric Voegelin. But also, on the other hand, to those political scientists who found themselves satisfied with the existing discipline. (Adcock 2009, p. 188)

Dahl outlined six factors to explain the behavioral approach in American political science. All are the outcome of specific attitudes in American culture. First, Merriam's role and the influence of European emigration. Second, the Second World War pushed many scholars to participate in administrative and planning works and institutions. A third and even more considerable impetus, still related to the war, was provided by the "Social Science Research Council,"

26 See for instance Heinz Eulau's interview in Baer, Jewell, and Sigelman 1991, 178 et ss.

whose presidency was assumed by E. Pendleton Herring, a political scientist. In particular, Herring helped create an SSRC Committee on political behavior.[27]. Such a committee focused on individuals' behavior in political situations by examining the political relationship among individuals to formulate and test hypotheses concerning uniformities of behavior in different institutional settings. Eventually, the other factors were: the 1949 first conference of political behavior, which interlinked various groups of scholars committed to these issues; the rapid growth of the survey methods and statistics, which became increasingly more "scientific"; finally, the influence of some institutions and private foundations like Carnegie, Rockefeller, and Ford.

Political behavior became the main object of research in the 1950s. Political scientists' attention to this aspect pre-dated that decade but only in those years assumed a central role, also thanks to the definitive establishment of quantitative analytical techniques.[28] Then, Behaviouralism may be assumed as a comprehensive approach that comprises a research focus on political behavior, a methodological plea for science, a political message about liberal pluralism, and the organizing concept of a political system. Behavioralists emphasized individuals and groups and, according to some historians, could be well defined as a group, with a proper 'manifesto' and even founding texts, like Easton's *The Political System*. (Easton 1953; Garceau 1951) However, as Easton put it, it did not represent a clearly defined movement for those who allegedly were "Behavioralists." As he continued: "[...] It was more clearly definable by those who were opposed to it because they were describing it in terms of the things within the newer trends that they found objectionable. So some would define behavioralism as an attempt to apply the methods of natural sciences to human behaviour. Others would define it as an excessive emphasis upon quantification. Others as individualistic reductionism." (Baer, Jewell, and Sigelman 1991, p. 207)

Despite this, it was familiar to all its proponents the vision of Behaviouralism as a transformative movement, a "rallying cry to promote change." Therefore, this view was first employed not to capture an already accomplished intellectual shift but to promote change. This latter point was explicitly recognized by Riker, who, in the 1950s, never joined the movement, still sharing many aspects of the protest. (Riker 1997. On Riker, see the next chapters)

In one of the first "post-behavioral" historical reconstructions of the development of Political Science, Albert Somit and Joseph Tanenhaus listed a series of features that represented the "key behavioralists articles of faith." (Somit and Tanenhaus 1967, pp. 177–9) These entail the emphasis on prediction and explanation of political issues, based on observation and data collection, the development of interdisciplinarity 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. Moreover, theoretical development occupies an important role in orienting and directing research. The behavioralists believed that systematic science depends on the cumulative interplay between theoretical innovation and empirical research, and they set out to remake both sides of this interplay. Therefore, according to Robert Adcock, there were two strands

27 See below

28 Up to the point that in a "state of the discipline" volume put together by the APSA in the 1940s it was affirmed that political behavior had largely replaced legal structures as the cardinal point of emphasis among political scientists (Griffith 1948).

in the behavioralist agenda for a disciplinary change in political science. One encompasses pursuing an "agenda of empirical theory," where empirical validation is embedded within theoretical development. Easton's work is perhaps the most important example of this agenda. To this group also belongs important works like Lasswell and Abraham Kaplan's *Power and Society* and Truman's *The Governmental Process*. (Lasswell and A. Kaplan 1950; Truman 1951)

The second strand instead focused on using more sophisticated empirical research techniques. It involved a passage from "low key" empiricism, with no preference for quantification and statistical analysis, to a more sophisticated one. It transformed the conception of what it meant to be scientific in political science, namely "being systematic." This second strand was more successful than the commitment to pure and cumulative political theory. Indeed, it comprises methods and analytical tools, often borrowed from other disciplines, like sociology and psychology, for instance, survey research in studies of voting behavior and public opinion. (Robert A Dahl 2005; Campbell et al. 1980).

As stated before, the Behavioral movement is central to how contemporary political scientists envision their discipline's past. For Adcock, Behavioralism was revolutionary in its character and impact, but some changes were not immediate nor radical as often simplified. For instance, Dahl's emphasis on the empirical character of Behavioralism could be misleading. Indeed, it is severed by recognizing the importance of theoretical analysis and the fact that empirical research was done even before the 1950s. Besides, the focus was more on the systematic study of politics and how to reach this systematization, than the object of study. Therefore, in this latter sense, the transformative aspects of Behavioralism lay in the call for a new kind of theoretical work and more sophisticated empirical techniques. Then, the revolutionary character of Behavioralism reduces to employing different and advanced research techniques. (Adcock 2009). However, this association of Behavioral Revolution with technical developments in the discipline can obscure the more complicated impact of Behaviouralism on more general political theory, especially since the transformative effects were far from successful or memorable. Behavioralism's conception of political theory rested on using "self-conscious" abstraction to produce analytical frameworks and generate real scientific progress. In this sense, the commitment to empirical research is essential because this is the key to cumulative progress in social sciences. But unfortunately, the results were disappointing because no general fundamental theory was discovered; neither a unified methodological and theoretical framework was accepted among the vast majority of the discipline's practitioners and theoreticians.

2.2.3 *Political Theory and Political Science*

DAVID EASTON'S *The Political System* (1953) has been seen by some as the "manifesto" for the new intellectual tendencies toward exact reasoning and empirical analysis in 1950s political science. (Dryzek 2006) However, behavioralism does not occupy a broad space in this work. Instead, more than offering a new comprehensive political theory or presenting new methods for political analysis, Easton discussed in detail "the state of political science." His aim was "to define the terms of the dispute to show the behavioralists that there was an important theoretical component that they were missing and, at the same time, to

show the anti-behavioralists that to be a behavioralist did not necessarily exclude an understanding of the importance of values or moral discourse." (Interview to Easton, in Baer, Jewell, and Sigelman 1991)

Consequently, his analysis is filled with discussions about the History of the development of Political Science and the attitude of political scholars about the scientific debate. For instance, one of the most famous chapters of his work addresses "the decline of modern political theory." (Easton 1953, 233 et ss.) Easton attributed this decline to historicism, by which he intended the almost exclusive interest for the History of political doctrines that characterized American political theory (mainly the works of Dunning, McIlwain, and George Sabine).[29]

Consequently, the "value aspects" of Political Theory and its empirical nature are overshadowed by historical considerations about political ideas, with the result that this kind of political theory is scarcely able to provide any valuable understanding of political realities. Establishing the nature of the relationship between "values aspects" of the theory and their empirical basis lies at the core of Easton's 1953 work and the subsequent developments of his analysis.

Easton started by acknowledging the disappointment of political science, ("a discipline already twenty-five hundreds of years old" [sic]) due to its failure to clarify the actual relationship between facts and political theory, as well as the "vital role in this partnership." (Easton 1953, p. 4) Therefore, "the search for reliable knowledge about political phenomena requires ultimately the construction of a systematic theory, the name for the highest order of generalization." (ibidem) Yet from the very first lines of his work, it is evident that Easton's attitude toward theoretical arguments in politics is all but unfavorable.

His concerns about the theoretical developments in political science were originated not from a possible "empirical derive" of social science over theorizing but instead by "the growing disillusionment about the whole of scientific reasoning as a way of helping us to understand social problems." (Easton 1953, p. 5) He explicitly defined it as a "flight from scientific reason."(p. 6) Then, in American political science: "[...] We have, in consequence, the peculiar condition among the members of this discipline of nominal acceptance of their role as scientists, with the rejection in the practice of the recognized logic and techniques of the scientific method. Today this historical reluctance to commit themselves to a scientific approach to social knowledge shows few signs of decreasing: indeed, because of the present intellectual mood in Western society as a whole, it is actually growing." (Ibidem)

Easton identified two different but intertwined ways this anti-scientific attitude revealed itself. The first was a movement away from the rational attitude towards life and toward feelings like emotion, faith, or tradition; the second, instead, was an increasing attitude to develop critical arguments against scientific methods. He associated the first attitude with such thinkers as the English writer and political theorist Michael Oakeshott. According to the latter, any rational approach to the understanding of politics separated from the "act of politics" was useless because reason was not only a set of tools or techniques but also encapsulated the "greater wisdom of prejudice, tradition and accumulated experience knowable largely through history." (Easton 1953, p. 19).

From these views originated the tendency to discuss the crisis in the methods in social sciences and to blame scientific attitude for the failures of understanding

29 This chapter is a reprise of a previous Easton's paper. Easton 1951

the political facts of the time. Unlike the repeal of reason in the name of faith and tradition, these discussions' focus was the failure to apply the scientific method in social sciences. Indeed, despite the increasing efforts made by many scholars to define the scientific reasoning in political sciences, many other social scientists and philosophers remained skeptical of the social sciences, their methods, and their scope.[30] However, Easton did not reject this kind of criticism but instead rejected their most radical conclusion, namely the impossibility of a reason-based analysis in Political Science.

To him, the development of Political Science as a discipline has "misconstrued the nature of the tools required for the attainment of reliable, generalized knowledge," preferring the "accumulation of facts and the premature application of this information to practical situations" to the development of verifiable theories. (p. 37) A conception he famously labeled as "hyperfactualism." (pp. 66 et ss.) Namely, the accumulation of historical and "empirical facts" about politics without systematic generalization. Moreover, "[...] in becoming preoccupied almost exclusively with problems of applying this factual information, political science has impeded its movement towards a fundamental understanding of political life, a kind of knowledge that would place the relation of means to ends on a secure foundation." (p. 89)

One of the most troubling aspects of the development of Political Science is the imprecision in defining even the most critical concepts of political analysis, and the result is the impossibility for a scholar to judge adequately between conflicting statements. To overcome this problem, the scholar must point to the "[...] gradual creation of a new meaningful vocabulary, to be distinguished from artificial and unnecessary jargon, the refinement of current concept, and the development of special techniques for observing and reporting data, collating and testing them." (p. 46) An explicit conception of scientific verifiability and the possibility of developing a cumulative knowledge,

This "empirical" stance does not undermine the importance of theoretical orientation for political analysis. By "theory," Easton meant not a normative judgment about values, as customary in political philosophy (which he defined as "value theory"), but instead the attempts to find relations between different political facts. In this latter case, he spoke of "causal theory." (pp. 52 et ss.)

These are not opposed, but one is involved in the other, and the distinction between them is only a matter of inner logic. "Causal theory" could be seen as a proxy for the stage of development of any science. Indeed, it brings the possibility of cumulative knowledge due to the growth of insights about the relationship between different facts. However, given the immense complexities of human and social matters, defining a "fact" involves theoretical reasoning. Thus, facts cannot be separated from theoretical assumptions about their definition: "A fact is a particular ordering of reality in terms of a theoretical interest." (p. 53) This point is interesting because Easton's interpretation follows the same lines of those scholars who denied the "distinction between facts and values," and thus the possibility of a purely objective social science, like that of Leo Strauss and his followers. Indeed, Easton's original position is not that of rejecting in toto the critiques of political science as a scientific effort but of defending it from the most damaging consequences of this negation instead.

30 among the earlier group, Easton inserted scholars like George Caitlin, Harold Lasswell, Abraham Kaplan, and Herbert Simon.Easton 1953, p. 22

The development of political theory lies at the core of the establishment of Political Science because the "pure technical refinements," like, for instance, the implementation of collecting data and the development of statistics, are insufficient. "Political science today is confronted with the need to recognize that scientific understanding of political life is ultimately possible only by clarifying the broad theoretical premises of research." (p. 63)

Easton defined three different ranges of problems confronting theoretical research: the basic concepts needed to orient political research, the categories of data that must be considered, and finally, the role of value judgment in formulating a theory. By "orienting concepts," he meant the concepts that permitted us to distinguish between political science and other social sciences (from economics, sociology, or psychology). Perhaps the most significant contribution of his volume belongs to this category, namely the idea of a "political system." (pp. 96 et ss.)[31] Under the label of "political system," he comprised "all these kinds of activities involved in the formulation and execution of a social policy [...] the policymaking process...constitutes the political system." (Easton 1953, p. 129) This entails an explicit rejection of all the previous theories concerning too simplistic conceptions of 'State"or "power." Related to this is his famous definition of 'politics' as "the authoritative allocation of values for a society." (pp. 29 et ss.). It represents a "convenient and rough approximation to a set of orienting concepts" for political analysis. Moreover, in Easton's pages, such a definition paves the way to the problem of addressing, and therefore establishing, precise meaning to concepts like "policy," "authority," and "society."

The construction of theory also involves discussing the different types of data that must be examined. In the development of American political science, Easton identified two different approaches to this problem: some authors emphasized institutional aspects, others on psychological issues. Moreover, in Easton's view, the "institutionalist group" is further divided between scholars who focused on "governmental institutions" aspects and those who focused instead on non-governmental groups. Instead, the "behavioral" approach focuses on "political behavior" and represents the genuine novel approach to developing political science as a discipline.

Finally, Easton's third problem is the "moral foundations of theoretical research," namely, separating objective facts and judgments. To him, the impossibility of political research free from involvement with values calls for a full exploration of the "moral premises" that lies at the core of any scientific effort. It applies particularly to Political Science because its inspiration is ethical: "Men want to understand the political system so that they can use this knowledge for their own purposes." (Easton 1953, p. 223) In this sense, the first part of the XXth century and the second postwar years have witnessed a firm rejection of the positivist faith in the possibility of the total moral neutrality of scientific enterprise. But to the author, "the mere statement [...] that values underlie all research does not in itself lead to the inevitable conclusion that these values must, by virtue of their presence, influence this research." (p. 225)

This influence is insufficient for appraising the value of scientific research, which instead relies only on this correspondence with reality. However, even if the relation between facts and values is inextricable, the task of social scientists

31 This idea (with modifications) will become the backbone of the subsequent development of Easton's political science, but the premises are contained in this work. (Easton 1965)

must be that of improving the reliability of our knowledge by making the values underlying it explicit, as a sort of "moral prelude to our main empirical theme." (p. 228) It needs what he defined as the "attainment of moral clarity," which in turn "requires training and experience in the concepts and procedures of moral inquiry, the kind of analysis we usually associate in political science with the study of strict political (value) theory." (ibidem) This moral clarity is related not only to the formal postulation of corresponding values but moreover to the "positive task of constructing an image of the political system flowing from these moral premises." (Easton 1953, p. 231) The latter is an approach that contrasts the appeal to make explicit the researcher's values. Instead, it aims to offer a constructive synthesis of values with facts to reveal their whole meaning.

The only discernible suggestion of a theoretical framework on the broad horizon of empirical research is, according to Easton, the "theory of political equilibrium." Through this idea, he intended two different modes of analysis: first, a way to understand the process within a political system (this is defined as "general equilibrium"); second, a way to offer a description of the system ("constitutional equilibrium"). Because, in his view, such general equilibrium entailed both normative and descriptive issues, the main difference with constitutional equilibrium is that the second could be defined more appropriately as an equilibrium determined by institutions, where the first is an equilibrium between different political parties and actors. In Easton's words, therefore, "constitutional equilibrium [...] deserves those necessary conditions for the existence of a constitutional order within a nation and peaceful relations among nations." (Easton 1953, p. 293)

General equilibrium instead entails two distinct ideas, the interdependency of all the elements in a political system (an idea subsumed in the concept of "political system") and the tendency to act and react to each other up to reach a point of stability. But if the first idea is not an absolute novelty (to Easton, its tracks in political science can be found in the growth of "pluralism," with its recognition of the multiplicity of social forces, especially social groups), the second is implicitly aimed to the development of far-reaching conclusion in political analysis, that is normative conclusions about the outcomes within a given political system.

The main difficulty with these concepts rests in the intertwining of the theory's normative and descriptive premises and aspirations. Thus, from a descriptive point of view, the issues are the possibilities to develop the general equilibrium concept into a full-fledged conceptual framework for political research, as it is, for example, in economics (this despite Easton recognizing that in economics, this concept was used as an analytical tool rather than a "substantive description" of the system) (Easton 1953, p. 274) An adequate model of "general equilibrium" would require the development of consistent quantitative analysis as well as "mental operations" in order to reconstruct the possible relations among interdependent political variables (exactly like economics).

These problems notwithstanding, "the idea of a general equilibrium implicit in so much empirical work in political science [...] can help to perpetuate the notion that political activity is part of an empirical system and a process of change through time. These are insights which future attempts at theory construction can scarcely neglect." (p. 306) Instead, the advantages of the concept of "constitutional equilibrium" lies in its being easier to employ to describe a general condition of political systems, namely their institutional arrangements.

To conclude, Easton warned that a ditch was being dug between different conceptions and approaches toward political theory, namely "political theory" and the "empirical part of political science." The way to escape this puzzle was to provide "reliable knowledge" about political life. But this attainment "[...] depends upon the development of the kind of analytical tool we call a conceptual framework. . . a general theory provides just such a set of criteria. It seeks to identify the major variables significant for understanding political life and showing their most important relations. It provides some test for determining the significance of any piece of empirical research towards an understanding of the whole of political life; the empirical investigation, in turn, contributes to the continuing task of improving the correspondence to reality of any existing theory." (pp. 317-8)

On Easton's page, there is perhaps the most critical discussion about the place of Political Theory in modern Political Science, made by scholars who, although with originality, were committed to the new empirical and quantitative developments of the discipline. This does not exhaust the treatment of the role of theory in the scientific approach of the discipline: nor from a historical point of view (for instance, there were also some critical debates regarding the development of "comparative politics." Adcock and Bevir 2006; Gabriel A Almond 1966) neither from a methodological (and philosophical point of view).[32] But Easton's work can be seen as a general mood of all behavioralist movements. The behavioralists were not strictly a-theoretical but instead favored developing a positive political theory. The main difference with respect to political theorists *à la* Strauss was that they accepted empirical methods. Moreover, the difference between behavioralists and those authors (starting from Riker) who took over the label "Positive Political Theory" lies mainly in their commitment to different tools for political theory and analysis.

2.2.4 *The SSRC and the mathematical social sciences in the second postwar*

As seen, some scholars, most notably Dahl, attributed much importance for the development of second postwar American Political science to the "Social Science Research Council,", especially to the executive role of political scientist Pendleton E. Herring, who became its chairman in 1948. (Baer, Jewell, and Sigelman 1991)

The SSRC was established in 1923 by a mixed group of academics and intellectuals, members of the American Political Science Association, The American Sociological Society, the American Economic Association and the American Historical Association, and (later) of the American Statistical Associations, the American Psychological Association, and the American Anthropological Association. The purpose of the Council was to offer an environment favorable to the development of systematic social sciences and that of favoring the cross-fertilization of such disciplines explicitly to offer new insights on present social problems. The search for systematic social science was mainly pursued by improving research methods.

32 Moreover, the attitude shaped by Easton did not last, albeit in a central place, in the subsequent disciplinary development, wherein, the term "political theory" came to be identified with such strictly philosophical approaches like that of Strauss (and his followers) or Sheldon Wolin.

This aspiration was intertwined with Merriam's and Chicagoan Political Science (as seen above, Merriam was one of the founders of the SSRC). As explicitly stated by such people as the economist Wesley C. Mitchell, the SSRC defended a "union-of-science viewpoint" against both more conservative approaches to social sciences and a rigid distinction between social sciences and natural sciences. In this sense, scholarly research should be separated by moral, social, and political goals. However, it was implicitly admitted that contingent social and political problems gave necessary inputs to social science scholar enterprises.

Second World War represented a decisive turning point in the History of American science. Indeed, during and after the war, the annual federal support for scientific research soared dramatically to 500 million dollars per year. Moreover, the second postwar saw the establishment of the National Science Foundation (NSF) as the primary funding source for science education in America.[33]

Regarding the social sciences, it is important to note that the SSRC continues the twofold focus on interdisciplinarity and methodological improvements. Still, its activities became increasingly more specialized and detailed.

The attitude toward social sciences and their role in modern society has been defended by Pendleton Herring in the first issue of the SSRC official review, *Items*, in 1947. Here, the author dealt with such vital questions as the nature of social science (sic), the responsibility of social scientists, the role of social sciences in modern life, and finally, their needed development. In his summary, Herring did not distinguish among different disciplines but maintained a general tone, speaking for the social sciences as a whole. To him, these "represent the approach to human relationship that emphasizes analysis rather than force." (Herring 1947, p. 2 italics in the text). In this sense, their importance is related to the need for a better understanding of social forces and human relationships to cope with the phenomena in the real world. Social sciences entailed, according to him, the rational approach needed to address social problems, not to supply "early solutions for such an enormous range of problems" but to "provide an approach to these problems that enables the human skill released by factual inquiry, by experimentation, and by analysis to make their contribution." (p. 3) To this aim, clarification of terminology is needed and "careful selection and rigorous training" to raise social scientists. Their work "[...]is not[...]to determine public purposes or humanistic objectives" but, at the same time, "the work of social scientists can make great contributions to the commonweal." (p. 4) Besides, in his view, the social scientist must address a range of problems of concern to both the politician and the ordinary citizen. The author also recognized one of the main risks associated with such scientific aspiration, namely "[...] the danger that social science, by perfecting manipulative skills, can be turned to anti-social purposes in the hands of unscrupulous leaders." (ibidem) Accordingly, Herring insisted on the need for reciprocal interactions and clarity between social scientists and society in general ("the public"). To this aim, he concluded his brief review by listing eight very general objectives for the "growing drive for sound scientific practice in the field of human relations." (Herring 1947, p. 6). These are: 1) the identification of the distinctive contributions of the social science;

33 The History of the relationship, often conflictual in the early years, between natural scientists and social scientists, about the proper role of the latter inside the NSF has been a matter of detailed historical reconstruction. This History also includes the internal debates among social scientists (within SSRC) on the convenience and risks of obtaining federal funding concerning the freedom of research. See Solovey 2013, 27 et ss.

2) the encouragement of the trend toward scientific inquiry; 3) the acceptance of social sciences on the part of scholars, government authorities as well as the general public; 4) the cooperation inside social sciences and among social sciences and other disciplines; 5) the focus on methodological research problems; 6) the application of social sciences to general social problems ("social engineering"); 7) the training of the social scientists; and finally, 8) the pursuit of governmental subsidies, private support, and academic recognition.

The last point is clearly connected with the debates regarding federal funding. However, all the previous points of Herring's list are instead striking examples of the attitude toward social science in American postwar, an attitude which some authors have negatively dismissed as "scientism" (one of the most outstanding examples being for sure Hayek. See for instance: Hayek 1952)

To pursue these aims, the SSRC continued along the lines established in the years after his foundation, i.e., through conferences and the organization of different sub-committees concerning specific issues (although in a multidisciplinary fashion). Two of them are important, given the scope of this research: the "Committee on Political Behaviour" and the "Committee on the Mathematical training of social scientists."

Initially, a committee focused on political behavior was appointed in 1945 and comprised, among the others, people like Herring, Charles Hyneman, and V.O. Key. However, the fully-fledged interdisciplinary "Committee on Political Behaviour"[34] was established only in December 1949, following the Conference on the same issue, at the University of Michigan, on August 27-September 2 of the same year, sponsored by SSRC.(Ranney 1974)[35] This Conference was attended by 29 social scientists, so divided: twenty political scientists, two sociologists, three anthropologists, three psychologists, and one statistician. The issue discussed spanned from methodological problems, focusing on political behavior, to more specific topics, like the role of governmental organizations, social authority, communities, political history, et cetera. Much emphasis was given to the psychological foundations of political behavior and the recent research techniques like field inquiries adopted in the newly published V.O. Key's *Southern Politics*. (Key 1949)

It is tempting to parallel the importance of such Conference with the pivotal "Conference on Activity Analysis of Production and Allocation," held the same year and sponsored by Cowles Commission (see above). However, in reality, if we examine the list of contributors and the effective role of the conferences in the practitioners' perceptions of the disciplines' development, some differences are evident. The Conference on Activity Analysis represents an outstanding "who is who" for mathematical economics. (Düppe and E Roy Weintraub 2014b) Indeed, among the contributors, we find such names as Arrow, George B. Dantzig, Robert Dorfman, David Gale, Koopmans, Harold Kuhn, Morgenstern, Samuelson, Herbert Simon, Albert W. Tucker, and Leonid Hurwicz.[36] Moreover, the Conference occupied a central role in shaping some participants' intellectual lives and also in their perception of their discipline's intellectual development (Kenneth J Arrow 1983).

34 From now on CPB

35 A report of the Conference was published on the last 1949 issue of *Items*. (Heard 1949)

36 Within this group, we found five future Nobel Prize in Economics, other than fundamental contributors, not only to economics, but also to mathematics, like Dantzig, Kuhn, and Tucker.

The situation is quite different if we look at the list of contributors to the Michigan conference. Among the participants, the only names of certain fame for the historian of the discipline are perhaps those of Truman, Key, and the quantitative sociologist Paul Lazarsfeld (other than Pendleton Herring, who was the organizer).

In my view, these differences can be adequately appraised by looking at the paths that political science and economics were pursuing. The true novelty of the Conference organized by Cowles consisted mainly in its presenting useful, and in some sense, ready-to-use, tools to promptly advance the progress of economics in a technical sense.[37] Instead, the Conference on political behavior was more embedded in a scholarly environment generally inclined to discuss the premises and the scope of political research. In this sense, the 1949 conference was neither an absolute novelty nor offered ready tools to advance scholarly research. Instead, it was more a mark of a distinctive mood inside the discipline. In other words, the two communities of scholars, the economists and the political scientists, were pursuing similar aims (namely, how to make their discipline scientific), but with different questions and, consequently, along different paths. The late 1940s economist (probably a young one), who attended the Conference, was interested in understanding the theoretical basis of activity analysis, namely its relevance to economic theory and its practical applications (how to compute solutions for specific problems). Accordingly, the Conference (and the publication of part of the paper discussed in a volume edited by Koopmans) was a striking experience to him, with little resemblance to that occurring in the Economics Departments at the time. Instead, the political scientist interested in systematic methods could have fulfilled its aspiration in places other than the Michigan conferences by confronting an intellectual sub-community that, for the vast part, did not attend the 1949 meeting.

Returning to the "Committee on Political Behaviour," this was appointed in December 1949, and its first chairmen were Key (1949-53) and Truman (1953-64). Its central concern was defined as "the development of theory and improvement in methods which are needed if social science research on the political process is to be more effective." (cit. in Ranney 1974) Among its outcomes, there was significant input toward collecting survey data on the American Presidential election, starting from the 1952 elections (and collecting data on the previous ones, up to 1824). But also the improvement of analytical methods and their adoption, by the organization of conferences and summer seminars, and finally, the focus on Comparative Politics, which, in 1954, became the subject of a new, separate committee within SSRC (the "Committee on Comparative Politics").

If the CPB was more focused on a general topic within political science, the SSRC was also interested in the training of social scientists. To this aim, in the early 1950s, much emphasis was put on mathematical education, and in 1952 was appointed the "Committee on Mathematical Training of Social Scientists."[38] The history of this Committee, as well as its purposes and activities, have been reconstructed by the Harvard statistician, Frederick Mosteller (one of its members), in an article published in 1974. (Mosteller 1974) The Committee did not limit itself

37 Perhaps the most famous product of the Conference was the first proof of the Simplex algorithm to solve linear programming problems, developed by the mathematician Dantzig.

38 Its original members were: William G. Madow (mathematician), E.P. Hutchinson (Sociologist), Jacob Marshak, George A. Miller (psychologist), Frederick Mosteller (statistician), Robert M. Thrall (mathematician) (Mathematical Training of Social Scientists 1955)

to organizing conferences and summer schools (although this was essential for its projects). Instead, it also recommended policies to advance the mathematical training of social scientists.

A policy statement was published on *Items* in 1955, and three ways of meeting the needs of social scientists for mathematics were identified: first, the introduction of a special curriculum in mathematics for social science students; the introduction of separate sections in mathematics courses; and finally a revised curriculum in universal mathematics to provide basic training for all undergraduate students in American colleges. (Mathematical Training of Social Scientists 1955) The main reasons for the necessity of mathematical training were primarily that of analyzing social problems mathematically (i.e., elaborating mathematical models) and recognizing the usefulness and limitations of the mathematical methods. (ibidem)

Focusing mainly on the first approach, the Committee members recognized that "the traditional undergraduate curriculum in mathematics - college algebra, trigonometry, analytic geometry, and calculus - does not afford satisfactory preparation to social scientists" (p. 14). Accordingly, they proposed some recommendations for undergraduate mathematical training. Moreover, some procedures for integrating mathematical and social sciences were suggested, like focusing on the social science training of mathematicians and interdisciplinary works. In fact, to them, " [...] social scientists will get better advice from mathematicians who participate than from those who are merely consulted. The most difficult question is often that of the mathematical formulation of social science problems. The social scientist should avoid both limiting the mathematician and having the mathematician limit him." (p.16) The final section of the statement also addressed the issue of mathematical training for graduate and postdoctorate students. One possible solution was identified in the organization of summer institutes.

The SSRC sponsored three summer schools ("SSRC Summer Institutes on Mathematics in Social Sciences") in 1953, 1955, and 1957: the first at Dartmouth College (and partially funded by the Ford Foundation), the second divided into two different sessions, one at University of Michigan and the other on Stanford, and the third, focused on more advanced training, at Stanford.[39] Among the participants, most scholars came from the fields of Economics, Psychology, and (although to a lesser extent) Sociology (other social sciences like political science were represented only by a few members). The activities of the Committee continued during the 1960s, focusing more specifically on particular topics. (Orozco Espinel 2020)

Mosteller's 1974 statement can be seen as a sort of "mission accomplished" report, inasmuch as he began and concluded his review with the following sentences:

> "The current level of mathematical training for social scientists in this country was not quickly achieved, nor did it grow by itself through natural evolution; instead, it has come about through a long, fairly deliberate process that has depended upon the ideas and contributions of a great many people and organizations [...] In 1953, in addressing the Council, I explained that the results would look meager for a long time, but that 'the carefully thought out program of the Council's Committee on the Mathematical Training of

39 For a more comprehensive discussion, and a list of activities, see: Mosteller 1974; Orozco Espinel 2020

> Social Scientists may move us slowly to a place we will be proud of in 20 years.' I think that has happened." (Mosteller 1974, p. 24)

It shows that, in the History of early postwar American social sciences, the role of the SSRC was decisive, not only in favoring interdisciplinary attitudes in social sciences but, moreover, in reshaping in a systematic fashion their "images." The case of mathematics, in this sense, is emblematic. The activities of the "Committee on Mathematical training" paralleled and were partially favored by the analogous development of Mathematical Economics in places like Cowles Foundation or RAND. Furthermore, although the State of training and research in Mathematical Economics was by far more advanced than in other disciplines, SSRC activities and summer institutes played an important role also in the generalization of the use of mathematics in Economics. (Orozco Espinel 2020)

However, if we look into the Committee's activities in the 1950s, we are struck by the negligible role of Political Science. Very few political scientists participated in such training, or so at least it seems, by the report of activities. This aspect could be explained by the contemporary involvement of the discipline's most active members in the Behavioral Revolution. Then, the aims of systematic social sciences were pursued along paths other than creating formal (namely mathematical) models of human behavior. On the other side, such a situation makes those political scientists who decided to follow the latter paths even more original and their intellectual efforts even more worthy of historical investigations.

3

THEORY OF GAMES AND FORMAL POLITICAL THEORY BEFORE RIKER

THIS CHAPTER WILL PROVIDE a general discussion on the development of the Theory of Games and some formal theories of politics in the 1950s.

The general climate of transformation in American social sciences embedded the development of these theories. The first chapter discussed the so-called "Behavioral Revolution," which radically changed American political science from the 1950s onward. The following pages will show how the economic analysis addressed political issues, like voting behavior, through formal modeling. Previously it was mentioned the pivotal role of *Theory of Games and Economic Behavior* in shaping the mathematical attitude of postwar Economics. Now, I will present how the theory of games moved across domains other than economics.[1]

Accordingly, this narrative is about Political Science and Game Theory history. For the latter, its cross-disciplinary employment is far from being unproblematic but raises problems about the general history of its evolution. Indeed the delayed acceptance of game theory into the mainstream economic theory dates back only to the beginning of the 1980s. Besides, what finally entered the toolbox of economists, was John Nash's theory (with the critical refinements made by John Harsanyi, Reinhardt Selten, Robert Aumann, Robert Wilson, and David Kreps, among the others) and not the theory von Neumann and Morgenstern presented in the 1940s (see below). However, the first attempts to employ game theory to address social and political issues in the 1950s involved both approaches.

American political scientist William Harrison Riker was undoubtedly a crucial character in the history of how Game Theory entered Political Science because he did not limit himself to employing game-theoretical notions. On the contrary, the theory of games also fulfilled his methodological concerns about political science. On this point, Riker was adamant. In the first chapter of his 1962 work, *The Theory of Political Coalitions*, he wrote: "the main hope for a genuine science of politics lies in the discovery and use of an adequate model of political behavior." (Riker 1962b, p. 9). Such a model was eminently game theoretical. Thus, rational choice and mathematical modeling matched the expectations of a scholar like Riker, who was striving for new methods in political analysis.

Riker's efforts also paralleled similar ones made by some economists. In the 1950s, scholars like Kenneth Arrow, Duncan Black, Anthony Downs, Martin Shubik (with Lloyd Shapley), up to James M. Buchanan and Gordon Tullock, some of them yet mentioned in the previous pages, produced analyses of collective choices, power, and voting in a formal fashion, and new research fields, like "Social Choice" or "Public Choice" were created. Despite many of these works not being, strictly speaking, game-theoretical, they adopted the same models of human behavior, generically referred to as "Rational Choice Theory," which flourished out of von Neumann's and Morgenstern's 1944 work.

From the above, it is apparent that GT in this story has a pivotal role. Thus, it influenced economics and social sciences in two different, although related,

1 Henceforth, GT

ways. On the one hand, it provided the first entirely consistent mathematical model of individual human behavior (thanks to von Neumann) that shaped all the subsequent models of rational choice. On the other hand, through the idea of strategic behavior and its early modeling. In general, both deeply affected the discourse and the meaning of rationality and rational decision-making in social sciences.

The chapter is divided as follows. The first section encompasses a somewhat detailed review of some analytical aspects of the theory of games, focusing mainly on von Neumann and Morgenstern's analysis. The second section discusses some results concerning social choice (Arrow), the analysis of voting and elections (Black and Downs), and their relationship with game-theoretic ideas.

3.1 "THEORY OF GAMES AND ECONOMIC BEHAVIOR"

MANY WORKS (starting from Weintraub, 1992) aimed to reconstruct and discuss the indisputable impact of GT in contemporary (post-1980s) Economics. Robert Leonard (2010) showed how this concept emerged in the first half of the XXth century, and the intellectual climate surrounding formalism and axiomatization in mathematics embedded it. In particular, the mathematician Ernst Zermelo's work on applying set theory to chess (1912) influenced the Hungary-born polymath and mathematical genius John von Neumann.

Another stream of influence, equally addressed in Leonard's work, came from the debates surrounding the nature of social sciences and scientific knowledge in general, which animated the Viennese intellectual environment until Anschluss (1938). In particular, the Theory of Games reflected the concerns and aspirations of his creators, the Austrian economist Oskar Morgenstern and von Neumann. Before leaving Austria in 1938, Morgenstern was among the most lively scholars engaged in such discussions. In 1928 von Neumann had published a mathematical result defining the rational behavior in a "game" with two players and opposite interests. (Neumann 1928) The two met at Princeton at the beginning of 1939. Morgenstern arrived there in 1938. Instead, Von Neumann had joined the Institute for Advanced Studies right after its establishment (1932), even if he continued to spend research time in Europe, at least until the political circumstances allowed it.[2] Jointly they published in 1944 *Theory of Games and Economic Behavior*. (Neumann and Morgenstern 1944)[3]

Leonard's historical work dealt with the invention of game theory. Other scholars, instead, focused on different issues, most notably, the place that the original conception of GT, as delineated by its creators in the 1944 founding text, occupies in the general history of XXth century Neo-classical Economics.[4]

The most compelling feature of the history of GT is that, although none can downplay its role in the development of postwar Economics, its influence on

2 Von Neumann was Jewish by origin. He was able to bring his and his wife's families into the U.S.A. in 1939. Morgenstern was not Jewish, and although his political attitudes were less libertarian than those of other members of the circle of Ludwig von Mises (to which Morgenstern belonged in the 1920s), he still opposed fascist policies. Therefore, in 1938, when spending a research period in the U.S.A., he decided not to return to Austria. Subsequently, he obtained a full position at Princeton. R. Leonard 2010 contains these and other biographical details. For von Neumann's intellectual and scientific biography: Israel and Gasca 2009

3 Henceforth TGEB

4 An example is Giocoli's work. (Giocoli 2003b)

the discipline was quite different from von Neumann's and especially Morgenstern's expectations, and its adoption was slower than hoped. Indeed, what the generation of young postwar economists found in the 1944 text was, on the one hand, a concise yet detailed mathematical toolbox to study optimization theory and, on the other, a precise characterization of the utility function under uncertainty for rational agents. These, especially the latter, filled a gap from which an enormous amount of scientific literature spread from the 1950s onward. However, by simply looking at the table of contents of TGEB, it is easy to dispute that these occupy the central place in their work.[5] A vast part of the work is occupied by analyzing those strategic situations that involve many players, more than 2, and permit some kinds of agreement and coalition building. This analysis rests on von Neumann's pivotal demonstration (1928) of an equilibrium solution for a 2PZSG, the famous Minimax Theorem (of whom a refined proof is offered in TGEB, too. See below). Still, the original part of their text is the latter. However, very little in those pages seems to resemble the current contents of a Game Theory Textbook.

The reasons to explain the delayed explosion of GT in economics are numerous. Nevertheless, they all point directly to the main features of von Neumann and Morgenstern's theory. Namely, for many decades it remained unclear what the aim of GT was and how GT could have helped economic theorists reach it. GT entailed strategic behavior, but it was apparent that the behavioral considerations behind coalitional games (the kind of games examined in the 1944 text) were somewhat fuzzy. Only the theory of 2-person games of pure opposition (also known as 2-Person Zero-Sum Games)[6] was partially satisfying. Then, what is the purpose of a theory of coalitions? It can hardly be prescriptive since complex real-world situations barely fit a game structure. For similar reasons, it cannot be predictive.[7]

From the point of view of technical economics, GT, both the original work by von Neumann and Morgenstern and the refinements made by Princeton scholars like Nash, Tucker, Kuhn and Shapley, among the others, provided a lot to economic theory in terms of instruments, solving techniques and theorems. For example, virtually any textbook of mathematical economics up to the 1970s contained at least a (brief) section devoted to the most important result of GT, i.e., the minimax theorem, since it was shown in the late 1940s, that it corresponds to a linear optimization problem. Another compelling example concerns the fixed-

5 As a matter of example, the two authors added the detailed axiomatic proof of their utility function only in the second edition of TGEB, published in 1947

6 Henceforth 2ZPSG

7 One interesting page of the development of GT in economics concerns the employment of a cooperative solution, the "Core," to prove the existence of the General Economic Equilibrium. The Core is the set of all undominated imputations (see below). Martin Shubik showed that it is part of the "contract curve" in the Edgeworth box, familiar from any microeconomics textbook. In a nutshell, given a group of households, some allocations of goods are proposed. The coalitions of households form to support or block the proposed allocations. A coalition blocks an allocation if another assignment is Pareto superior for its members. Therefore, the Core of the economy is the set of feasible allocations that are not blocked by any coalition. Many "existence papers" proving that the competitive equilibrium allocation is in the Core appeared in the 1960s by scholars like Debreu, Shapley, Shubik, and Herbert Scarf. (See Cogliano 2019). However, since these results did not seem ostensibly better than the existence theorems proved by more traditional methods (like the Arrow-Debreu model), they did not enhance game theory's role in economic theory. On the contrary, to many scholars, it definitely showed that GT was only a particular case of the general approach.

point theorems, a strong topological idea firstly employed by von Neumann[8], but also by John Nash in his Ph.D. dissertation, and after him by Kenneth Arrow & Gerard Debreu and Lionel W. McKenzie in their proof of the existence of General Economic Equilibrium.[9]

However, the theory of games remained peripheral in economics until the 1980s. Still, economists employed a lot of rational choice theory under certainty and uncertainty, and virtually any theoretical economic problem encompassed optimization techniques. Besides, nearly any economic problem came to be modeled in mathematical mode. Nevertheless, only a tiny group of scholars took GT seriously and became committed to both the essence of GT and the theoretical reasoning behind it.[10] This situation lasted until the late 1970s and early 1980s. Only then, due to economists' freezing enthusiasm for General Economic Equilibrium Theory and the development of powerful ideas about how to model into games significantly richer economic problems (like the different types of competition), GT conquered the hearts and the soul of economists.[11]

Although simplified and incomplete, the narrative above holds for the case of economics. However, GT is not restrained only to this discipline. This point is well-known to the many historians of ideas, or sociologists of social sciences, who, in reconstructing the history of contemporary social sciences, point out how economic techniques crossed the disciplinary borders to "invade" confining provinces. It is not my purpose in this section to fully contend their works,[12] but only to note that in the case of GT, this story is less linear than often presumed.

Indeed, despite the development of GT profoundly embedded in the theoretical conundrums of Neo-Classical Economics, like the issue of equilibrium, rationality, and economic prediction, nevertheless GT was more "social" and less "economic" than often assumed. This point displays another well-known feature of GT's history: military strategists, international relations specialists, other government officials, and scientists exploited game-theoretical ideas to address hot and widely debated Cold War topics.

This latter story is quite curious. The initial failure of GT in economics was mainly due to the deadlock in responding to a (seemingly) simple question: what modeling a game can explain to a researcher about the real world better than the existing models? The difficulty of correctly answering this question pushed economists away from this theory. Nevertheless, researchers who spent their time working on more concrete and decisive issues than trying to model market-clearing manifolds seemed satisfied with what the theory offered them.

A sounding answer for the one who knows both GT and its history could be the following. Von Neumann and Morgenstern's theory of n-persons games

8 In the only work he devoted explicitly to economic theory, *A Model of General Equilibrium* (Neumann 1945). A history of this contribution is contained in E. Roy Weintraub 1983

9 For the history and comment on these fixed-point techniques see Giocoli 2003a

10 Many personal reminiscences and anecdotes of economists show how limited the study of GT was even in those graduate programs extremely committed to mathematical mode, like MIT. See Fudenberg and Levine 2016; Roth and R. B. Wilson 2019

11 For a historical-review paper quite challenging of the adequacy of GT in providing an effective solution to Industrial Organization problems, see: F. M. Fisher 1989. Similarly, Abu Turab Rizvi contended that GT was able to "rescue" economic theory from the impasse of the General Equilibrium Theory (due to the impossibility of proving that competitive equilibria display "uniqueness" and "stability," other than existence. On this latter point, see: Ingrao and Israel 1987) The main reason lies in the arbitrarinesses of the models' assumptions, with special regard to the role of information. See Rizvi 1994

12 Something will be said in the conclusions and the text when dealing with specific issues

did not capture the many facets of general economic equilibrium or individual rational action. The theory of Non-Cooperative Games provided better service to the researcher. Still, one could not compare it to existing rational choice theories.[13] Instead, when the number of players is small, for instance, only 2, more straightforward (i.e., not technical) employments of GT could be advantageous, for instance, by representing choices and strategies in a matrix form. Since a vast part of 2-person games had been yet developed in the 1950s, one could deduce that GT could be pretty functional for some political phenomena, even if economic problems were better treated in the ways economic theorists used to in the 1950s and 1960s.

Instead, a more sociological answer, less grounded on theoretical aspects, could focus on the type of intellectual commitment found in those institutions that shaped the so-called "cold war rationality." (see Erickson et al. 2015; Erickson 2015)

Then, when one thinks of the employment of GT in political science, the raw image that comes to her mind is a Strangelovesque connection with cold war strategy, nuclear deterrence, and so on. However, less known is that GT entered political science by another route, namely through the scholarly activities of Riker. Unlike international relations theorists or "cold-war warriors," Riker committed himself to establishing an entire new sub-field of political research, where GT was not only a tool but rested at the essence of any adequate theoretical development. Historian Sonja Amadae and Political Scientist Bruce Bueno de Mesquita stated that: "it was Riker that bestowed on the game theory the promise of 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 Mesquita 1999, p. 278). In my eyes, this statement is a little bit too generous. Indeed GT never ceased to be studied in places like RAND, even in the apical days of General Economic Equilibrium Theory, in the 1960s and 1970s. Besides, among economists, the reception of Riker's GT work was close to zero, at least in the 1960s.

However, Riker's role in the cross-fertilization between game theory and other social sciences can hardly be ignored. He exploited some of the ideas in von Neumann and Morgenstern's original view, the same theory that economists apparently discarded, which was not employed either by International relations scholars. Besides, he did it with total commitment, deeply convinced of the usefulness of GT not only to model but also to utter predictions about political events. Eventually, the outcomes of Riker's advocacy of GT for political analysis mushroomed after the 1960s.[14]

The following section aims to review some concepts of Game Theory, starting with the cooperative GT of von Neumann and Morgenstern since these are the basis of Riker's subsequent analysis of political coalitions and many results that came next.

13 Especially because GT was by far less intuitive in its theoretical premises. See the following paragraph

14 Toward the end of that decade, scholars like Richard McKelvey and Peter Ordeshook (among the others) developed a type of Political Science that followed Economics at the heights of mathematical modeling. Both possessed a Ph.D. in the graduate Political Science program Riker established at the University of Rochester, starting in the mid-1960s. These topics will be part of this dissertation's final part.

3.1.1 *"The best way of playing a game": rationality and taxonomies of games*

At the Core of GT lie the notions of "game" and "rationality." The idea of developing an algorithmic procedure to fully assess the best way of behaving in such situations as parlor games had some popularity among mathematicians in the first half of the XXth Century. Chess offers the most famous example as a case of a pure opposition game.[15] At the same time, economists were struggling to present a theory of human behavior that could help to understand the rationale behind economic choices. In the 1930s, as seen, the debates about the notion of equilibrium, perfect foresight, and the way to model economic phenomena were of particular intensity.

In 1928 von Neumann wrote the first mathematical paper on GT (which he labeled *Gesellschaftsspiele*, i.e., games of society). In that work, he explored the issue of pure-opposition 2-players games, showing how in such a game, rational players should behave: the famous "Minimax Theorem." (Neumann 1928) In the joint work with Morgenstern, he extended this idea to n-person games, where no natural opposition of interests arises between players but instead between groups of them or "coalitions." In the 1944 work, the problem of rational behavior is the starting point on which the authors grounded all their analysis. Therefore, other than discussing the problem in the first (verbal) chapter, they mathematically characterized the idea of individual rational action. They did so in two ways: first, by providing an axiomatic treatment of individual utility, one of the long-lasting issues in the economic theory of their time. (Moscati 2018)[16]; second, by refashioning the Minimax in a simpler way (providing also the introductory mathematical notions to grasp the general result).

Starting with the latter, they axiomatically built an "Expected Utility Function," namely a real-valued function which looks like: $\sum_{i=1}^{n} p_i u_j(x) \quad \text{for} \quad i, j = 1 \dots n$, where p are the probabilities associated to each possible outcome $u(x)$. I.e., for each individual, utility is computed as the sum of each possible disjoint event times the probability of its occurrence.[17] After their work, it became customary for economists to define rationality as the consistency with axioms related to such properties as orderings and pre-orderings between alternatives (or preferences), and to use these properties for constructing continuous and differentiable utility functions.[18]

15 In the game-theoretic jargon, chess is 2PZSG, finite, and with perfect information. "Finite" means that there is a finite series of moves. "Perfect information" means that each player knows the opponent's moves. Before GT was invented, German mathematician Zermelo provided the valuable insight that such a game always had a solution. Princeton mathematician Harold W. Kuhn proved Zermelo's intuition showing that a 2PZSG, finite and perfect information has a Nash Equilibrium for pure strategies. See Harold William Kuhn and Tucker 1953

16 The two authors made a decisive step forward in the debate between the "ordinalist view" of utility and the "cardinalist view." After the so-called marginalist revolution, this controversy started in the last decades of the XIXth century and lasted until the 1930s. In that decade, the idea that a consistent approach for representing a utility function required only that such a function was unique up to any monotonic increasing transformation revolutionized Consumer Theory. (Hicks and R. G. Allen 1934) However, dealing with the issue of choice under uncertainty, which involves probability, von Neumann built a cardinal mathematical function, i.e., it is unique up only to a class of transformation, namely the positive linear ones. He showed precisely where the differences between choices under uncertainty and choice under certainty rested.

17 A verbal discussion, with axioms but without mathematical proof of this function, is contained in the first chapter of TGEB.(Neumann and Morgenstern 1944, 24 et ss.)

18 Kenneth Arrow described Rational Choice as the choice that satisfies the properties of connectedness and transitivity. (See Kenneth J. Arrow 1951b, 17 et ss.). Gerard Debreu's classical proof

The success of von Neumann & Morgenstern's utility function was immediate. It paved the way for a long series of debates on the real-world features of utility theories (for instance, using psychology and laboratory experiments) and the formal characterization of utility theory and rational choice. However, the role of utility theory in von Neumann & Morgenstern's GT was not much meaningful. It served mainly to make sense of the idea of payoffs, representable by a single number and, therefore, strategic action. To define the best course of action in a game, a more powerful characterization of each player's rational behavior was necessary, like that offered by the Minimax. This result, originally demonstrated by von Neumann in 1928 and reprised in TGEB, simply states that in pure-opposition situations with two players (in game-theoretical jargon 2PZSG), the best solution for each player is to pick that strategy that minimizes the maximum the other could obtain.(Neumann and Morgenstern 1944, 85 et ss. Neumann 1928)

The "minimax" is a very clear mathematical result, which employs a fundamental property of the theory of functions, the existence of "saddle points." Namely, $Max_x Min_y F(x,y) = Min_y Max_x F(x,y)$ is true when exists a saddle point (x_0, y_0) satisfying: $F(x,y_0) = Max_x F(x,y)$ and $F(x_0,y) = Min_y F(x,y)$. This result (of which von Neumann presented to the reader a simplified proof) is used to solve a version of the 2-person game, where only "pure" strategies are adopted (there is no chance), and total opposition of interests arises (the game is "zero-sum"). The "zero-sum condition" is the following: $F_1(s_1,s_2) + F_2(s_1,s_2) = 0$, from which $F(s_1,s_2) = -F(s_1,s_2)$.[19] Player 1 wants to maximize $F(s_1,s_2)$, player 2 wants to maximise $-F(s_1,s_2)$, which is equivalent to minimize $F(s_1,s_2)$ Therefore, each player's behaviour can be written as:

$$MaxF(s_1,s_2) = MinF(s_1,s_2) \tag{1}$$

The mathematical and conceptual difficulty is that each player controls only her strategy, but what the other player will do contributes to determining her payoff. Von Neumann tackles the problem by dividing the game into two subgames: a "Minorant Game," where player 1 chooses his strategy before player 2, and a "Majorant Game," where player 2 chooses his strategy before player 1. Starting with the first one, the "good way" to play for the first player is to choose a strategy that maximizes the function $Min_{s2}F(s_1,s_2)$. Indeed, player 2, acting after player 1, will choose a s_2 for which $Min_{s2}F(s_1,s_2)$. Therefore, to the first player, the value of such a game is: $Max_{s1}Min_{s2}F(s_1,s_2) = v_1$

A similar discourse holds for player 2 (the "Majorant Game") with the difference that he acts after player 1. Consequently, knowing that player one will maximize his payoff, he will choose a minimizing strategy. If v_1 and v_2 are equal, then exists a saddle point for $F(s_1,s_2)$ and therefore a solution ever exists. This result rests

of the existence of a utility function on a compact space if the preferences are continuous adopts a similar axiomatic approach. (See Debreu 1959) The review of the enormous debate around the concept of expected utility and the meaning of the EUT function "cardinality," which involved not only economists and decisions theorists but also psychologists, is beyond the scope of these pages. From a historical point of view, the most recent and most comprehensive review is by Moscati: Moscati 2018. Otherwise, the discussions around EUT, its properties, and developments (like Leonard Savage's theory, and Aumann-Anscombe's, can be found in any advanced microeconomics textbook

19 Note that x and y are now s_1 and s_2, that is, player 1's strategy and player 2's strategy. I am using a different, and simplest notation than that used by von Neumann.

upon the existence of a pair of strategies that are "saddle points." Of course, their existence is not automatically guaranteed in the case of pure strategies. Still, von Neumann showed that this is the case with mixed strategies (where the players must choose the probabilities that maximize their payoffs).[20]

This conclusion, avoiding any reference to players' psychology, offers a clear prescriptive justification for their action, other than an objective criterion to determine what is a rational strategic choice. Then, many scholars have linked this result and the mathematical proof offered by the author to von Neumann's concerns and inner aims in developing the theory of games. For instance, according to Giocoli, von Neumann was particularly interested in the prescriptive content of the analysis of rational behavior, and this point is confirmed by his adoption of a "direct proof" in the 1944 work.[21] Leonard instead framed this interest in the Hungarian mathematician's raising concerns about the disruption of European political and social order, menaced by Communism and Fascism. (Giocoli 2003a; R. Leonard 2010)

The normativity of von Neumann's idea of rationality is made apparent by the formula he and Morgenstern used in TGEB to define it: the "best way of playing a game." Thus, the two authors devoted many pages in the first chapter to present the reader with some analysis of what rational behavior is; however, in the end, they rested upon the mathematical strength of the Minimax, as they attributed the missing of a satisfactory analysis of rational behavior in social science to the "the failure to develop and apply suitable mathematical methods to the problem." (Neumann and Morgenstern 1944, p. 11)[22]

Let us explore this point further. A rational individual, in the simplest case (the classical "Robinson Crusoe"), tries to obtain the maxima of something (e.g., utility or money). However, this "maximum problem" assumes new features when an individual faces social situations involving a "disconcerting mixture of several conflicting maximum problems." In the authors' words: "He too tries to obtain an optimum result. But in order to achieve this, he must enter into relations of exchange with others. If two or more persons exchange goods with each other, then the result for each one will depend in general not merely upon his own actions but on those of the others as well. Thus each participant attempts to maximize a function of which he does not control all variables." (Neumann and Morgenstern 1944, p. 11) Therefore, characterizing rational behavior in a positive sense is challenging because rational decision-making involves considering what others will do. Morgenstern famously, in the 1930s, had shown how this could represent a paradox amenable to making nonsense of any idea of equilibrium based upon the hypothesis of perfect foresight. (Morgenstern 1976a) Instead, the Minimax could break this infinite chain by showing each player's rational strategy if the opponent is also a rational player.

20 Note that the pure strategy is a particular case of the mixed one, i.e., the case when $p = 1$

21 Giocoli summarises very clearly the difference between the two approaches: an "indirect proof" gives only necessary conditions, that is, if an equilibrium exists, then the theory is consistent. Instead, the direct method gives sufficient conditions, paving the way for a better defined prescriptive characterization of strategic rationality, and entails normative value. It could also explain von Neumann's well-known coolness toward Nash's solution, namely the "Nash Equilibrium." Indeed, the latter has far less substantive content than the "minimax": only NE is the game's solution, and it does not matter how it is reached or why people pursue an equilibrium strategy.

22 Morgenstern is referring to having rested mainly on optimization theory using calculus instead of axiomatic treatment. However, the second does not exclude the first

However, the Minimax as a behavioral criterion was subject to many criticisms, especially for being too defensive, therefore undermining von Neumann's aspiration to provide a normative justification of Rational Choice (how and why agents are rational). Indeed, this criterion gives each player a gain irrespective of what the other player is doing. Assuming that the other player behaves rationally, picking that choice that minimizes the maximum possible the other can receive represents each player's security payoff. Nevertheless, it is a poor description of actual behavior because it points not to exploit the opponent's possible lacking of rationality but instead securing a minimum for themselves.

A more general characterization of the rational Choice would be necessary, not inexorably related to a "negative" result like the "minimax." John Nash famously provided this solution, the "Nash Equilibrium." (Nash 2002a)[23] He also showed that for 2PZSG, the Minimax and the "Nash Equilibrium" were the same. But NE, perhaps the most crucial idea of contemporary Economics, is far less intuitive as a prescription of rational behavior than Neumann's. Here rests the main difficulty in GT and Rational Choice Theory, from the substantive point of view: if prescriptive, they are likely to be not predictive because their prescriptions are hard to be followed in actual interactions among individuals. On the other hand, if predictive, they cannot be easily prescriptive because none can say with certainty if a real-world situation corresponds to a Nash Equilibrium.[24]

Another feature of GT concerns the different taxonomies of games. This aspect, especially the fundamental distinction between so-called "Cooperative Games" and "Non-cooperative," also affects the different views of rational behavior envisaged. Indeed, even if the idea of rational choice could be intuitive when there is pure opposition of interest and a limited number of players, it is more complicated if the number of players is high, the rules of the game permit different kinds of cooperation, for instance, binding agreements, or the temporal horizon of the game is ill-defined.

In TGEB, von Neumann and Morgenstern defined a game as the "totality of the rules which describe it" (Neumann and Morgenstern 1944, p. 48); among these rules, there is the degree of permitted cooperation among players. However, they did not explicitly address this point. Indeed, every n-players game is solved by creating coalitions, a process for which they present a particular solution, the "stable set." (See below) Therefore, the possibility of joining a coalition provides different ways of solving a game.

The distinction between "Cooperative" games and "Non-cooperative" games was introduced by Nash, focusing on the degree of communication permitted among the players. (Nash 2002b; Nash 2002a) In a situation where communica-

23 Henceforth NE. This idea refers to strategic or "non-cooperative" games (see below). Each player i in the game has a set of strategies S, from which he selects some $s_i \in S_i$. If this selected strategy is the best response to the other players' strategies for all the players in the game, then this is a Nash Equilibrium. Assume a strategy s_i and a strategy s_{-i} (which denotes the strategy selection for all players but i). A Nash equilibrium is written as $u_i(s_i^*, s_{-i}^*) \geq u_i(s_i, s_{-i}^*)$ for all players).

24 In other words, it is intuitive why a NE is the solution of a strategic game, as well as why the Minimax is the solution to a 2PZSG. Less intuitive and more debatable is why a NE is played. The idea of "rationalizability" was introduced to cope with this problem. This notion entails the immediate idea that each player could be rational even if her beliefs are incorrect. Namely, a strategy could be the best response to another player's move, which is compatible with the idea that the other player is choosing her best response to player one's strategy, and so on. NE is then a rationalizable strategy, but not all rationalizable strategies are NE. Douglas Bernheim and David Pearce first introduced this notion separately in 1984. See Bernheim 1984; Pearce 1984

tion is impossible, each player follows the course of action that she thinks will generate the higher benefit (a case that resembles how economists used to model competitive markets). Nash demonstrated the fundamental theorems for such games, i.e., the "Nash Equilibrium," namely, that profile of strategies for which each player has no incentive to change his action if the other player adopts his best strategy.[25] The Nash Equilibrium is the most important result in GT, but it holds only for "non-cooperative" games or games of pure strategy. Besides the original idea of Nash needed much refinement to address richer situations, like those involving games made up of many stages or devoid of perfect information. Refinements that sometimes came only decades later.

In 1953 Nash returned to this distinction when discussing bargaining theory. He analyzed the solution of an economic situation where two agents have opposing interests (although "neither completely opposite nor completely coincident," Nash 2002b, p. 99) and the incentive to reach an agreement using negotiation. Such a situation is "cooperative" because the "two individuals are supposed to be able to discuss the situation and agree on a rational joint plan of action, an agreement that should be assumed to be enforceable." (ibidem).[26] After Nash, the theory of von Neumann & Morgenstern came to be defined as "cooperative."

In their 1944 joint text, they yet introduced some of the notions that became customary in GT analysis, like the matrices to represent two-person games or the game trees to represent games as a multistage process (respectively, games in "normal form," and in "extensive form").[27]They also showed that a game could be easily represented through set-theoretic notions, like partitions, to address, for instance, the degree of information among players; finally, they provided a precise characterization of the notion of "strategy," as a function that relates each possible course of action with an expected payoff. (Neumann and Morgenstern 1944, 79 et ss.) Nevertheless, they supplied the most detailed analysis for those games, which allows for the establishment of coalitions and whose solution is therefore represented by a set of possible arrangements and not, as in the successive case of Nash, by a profile of strategies. This points to the crucial differences between the meaning of finding a solution for a "cooperative game" and finding the NE (one or more) of a non-cooperative game.[28]

Eventually, one must also note that the distinction between "cooperative" and "non-cooperative" games has been somewhat lifted, starting from Nash himself. Indeed, in his last decisive contribution to GT, he advanced an attempt to establish

25 To be more precise, not all non-cooperative games have a NE. To provide the existence of the NE, the set of actions of each player must be non-empty, compact, and convex, and the preference relation on this set must be continuos and quasi-concave. To explore this point, see any intermediate textbook of Game Theory, for instance, (Osborne and Rubinstein 1994).

26 See also how Duncan Luce and Howard Raiffa, in their crucial exposition of Theory of Games, published in 1957 (see below) defined "Cooperative games": "By a *cooperative game* is meant a game in which the players have complete freedom of pre-play communication to make joint *binding* agreements. In a *non-cooperative game* absolutely no pre-play communications are permitted between the players." (Luce and Raiffa 1957, p. 89 Italics in the text)

27 The authors used the expression "normalized form." Neumann and Morgenstern 1944, p. 85.

28 Note also that there are different definitions and domains of Cooperative GT. As the game theorist and mathematician William F. Lucas put it: "[...] in the cooperative case, one assumes that the participants can communicate, form coalitions, and make binding agreements. These games are primarily concerned with which coalitions will form and how the resulting gains (or losses) will be allocated among the participants." (Lucas 1994, p. 544). Besides, game theorist Roberto Serrano also identifies four other definitions in the literature, based on such concepts as "fairness," "enforcement authority," 'normativity' other than coalitions. (Serrano 2005)

the "non-cooperative" foundations of "cooperative games." (Nash 2002c) [29] Thus, he elaborated a bargaining model where the bargaining solution was obtained through a non-cooperative threat game. From this point, an entire research program, the so-called "Nash Program," was derived, especially after the 1980s, after John Harsanyi and Reinhardt Selten invented some pivotal extensions and refinements of Nash Equilibrium. (Serrano 2005; Binmore and Dasgupta 1987)

To conclude, another crucial distinction concerns the total number of players. It exclusively affects "Cooperative games" because, in this case, each player's strategy encompasses the evaluation of joining different coalitions. Indeed in those cases, rational behavior is associated with different behaviors, that is, immediate interaction with only one other player or, instead, the interaction with different players, where a different set of choices is available. Furthermore, this means that for a 2PG, the distinction between "cooperative" and "non-cooperative" games can be lifted. In n-players games, it is fundamental and changes the solution concepts adopted and the significance and valuable applications of the theory. Indeed, when dealing with the case of three-person ZSG, von Neumann and Morgenstern wrote:

> "We saw that the zero-sum one-person game was characterized by the emergence of a maximum problem and the zero-sum two-person game by the clear cut opposition of interest which could no longer be described as a maximum problem. And just as the transition from the one-person to the zero-sum two-person game removed the pure maximum character of the problem, so the passage from the zero-sum two-person game to the zero-sum three-person game *obliterates* the pure opposition of interest." (Neumann and Morgenstern 1944, p. 220. My italics)

Then, where a "pure opposition of interests characterizes 2PZSG", a n-person game involves allying with other players; in other words, a "parallelism of interests" can arise. According to the authors, this condition makes cooperation desirable and raises the chances of agreement among the players. As they concluded: "Of all this, there can be no vestige in the zero-sum-two-person game. Between two players, where neither can win except (precisely) the other's loss, agreements or understanding are pointless." (Neumann and Morgenstern 1944, p. 221)

This also affects the idea of rational behavior. There, the dilemma does not only encompass the issue of "positive vs. normative" but also that of the substantial behavior of each player. Then, "what does it mean to behave rationally" could become a misplaced question with a solution concept containing infinitely many elements.

3.1.2 *The general solution for n-person abstract games*

VON NEUMANN AND MORGENSTERN did not envisage pure opposition of interests when the number of players is greater than two. Consequently,

29 Nash devoted the remnant part of the 1950s to work on problems of pure mathematics before his dramatic and well-known collapse into mental illness, which inhibited him from scientific activity for almost twenty years. He recovered only in the 1980s and in 1994 was the first game theorist, together with John Harsanyi and Reinhardt Selten, awarded the Nobel Prize in Economics. For a compelling biography of Nash, see Nasar 1998. For a collection of Nash's papers and a review of his scholarly accomplishments, see Harold W. Kuhn and Nasar 2002

finding a solution to such a game implies finding a set of players' payoffs showing some "stability."

As the authors defined a game as the "set of rules which describe it," a solution is the set of rules for each participant telling him how to behave in every situation which may arise. More specifically, one can adequately describe a solution for a game as a "set of imputations" (which, in the case of 2PZSG, comprises only one element). By "imputation," it meant the solution for each participant, that is, behavior that meets reasonable requirements for "optimum behavior." For example, take the case of an n-person zero-sum game: players can combine and exclude the remaining ones, a solution must take into account the gain for each player of the coalition, also considering that the coalitions can break, and players can enter in new ones if they can secure higher yields.

Differently from the Minimax seen above, a solution for the n-person zero-sum game embodies also "an absolute state of equilibrium in which the quantitative share of every participant can be precisely determined."(Neumann and Morgenstern 1944, p. 34) In Morgenstern's words: "In conceiving of the general problem, a social economy or equivalently a game of n-participants, we shall expect the same thing: a solution should be a system of imputations possessing in its entirety some kind of balance and stability the nature of which we shall try to determine."(Neumann and Morgenstern 1944, p. 36)[30]

Generally speaking, a solution V is a set of payoff distributions (or "imputations") x and y for which the following conditions hold:

1. no $x \in V$ is dominated by an $y \in V$
2. every $y \notin V$ is dominated by an $x \in V$

In a n-person game, different arrangements of players are possible, and the payoffs can be distributed among the players in different ways. A solution is a set of "distributions" which displays both "internal stability" and "external stability."[31]

Morgenstern openly related the idea of imputation to that of partial equilibrium in economic theory. However, he went further for the case of "sets of imputations," relating it to the standard behavior in a social organization. In a social context,

30 These words, and the next paragraph refers to TGEB's first chapter. This chapter is likely Morgenstern's only substantive contribution to the book. Many of the complex mathematical techniques von Neumann employed were out of reach for Morgenstern. Indeed he, as customary at the time, lacked advanced training in mathematics (even if through his friendship with Viennese scholars, like Karl Menger and Abraham Wald - other than von Neumann himself - Morgenstern's understanding of mathematical economics was by far superior to many economists of his time, like for instance Schumpeter, or Hayek). However, Morgenstern's contribution cannot be downplayed. The first chapter indeed links the theory of games with the current debates in economics, of which von Neumann was not entirely aware. Besides, this chapter is important also because the general significance of the game theory concepts is explained in verbal terms, easy to follow also for the mathematically untrained reader. On Morgenstern's role in TGEB, see: Morgenstern 1976b; Schotter 1992; Rellstab 1992; R. Leonard 2010.

31 "Internal stability" and "external stability" are habitual definitions nowadays for referring to the conditions above (see Shubik 1984). However, neither von Neumann & Morgenstern nor early textbooks, like Luce & Raiffa's, adopted them. "Domination" refers to the vectorial nature of the idea of imputations. Therefore it does not simply mirror the idea of "greater" or "lesser." For example, x dominates y when a group of participants prefers x to y and can form a coalition, an 'effective set' for x over y. If such a set exists for y over z, it does not logically imply an "effective set" for x over z.

the authors stated, the system of imputations describes the "established order of the society." Moreover, since the set of imputations also has inner stability, "once they are generally accepted, they overrule everything else and no part of them can be overruled within the limits of the accepted standards." (Neumann and Morgenstern 1944, p. 42) A potential flaw of this notion rested in the missing of "uniqueness," i.e., many (potentially infinite) solutions to a single game can exist.[32]Nevertheless, according to the authors, this is not necessarily a problem. Indeed, if stability refers to "standard of behaviors," then "given the same physical background, different "established order of society" or "accepted standard of behavior" can be built, all possessing those characteristics of inner stability which we have discussed." (ibidem)

The verbal nature of these pages, together with the analogy between the solution of a game and different "standards of behavior," drove some scholars to talk about an "institutionalist side" of von Neumann and Morgenstern's thought (see, for instance, Giocoli 2003b; Schotter 1992). This is not a surprising statement. Indeed, the authors stated that "the procedure of the mathematical theory of games of strategy gains definitely in plausibility by the correspondence which exists between its concepts and those of social organizations." (Neumann and Morgenstern 1944, p. 43) Thus, according to them, the theory of games could offer an alternative theory for social sciences, where "almost every statement which we - or for that matter, anyone else - ever made concerning social organizations runs afoul of some existing opinion. And by the very nature of things, most opinions thus far could hardly have been proved or disproved within the field of social theory." (ibidem)This aspect could also explain why many scholars, von Neumann and Morgenstern included, seemingly took for granted that the natural boundaries of game theory were to be extended to social sciences in general, not only to economics. [33]

The same discussion presented above was carried out rigorously, as stated, in the sixth chapter of TGEB (von Neumann and Morgenstern, cit., pp. 238-290). Here, the reader is taken through a quasi-textbookish step-by-step procedure to define the ideas concerning coalitions formally. Namely, each coalition's payoff and how to distribute it in such a way as to make the outcome acceptable to each player.

The first step was reducing the complex possible outcomes of a game to a single number. In mathematical terms, the device adopted was that of forming a real-valued function whose domain is the set of all the subsets of $N = 1, ..., n$ (the set of all players), that is, the set of all possible coalitions. Such a function, called "characteristic function" and denoted $v(S)$ for each $S \subset N$, provides a numerical value for coalitions. To determine the characteristic function mathematically, von Neumann treated each n-person game as a 2-person game, where players 1 and 2 are each coalition and its opposite. Therefore, one can apply the minimax theorem and determine a single value for each coalition. (Neumann and Morgenstern 1944, pp. 239–40)

The idea of characteristic function comprises "everything that can be said about coalitions between players, compensations between partners in every coalition,

32 To fully grasp this aspect, the verbal definition is not sufficient anymore, and the mathematical treatment is necessary

33 Take, for instance, the foreword and the collection of essays edited by economist Martin Shubik (who was a student of Morgenstern at Princeton), under the title *Readings in game theory and Political Behavior*. (Shubik 1954)

mergers or fights between coalitions." (Neumann and Morgenstern 1944, p. 240) However, at the same time does not say anything about how to divide the value among the coalition members. Indeed, from a mathematical point of view, $v(S)$ is not simply the sum of the individual payoffs of each member of the coalition. The notion of "characteristic function" is conceived in such a way as to take into account the possibilities each player has to forsake their coalition and join another. Mathematically, $v(S)$ has three main properties:

$$v(0) = \emptyset \tag{2}$$

$$v(S) = -v(S) \tag{3}$$

$$v(S \cup T) \geq v(S) + v(T) \text{ if } S \cap T = \emptyset \tag{4}$$

Intuitively the first means that the value of a coalition without players is zero. The second, instead, indicates that the value of a coalition is equal to its opposite (from the reduction of the n-person game to a 2-person game and the zero-sum condition). The third finally means that the value of a coalition which is the union set of two other coalitions, cannot be less than the sum of the original coalitions. From the results above, others follow logically:

$$v(N) = 0 \tag{5}$$

$$v(S_1 \cup \cdots \cup S_n) \geq v(S_1) + ... + v(S_n) \tag{6}$$
$$\text{if } S_1 ... S_n \text{ are pairwise disjoint subsets of } N$$

$$v(S_1) + ... + v(S_n) \leq 0 \tag{7}$$
$$\text{if } S_1, ..., S_n \text{ are pairwise disjoint subsets of } N \text{ with the sum } N$$

The bulk of von Neumann's mathematical argument is that analyzing any n-person zero-sum game can be set forth by employing characteristic functions.[34]To show it, he defined the concept of "strategic equivalence." It means that, if in two different games Γ and Γ', the strategic possibilities are the same and the only differences consist in the fixed payments to each player,[35] the two games are equivalent. Similarly, he introduced the concept of "reduced form" of $v(N)$, that is, $\bar{v}(S)$.[36]

34 Von Neumann also provided elementary proof of the properties above (Neumann and Morgenstern 1944, pp. 241–3. Very intuitively: if the value of the coalition without players is zero, and the game is zero-sum, its opposite, namely, the coalition of all players has a zero value too. If each coalition cannot be valued less than the sum of its pairwise disjoint subsets, this holds for any number of subsets. Finally, from above, if the sum of pairwise disjoint subsets is N, then the value of the coalition of their sum cannot be more than zero.

35 In formal terms: $v'(S) = v(S) + \sum_{k \subset s} a_k$ where $a_1, ..., a_k$ represents what plays k obtains in Γ' more than in Γ

36 Practically, what von Neumann is doing is simply providing a method for treating the characteristic functions numerically in a given game. Therefore, by normalising the values of one-element sets as γ and the values of every $(n-1)$ elements set as $-\gamma$, it is possible to obtain the value of each p-element set: this can be written as: $-p\gamma \leq \bar{v}(S) \leq (n-p)\gamma$. (See also: Luce and Raiffa 1957,

A further aspect concerning the characteristic functions of an entire game is the distinction between "inessential" and "essential" games.[37] Then, if $\bar{v}(S) = 0$ for all S, the game is "inessential," none can form a coalition because each player can obtain alone what he can obtain in a coalition with others. The opposite case, where $\bar{v}(S) > 0$ is defined as 'essential games'. Only essential games are interesting for von Neumann & Morgenstern's analysis.[38]

Finally, another critical point is the role of "symmetry" in the games and, related to this, the concept of "fairness." The idea of "symmetry" was introduced when discussing 2PZSG, and it means that by changing the role of the players, there will be no effect on the game. Apart from the considerations about consequences this concept has on any considerations of the player's individuality, symmetry plays a more significant role in n-person games.[39]

Once displayed the axiomatic foundation of the "characteristic function," von Neumann derived the solution concepts to solve the general case of n-person games, starting with the simplest case of a 3-person zero-sum game. (Neumann and Morgenstern 1944, pp. 260–3) In this situation, everything in the game is determined by 2-players coalitions, and assuming the reduced form ($\gamma = 1$), the following characteristic functions for different coalitions are possible:

- $v(0) = 0$
- $v(1) = -1$
- $v(2) = 1$
- $v(3) = 0$[40]

Assuming the coalitions formed by two players, these are $\{1,2\}, \{2,3\}, \{1,3\}$, therefore for each player, the following payoffs are possible: $v(1,2) = (\frac{1}{2}, \frac{1}{2}, -1)$, $v(1,3) = (\frac{1}{2}, -1, \frac{1}{2})$, and $v(2,3) = (-1, \frac{1}{2}, \frac{1}{2})$. Each member of the coalition splits the total value, whereas the member excluded obtains -1 (the value of the one-member coalitions). These distributions, labeled as "imputations," correspond to all the game's strategic possibilities. The solution is the set of all three. Only these distributions determine a stable equilibrium, but this stability is a characteristic of all three distributions taken together. Indeed, each coalition could be easily

pp. 185–9) The role of the inequalities above is further explored in von Neumann and Morgenstern, cit., pp. 252-3. Note that for n = 3, it has a definite value, 0,1, n -1, n. This inequality represents the "range" of possible values for each normalized CF for every number of elements in S. As I will show in a subsequent section, the notion of "the range of characteristic functions" has a crucial role in Riker's attempts to develop a formal argument upon his "size principle". See the fourth chapter of this dissertation

37 In reality, this distinction has been yet introduced in the previous chapter, discussing the case of n = 3, but now the discourse is generalized to $n > 3$, and using the 'reduced form' of the game.

38 Another way of defining 'inessentiality' in terms of the properties of characteristic function is the following: $v(S \cup T) = v(S) + v(T)$ (p. 251) This is called "additivity". Therefore, essential games have a non-additive characteristic function.

39 The condition of symmetry has been so defined: "the symmetry [...] requires that the names of the players play no role in determining the value, which should be sensitive only to how the characteristic function responds to the presence of a player in a coalition. In particular, the symmetry axiom requires that players who are treated identically by the characteristic function be treated identically by the value. "(Roth 1988, p. 5)

40 Where the number in brackets indicates the number of players in each coalition.

circumvented, for instance, if one player defects and form a 2-members new alliance with the excluded ones.[41]

In the general case of the n-person zero-sum game. (Neumann and Morgenstern 1944, pp. 263–72), the imputation has the following formal properties:

$$a_i \geq v(i) \quad \text{for} \quad i = 1 \ldots n \tag{8}$$

and

$$\sum_{i=1}^{n} a_i = 0 \tag{9}$$

These properties naturally pertain to the intuitive notions of "individual rationality" and "Pareto optimality."[42] The first property refers to the fact that a player, in a coalition or not, is expected not to accept a payment less than what he could receive being in a coalition comprised only by himself. The second property means that the sum of all imputations cannot exceed the value of the set of all players (which, as seen, is zero). But, at the same time, it also cannot be less than that because, in this second case, a player could gain without loss to the others. (Luce and Raiffa 1957, pp. 192–3) Mathematically, an "imputation" is a vector in a n-dimensional vector space L_n. An imputation $\vec{a}$ dominates another imputation $\vec{b}$ (i.e., $\vec{a} > \vec{b}$) if there exists a S with the following properties:

$$S \neq \emptyset \tag{10}$$

$$S \quad \text{is effective for } \vec{a}^{43} \tag{12}$$

$$a_i > b_i \quad \text{for all } i \in S \tag{13}$$

A set of imputations, V, is a solution if it satisfies the following properties:

- no $\vec{b} \in V$ is dominated by a $\vec{a} \in V$
- $\forall \vec{b} \notin V$ is dominated by some $\vec{a} \in b$ [44]

Then, the elements in the set of imputations are precisely those un-dominated by any element of the set and dominate all the elements outside the set. It is the exact definition given previously (see above), but now every term has a precise meaning.

41 In von Neumann & Morgenstern's words: "In each of the three distributions [...] there is, to be sure, one player who is desirous of improving his standing, but since there is only one, he is not able to do so. Neither of his two possible partners gains anything by forsaking his present ally and joining the dissatisfied player: already gets $\frac{1}{2}$, and they can get no more in any alternative distribution."(Neumann and Morgenstern 1944, pp. 262–3)

42 Even if von Neumann and Morgenstern did not label them in this way in the original 1944 text.

43 A subset $S \subseteq N$ is 'effective' if:

$$\sum_{i \in S} a_i \leq v(S) \tag{12}$$

44 Note that this does not exclude the existence of some $\vec{b} \notin V$ which dominates an $\vec{a} \in v$

However, although these precise definitions show how the solution of a n-person game can be considered a "standard of behavior" and possesses "any kind of stability," as stated in the verbal discussion, the same game rarely has a unique solution. Therefore, "several stable standards of behavior may exist for the same factual situation. Each of these would, of course, be stable and consistent in itself, but in conflict with all others." (Neumann and Morgenstern 1944, p. 266) For instance, the authors showed that for an inessential game, there exists precisely one imputation, which corresponds to the value obtained by a single player, $a_i = v(i)$. In contrast, infinitely many imputations exist for an essential game, but not that above.[45]

3.1.3 *The reception of von Neumann & Morgenstern's Theory of Games*

WORKS AS THE aforementioned by Giocoli and Leonard explored the reception of TGEB among the community of economists (see above). To summarize what happened to GT after the publication of von Neumann & Morgenstern's work, all the scholars interested in the development of mathematical economics viewed TGEB as an outstanding work. However, only some aspects of it captured the economists' attention. These, as seen, encompass EUT and von Neumann's introductory analysis of such topological ideas as convexity and linearity and their use to solve optimization problems.

The fate of von Neumann and Morgenstern's stable set solution confirms it. Indeed, from a general point of view, in the development of GT, this idea was quickly supplanted by the more specific (although significantly different) notion of NE. Focusing only on those games that Nash labeled as "cooperative" (the n-person games analyzed in TGEB), mathematicians and game theorists developed other solution ideas in the 1950s to address those features of social situations that were problematic in von Neumann and Morgenstern's analysis.[46]

Indeed, as showed by a round of empirical tests conducted at RAND in 1952, it was not easy to judge the von Neumann-Morgenstern solution because it was not entirely clear what the theory asserted. (R. Leonard 2010, p. 328; Kalisch et al. 1952) Therefore, mathematicians and game theorists seemed aware that this solution idea was too broad to present an adequate predictive or descriptive account of real-world situations.

Von Neumann and Morgenstern have treated the presence of possibly infinite solutions to each game as simply a reflection that a rational player faces different equally adequate courses of action in each situation. To overcome such a

45 Despite the infinitely many possible solutions, von Neumann conjectured that V is never empty (Neumann and Morgenstern 1944, pp. 277–8. William Lucas proved that this is not true, and the V-set for certain games may be empty. Lucas 1994

46 Duncan Luce and Howard Raiffa's *Games and Decisions* represents the first comprehensive analysis of early game-theoretical contributions up to the mid-1950s and the work many students and scholars used in their training in game theory tools. It is also the case with Riker. In their textbooks, the authors provided at least 5 different solutions ideas for n-person games, other than von Neumann and Morgenstern's. These are the "core," the Ψ-stability, the "reasonable outcomes," and the "Shapley-value." (Luce and Raiffa 1957, pp. 180–252). Furthermore, in a way that would become customary in the analytical treatment of the topic, they started with the "core," which, although successive to the "stable set," generalizes it. Note, finally, that the number of solution ideas for such games continued to grow. For instance, in a "middle-level" review by Martin Shubik, in the 1980s, the list presented totaled 8 main concept, which partially overlapped that provided by Luce and Raiffa.(Shubik 1984)

peremptory conclusion, which undermined any possible practical application of GT, many other ideas about solutions in the same social cases envisaged by von Neumann and Morgenstern appeared. Some of them, such as the "Core" or the "Shapley Value," were easily extended in the analysis of political situations, and I will return to them briefly in the next chapters. However, none of them was significantly better than von Neumann and Morgenstern's solution, at least in Riker's eyes. Then, he decided to base all his analysis of political coalitions on a particular case of their analysis. (Riker 1962b)

TGEB had nevertheless a surprising series of reviewers, most notably Herbert Simon, Leonid Hurwicz (future Nobelists), Jacob Marschak, and statistician Abraham Wald, other than mathematicians like Arthur Copeland.

Simon's review appeared on the *American Journal of Sociology*, Copeland's, instead, in the *Bulletin of the American Mathematical Society*. Both were published in 1945, together with the perhaps most famous review by Leonard Hurwicz, in *The American Economic Review*. In 1946 and 1947, Jacob Marschak, at Cowles Commission[47] and Abraham Wald published their famous review, the latter partially reprinted in Shubik's collection (see above). (Simon 1945; Copeland 1945; Hurwicz 1945; Marschak 1946; Wald 1947) Marschak's and Wald's reviews had a pivotal role in the early spreading of this work among economists, even if the early ephemeral enthusiasm among mathematical economists quickly paved the way to other issues like General Equilibrium.[48]

Simon's instead had a lesser impact. However, when he reviewed TGEB, he did it through the lens of the social scientist, not the economist, making his brief analysis extremely interesting. Copeland's, instead, is simply a restatement of the essential points of the theory. Still, his role in spreading it in the mathematicians' community, far from Princeton, is important and often neglected (see below).

Herbert Simon's name is highly famous among social scientists. He coined the notion of "bounded rationality" and was awarded the Nobel prize in Economics in 1978. Perhaps less known is that he was not trained either as an economist or a mathematician. He was a political scientist educated at Merriam's "Chicago School," even if his research interest was policy-making and organization theory. (Simon 1996) Simon started his review by explicitly recognizing the importance of TGEB for social sciences as a whole. As he wrote: "[a]lthough no explicit applications are made to sociology or political science, the schema is of such generality and breadth that it can undoubtedly make contributions of the most fundamental nature to those fields." (Simon 1945, p. 637) He appraised the development of mathematical economics and, at the same time, conceded that no similar attempt was outlined in other social sciences, letting apart from the works of people like Talcott Parsons or quantitative sociologist Stuart Dodd, that, despite resting on formalism, were not mathematical at all. He continued: "The *Theory of Games* is both more modest and infinitely more impressive than any of these earlier attempts. It seeks merely to develop in systematic and rigorous manner a theory of rational human behavior." (Simon 1945, p. 639) Simon also acknowledged the mathematical novelty of von Neumann's approach, namely his employing of set theory and topology over calculus and differential equations.

47 See the first chapter

48 However, Marschak's championing of expected utility had a substantial impact on his reception among economists. (See Moscati 2018)

According to him, the second chapter, where the authors describe, both informally and formally, the notion of "game," contained the most crucial contribution of TGEB to social sciences.

> "Sociology has been forced to treat of human behavior (at least in its rational aspects) in terms of "ends" and "means"; for example, these are fundamental categories in *The Structure of Social Action*. It could easily be shown that these two terms complicate rather than simplify the analysis of human rationality, and it is to be hoped that they will now be discarded, both in sociology and in ethics, in favor of the schema of "alternatives," "consequences," and "values" attached to "consequences" (the terminology here is the reviewer's and not that of *Theory of Games* which the description of games of strategy provides. This schema quite obviously owes its origins to the utility calculus of economics, but in its generality it can be applied, at least descriptively, to all behavior, whether rational or not." (Simon 1945, pp. 638–9)

Furthermore, within this schema, it is possible, for the first time, "to define unambiguously and to analyze the concepts of "competition" and "cooperation" which have become such important categories of sociological political and economic theory." (ibidem) Other than a sound theory of administrative behavior.

Turning toward the general solution of n-person games, Simon recognized that "the concept of "stability" [...] is perhaps not entirely free from objections in its details" however "it certainly points in a proper direction," other than offering a precise analysis of the formation of coalitions. (ibidem)[49])

Simon went further, advancing a list of potential topics in sociology and political science that could be addressed using the theory of games. "For example, it should be possible to identify the theory of revolutions with the theory of stability and instability of "standards of behavior" in certain games. For this purpose, the theory will probably have to be developed from a static to a dynamic one, however. In the field of politics, one might construct games which would illustrate the formation of two-party or multi-party systems, respectively, and this could lead to a comparison of the circumstances favoring one or another type of equilibrium." (ibidem)

Unlike Simon, Arthur Copeland was not a social scientist but a mathematician. Therefore he provided the reader with a long (16 pages) and somewhat detailed analysis of TGEB. (Copeland 1945) Copeland's main research field was Probability theory. Since his review aimed at the mathematicians' community, he limited it to the description, also using some notation, of the content of TGEB. Besides, Copeland did not go further, as instead, Simon did, to suggest profitable employments of the theory to address specific problems in social science. However, Copeland also showed a particular interest in voting theory. Specifically, he proposed a pairwise voting method labeled after him.[50] More interesting, Copeland was Howard Raiffa's Ph.D. thesis advisor at the University of Michigan. Raiffa, who, as seen, co-authored with Duncan Luce the most influential game theory textbook before the boom of GT in economics, *Games and Decisions*.[51]) In

49 In a personal exchange he had with Morgenstern, Simon was more critical on this point. (R. Leonard 2010, pp. 260–1

50 To sum up, each candidate obtained a point for each pairwise comparison he won (and half-point if there were a tie). The election was won candidate with the greater number of points

51 This, needless to say, was accompanied by the publishing of such influential textbooks, like Kreps' or Tirole and Fundenberg's. (Kreps 1990; Fudenberg and Tirole 1991

particular, Raiffa recalled that Copeland chaired a small seminar in the academic years 1948-9 and 1949-50, where he addressed some game theory topics, with particular attention to 2PZSG and their extensive form. (Raiffa 1992) Therefore, the influence Copeland had on the development of GT was indirect, although significant.

However, when economist Shubik was asked to edit some essays to provide a brief volume of readings about possible extensions of game theory to the analysis of political behavior, he picked up neither Copeland's comprehensive review nor Simon's insightful comments. He opted instead for the equally valuable provided by Wald. Besides strict game-theoretical analyses, he also inserted pieces from theoretical work but not game theory, such as Arrow's analysis of Social Choice and Black's commentary essay about the unity of Economics and Political Science.

3.2 FORMAL THEORIES OF POLITICS IN THE 1950S

GAME THEORY GAVE input to the development of Mathematical Economics, although the results were, as seen, much different from the authors' expectations. Parallel to this scientific development, the formal approach was also extended to the political and social sciences during the 1950s.[52] The fields covered included the study of collective choice behavior and voting, other than such long-lasting debated issues as "power."

The fifties opened with the publication of Kenneth Arrow's innovative analysis of Social Choice, *Individual Values and Social Choices*, but yet in the late 1940s, Scottish economist Duncan Black addressed a similar topic. Then, in 1954, Princeton and RAND game theorist Lloyd Shapley, together with Martin Shubik, published in *The American Political Science Review* a brief paper when using a theory built upon Shapley's general solution for n-person games, they provided an innovative way to analyze power. Finally, the study of international politics was sustained by the employment of game-theoretic techniques, striving for more precise results when dealing with Cold War issues. Black's work and Anthony Downs's were the milestones from which the entire field of Spatial Analysis of Voting and Elections came out. Lastly, in the same period, James M. Buchanan and Gordon Tullock paved the way for establishing the so-called "Public Choice" approach in Economics. (Kenneth J. Arrow 1951b; Black 1958; Downs 1957; James M Buchanan and Tullock 1962).

As I showed previously, the development of such formal theories did not become mainstream in the 1950s Political Science. Only after Riker's commitment and appointment at Rochester did the formal approach, namely what he labeled as "Positive Political Theory," become a well-definite subfield of the discipline. Besides, there are some differences between the early attempts to elaborate formal political analysis. The most important involves the role of GT. As I will show in the next chapter, Riker discovered GT in the second half of the 1950s and became deeply committed to it, even adopting a somewhat "lobbyist" position toward its employment in the study of politics. However, such works as Arrow's, Black's, and Downs' did not employ GT, despite resting on some ideas derived from it. The case of Shapley & Shubik is still different, and I will discuss them in relation to Riker in the following chapter. Instead, in the following pages, I will provide

52 I will spend few words defining the meaning of "formal" in the next section

a general outline of how the language of economics entered into the domain of political issues, most specifically, collective choices and voting. Eventually, by looking at the works of Arrow, Black and Downs, I will detect some central themes of these works, especially in their link to GT and economic methodology.

3.2.1 *Economists and Political Scientists*

As ECONOMISTS' language became more and more mathematical, some scholars interpreted it as a natural, or most effective, language for social sciences at all. Then, albeit with some differences, and sometimes being aware that a long intellectual tradition existed before (especially in the case of Arrow (see below)), they addressed old topics with the new powerful means offered by mathematical formalism. Since, for example, the "Behavioral Revolution" still profoundly affected the American Political Science environment, but it never became formal, this enlarged the gap between Economics and fellow disciplines. Therefore, the contributions made by economists did not easily merge in the 1950s Political Science.

It is difficult to explain what it means "to be formal" in disciplines other than mathematics. As a vast amount of literature has shown, the development of Mathematical Economics was characterized by the adoption of a well-definite set of techniques and tools. As I showed before, this process was strengthened by linking to a well-defined idea of mathematics, namely the "formalist program" and its variants (for example, Bourbakism). Still, at the same time, that path, as well as that outcome, was far from being the only possible. ([53]

In these pages, however, a broader definition of formal will be adopted. That is, "formal political theory" is intended as the kind of political theory built upon individual action, choices, and preferences, i.e., in general, upon Economic Theory. In a letter Riker wrote in the early 1960s explaining his work, he defined "formal" as saying that a theory can be expressed in algebraic rather than in verbal forms. ("Supplementary Statements", Riker to Tyler, December 4th, 1959: Riker 1959b; Riker n.d. (WHRP). On this, I will say more in the next chapter) This definition also subsumes the idea that to rest on mathematical and logical proofs is a necessary condition of a well-formulated new emerging theory.

A consequence is that, among the authors mentioned above, only Arrow's Social Choice was entirely formal in the latter sense. Black's employment of mathematical language was less sophisticated than Arrow's, even if he logically proved his statements, starting from the most famous, the "Median Voter Theorem." Downs' work, on the contrary, was exclusively verbal (despite the author's training as an economist).

These works were produced in economics by people trained as economists (in the case of Arrow, as a cutting-edge mathematical economist).[54] These authors were aware of the existence of an established research tradition in Political Science. Therefore, in their works, they tried to justify their use of such a formal analysis through methodological arguments, or at least by highlighting the closeness between traditional political issues, like voting, and the collective choice problems traditionally addressed by economists, like Welfare Economics.

53 Examples can be found in works like E. Roy Weintraub 2002 and Ingrao and Israel 1987

54 The only exception is represented by Gordon Tullock, who was trained as a law scholar

Let us now explore some terminological questions. What I described above as formal theory has been defined, yet at the end of the 1950s, by Riker as "Positive Political Theory," or also as "Formal, Positive Political Theory" (Riker 1962b, p. 33). Being formal was the main feature of this approach. However, people like Black, Buchanan, Tullock, or Riker himself discussed no precise definition of what formal meant, suggesting 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 Economics), some other definitions were adopted. Black spoke of a "Pure Science of Politics"(Black 1950; Black 1958). Tullock instead referred to a "Strict Theory of Politics"(James M Buchanan and Tullock 1962). The "Genuine Science of Politics" Riker mentioned in his 1962 work can be added to these. (Riker 1962b) All three seem to suggest that their proponents aimed to make political science really scientific. Consequently, Economics, the only social science where formal modeling was widely adopted, appeared to offer a better example to follow. However, such a relationship with economics is more problematic, especially concerning the relationship between empirical confirmation and formal theory in Riker's analysis.

It was not during the 1950s that political scientists began to make references to economics as a possible role model for their discipline. Think, for example, of Graham Wallas at LSE. (Wallas 1920, see the previous chapter) Also, Charles Merriam, when comparing the methods of political science with that of other disciplines, referred to economic analysis. In a 1923 paper for the APSR on the "recent advances in political methods," he (like Wallas) summarised the development of Economics as a move away from apriori methods (typical of the Classical school of Political Economy) toward the progressive integration of statistical analysis and psychological insights. (Merriam 1923) However, in the cases of Wallas and Merriam, undoubtedly two of the most important representatives of the "going scientific path" of Political Science in the early XXth century, economics was to be followed not into the heavens of "high theory" but in the more concrete terrain of quantitative analysis. Also, in their discussion, political scientists were invited to consider, as a possible role model, not only Economics but also other disciplines, like Biology and Anthropology (other than historical and sociological studies). Finally, both of them overemphasized the psychological insights in the choice models of economists, up to presenting a seemingly simplistic view of the development of economics that would be embodied in the forthcoming behavioral revolution (see below).

The spectacular growth of formal modeling in economics from the late 1930s onward made it possible to reshape classical political issues in a new fashion, namely a mathematical one. The mathematical turn in the 1950s aimed to define precisely the problems at stake in political analysis and provide new consistent theories to appraise how polities work and solve social and political contentions. The new theories and tools developed in economic theory matched this scope, starting from GT. However, the emphasis on formal reasoning undermined that on psychological and behavioral insights, substituting them with the issue of inner logical consistency. This proves the great difference between Wallas and Merriam's views about economics and formal modelers in the 1950s and 1960s.[55]

55 Note, however, that the two problems were never really separated, neither in economics nor in decision theory. (Moscati 2018) For instance, as will become apparent discussing Riker's view, this problem could entail the empirical validation or the "positive" vocation of a helpful theory.

3.2.2 *Social Choice and Voting: a review of Arrow, Black, and Downs*

THIS CHAPTER aims to briefly analyze some aspects of the works of Arrow, Black, and Downs. This analysis will not be comprehensive, not from the technical point of view or even the historical one. Instead, I prefer to focus on some methodological aspects of their model. Black and Arrow, it will be shown, made some use, in their analyses, of some ideas derived from TGEB. Indeed, the latter's theory of rational choice is presented in a way close to the axiomatization of utility theory set forth by the creators of GT. Besides, even Black advanced some considerations on strategic behavior in politics, although he adopted a view of rationality much different from Arrow's.

Despite his most famous work dating back to 1958, Black's early results on the theory of voting were published in the late 1940s. Therefore, this review will commence with him. Duncan Black occupies a central place among the scholars that analyzed the relationship between individual choice and collective decisions. Indeed he shared with the American Economist Anthony Downs the role of being the "founder" of the so-called Spatial Analysis in Political Science.[56] The "spatial" character of this analysis refers to the fact that each voter's preferences have a location in issues space. For instance, in perhaps the simplest way, they are preference curves in a cartesian coordinate system. On this assumption, Black demonstrated a straightforward yet important result: in each simple majority decision, the median voter's preference was decisive on a single issue. Downs generalized this effect to one-dimension party politics, namely, each party tried to capture the vote of the median voter. These results were extended and fully explored mathematically from the 1960s onwards, involving GT use (see the final chapter of this dissertation).

Black's early results are also related to the most famous arguments advanced by Kenneth Arrow regarding what the latter defined as "the General Possibility theorem for Social Welfare Functions." (Kenneth J. Arrow 1951b) Then, despite something similar, namely the voting paradoxes, had been yet discovered in the previous century, starting from the middle ages (and, most important, French writers like Jean Charles de Borda and Condorcet at the end of XVIIIth century), Arrow's and Black's works marked the emergence of the new sub-field of the analysis of collective choices using rational choice analysis. (Black 1958;McLean 2015)[57]

This parallel notwithstanding, there are also significant differences between Arrow's work and Black's, starting with the scope of their research and the degree of generality and mathematical sophistication. Black was closer to the idea of a "genuine science of politics," as later professed by Riker, and therefore can be seen as a precursor of both "Positive Political Theory" and "Public Choice." Indeed, in the 1960s, he became well acquainted with the two intellectual communities.

56 However, the American statistician and economist Harold Hotelling anticipated them. (Hotelling 1929) He directly influenced Downs, whereas Black focused on committee decisions and voting. But Hotelling's result pertained to a different kind of problem, namely that of a duopoly with homogeneous goods. He famously advanced a spatial model concerning which location two stores in the same street were to choose to maximize their revenues. He also tentatively presented a political argument concerning parties and issues, but this is not the main topic of his analysis. Eventually, Hotelling was also Kenneth Arrow's Ph.D. supervisor at Columbia University. On Hotelling, see Gaspard and Muller 2021

57 Black devoted the entire second part of his 1958 to reconstructing these works and these debates historically, up to Victorian Great Britain.

Therefore, whereas to Arrow, voting represented an example, among many, of collective choices based on individual preferences (namely axiomatized Social Welfare Functions), to Black, it represented a positive phenomenon to be explored through formal theory as well as empirical analysis.[58]

Black graduated from the University of Glasgow, where he had focused on mathematics and physics but developed a strong attitude toward Economics and Politics. In the 1930s, at the Dundee School of Economics, he started his solid lifelong friendship with the future Nobelist Ronald H. Coase.[59]

Since he was accustomed to scientific studies (in a way superior to that customary to many economists in the 1930s) and displayed a sheer interest in the analytical approach to social sciences, Black aimed to develop a "pure science of politics." This notion was also the title of the first draft of the general work, published in 1958, albeit with the less ambitious title, *The Theory of Committee and Elections* (Black 1958). His 1958 work contained a series of papers written in the second half of the 1940s and published in the most prestigious mathematical economic journal, *Econometrica*, other than on the *Journal of Political Economy* and the Italian *Il Giornale degli Economisti*. (Black 1948c; Black 1948a; Black 1948b). He also produced a more detailed brief analytical work, written with the physicist R. A. Newing, titled *Committee Decisions with Complementary Valuation*. Finally, a long series of papers must be added to these systematic works, many of them unpublished by the author or published only many years after their writing. (McLean, McMillan, and Monroe 1998; Brady and Tullock 1996) Black's early papers were written and published in the same period when mathematical economics took off in the American economists' community. Therefore, it is not surprising that they captured young scholars' attention, especially Kenneth Arrow, who later generalized the same argument in a different mathematical form. (Kenneth J. Arrow 2014)

Black spent his entire career in the United Kingdom, between Belfast, Glasgow, and the University of North Wales, at Bangor. However, he came as visiting professor in many American Universities, most notably Chicago, Rochester (after Riker arrived in 1962), and the University of Virginia, where Buchanan and Tullock established the Public Choice School. If he remained somewhat peripheral in the British community of political scientists, he had long and fruitful exchanges with the American colleague interested in voting theory. [60]

In Black's view, the scope of Political Science as a discipline is to develop a method of aggregating preference schedules. Therefore, the starting point of all the analysis is the individual, taken as equivalent to his schedule of preferences (in Economics, the notion of preferences refers to good, in the study of politics, to motions among which to make a choice). These ideas are contained in a brief methodological paper published in 1950, *The Unity of Political and Economic Science*, other than reprised in his early theoretical papers. (Black 1950; Black 1958).

58 A point also remarked by Arrow: "Black intended his work to be a contribution to the analysis of actual political behavior rather than to that of social welfare. [...]" Kenneth J. Arrow 1951b, p. 79.

59 On this point, see the biographical memoir Coase wrote for Black after the latter's death in 1991 and reprinted in: Coase 1994). On the Dundee School of Economics, established in the 1930s with a vocational attitude, see: Tribe 2022.

60 For some biographical information about Black, see Coase's preface to McLean, McMillan, and Monroe, 1998 and the first section of the editor's introduction in the same volume. Other than: Coase 1994. On the activities of Buchanan and others in Virginia in the 1950s, see: Levy and Peart 2020

In his 1950 article, Black explicitly conveyed "the unity of economics and political science" as a necessary step to build a "pure science of politics." Such a view rests upon the assumption that both disciplines, in reality, are subsets of a broader one, namely the "Theory of Choices." The core of this scientific approach to politics entails elaborating a set of appropriate formal and mathematical propositions, starting from a theory of committee decision-making. Black maintained that such a theory would not cover the workings of all committees but could provide only very general theories of political analysis. However, the improvement of existing political theory could be huge. In this sense, to him: "[...] a satisfactory Political Science [...] will have the same distinguishing marks as Walras' *Élements* or Pareto's *Manuel* - or perhaps Marshall's *Principles*, with the admixture of the rigorously formal and the descriptive treatment -rather than those of the existing texts in Politics." (Black 1950, p. 506)

The essential features of this pure theory are "precisely those of Economic Science" (p. 507) because both disciplines disengaged from the actual facts to deal with the complexities of the social world. What Black labeled as "the economic mode of abstraction" started from analyzing the most straightforward problems. Lesser abstraction can be introduced only after an adequate knowledge of them has been reached. Thus, the preferences of the individuals are the starting point of the analysis. These can be represented by employing preference schedules like economists are used to. Although Economics concerns facts for which individuals usually have much knowledge of the possible outcomes, i.e., prices and money, "there is no difference in principle between the economic and political estimates which people must make." (p. 511)

Even the essential concept of modern economic analysis and its mathematical representation, namely that of "equilibrium," can be easily extended to the study of politics. In this latter case, "political equilibrium" refers to analyzing how by using political adjustments (namely different voting procedures), a collective decision is reached given individual valuations.

The main difference with economics involves the object of these preferences. More in detail:

> "In Political Science the motions before a committee stand in some definite order on the scales of preferences of the members. Equilibrium will be reached through one motion being selected as the decision of the committee by means of voting. The impelling force towards having one particular motion selected will be the degree to which the members' schedules, taken as a group, rank it higher than the others. The barriers to its selection will be of two kinds. On the one hand, there is the degree to which the group ranks other motions as high as, or higher than, the motion concerned. And on the other, there is the particular form of committee procedure in use; and it can be shown that with a given group of schedules, one procedure will select one motion, while another procedure will select another.' If so, equilibrium in Politics is "the resultant of tastes and obstacles "; and these are the words Pareto used of equilibrium in Economics" (Black 1950, pp. 512–3)

As a forerunner of mathematical economics, Black seemed, however, not aware of the content of GT. Indeed, recognizing that such a political theory could necessary formulated in mathematical terms, he stated that the "drawback that much of the existing mathematics was developed to deal with physical problems and is not well adapted to deal with the human sciences. In time a new Mathematics will be invented." (Black 1950, p. 513, footnote 2) This is a somewhat classical

and perhaps surprising statement: namely, the classical objection against the employment of mathematics in economics advanced by Austrian economists Hans Mayer, well known to Morgenstern. It is the same objection the latter replied to in the first chapter of TGEB (see the previous chapter)

In further works, especially in his 1958 work, Black attempted to sketch political behavior as strategic. Notably, his most famous theoretical result, the "Median Voter Theorem," is easily framed as a Non-cooperative Game, where the median voter preference is the only NE. However, it is not about this aspect that Black considered strategic behavior. Instead, he referred to parties. Political parties must adapt to the change in the institutional environment they operate. Therefore, they can also dissimulate their preferences, which leads to game-theoretical analysis. However, Black did not set forth it in this work. He was not in touch with game-theoretical analysis and did not appreciate it, given GT's incapability, in his eyes, to provide results exactly. [61]

Black's employment of the idea of rational behavior can also confirm his distance from GT. In TGEB, there was an apparent ditch between the idea of EUT and the notion of rational behavior entailed in n-person games. However, the foundation of utility analysis on axiomatic bases was one of the most important legacies of von Neumann and Morgenstern's work. It points to Arrow's analysis of Rational Choice (see below). Instead, Black's theory is different.

It misses a Rational Choice Theory or a proper definition of rationality, neither in axiomatic terms (like Arrow) nor utility maximization. Black's analysis is built upon individual preferences rankings. However, although the choice of the preferred alternative could be easily interpreted as a "Rational Choice," Black never explicitly advanced such a parallel. Indeed he limited himself to assert that it could be irrational for an individual to choose an alternative he did not prefer (therefore, if a voter is indifferent between the two, it could be assumed to abstain from voting. Black 1958, p. 5)

Eventually, Black's views toward rationality were made explicit in a paper he wrote in 1969, discussing Arrow's Impossibility Theorem. (Black 1969) There, he defended his choice of the term "transitivity" over "rationality," using the following argument:

> "Rational choice' is an emotive term, with the danger that it may induce us to prejudge issues rather than analyse them. In a purely scientific part of a treatment of politics, however, we would wish, so far as possible, to avoid language of this kind and employ only neutral terms. Besides this, the term tends to label alike all procedures which do not secure complete transitivity, whether the intransitivity occurs once in ten cases or once in a hundred million. But, in regard to committee procedures, intransitivity is *essentially a quantitative matter* and 'irrational' would seem to be a wrong designation of a procedure that gave one intransitivity among a hundred million decisions." (Black 1969, pp. 233–4)

The work that paralleled, and in a certain sense, generalized Black's was that of Kenneth Arrow. Arrow focuses on the apparent inconsistency between individual

61 It is apparent from his exchanges with Ronald Coase when the latter was working on his theory of social cost (1960). Indeed, he suggested that the specific problem Coase was dealing with, the proper relationship between the efficient allocation of goods and the initial rights assignments, could be tackled using game theory. However, he added, "Neumann and Morgenstern have shown that It is hopeless to attempt anything here in an exact way." (Letter to Coase, July 30, 1959, cit. in Medema, 2020)

preferences and social choices, where the number of possible choices is greater than 2, and any kind of individual ranking of preferences is admissible. As he remarked, the origins of his interest in this problem were twofold: on the one hand, his mathematical studies, as an Economics graduate student at Columbia University in New York City, under the supervision of Hotelling. On the other, the stimulating intellectual environments of the Cowles Commission at Chicago, where he spent a research period in 1948, and RAND (in 1949). (Kenneth J. Arrow 2014) Especially at the RAND, scholars like philosopher Olaf Helmer conducted game-theoretical analyses primarily to study international politics. The latter in particular, as Arrow recalled:

> "[...] was troubled about the application of game theory when the players were interpreted as nations. The meaning of utility of preference for an individual was clear enough, but what was meant by that for a collectivity of individuals? I assured him that economists had thought about the problem in connection with the choice of economic policies and that the appropriate formalism had been developed by Abram Bergson in a paper in 1938; it was a function, called by him the Social Welfare Function, which mapped the vector of utilities of the individual into a utility." (Kenneth J. Arrow 2014, pp. 147–8)

Arrow proved that, in a certain sense, Helmer's concerns were correct. Indeed, Bergson showed how it could be possible to derive the conditions of maximum economic welfare without the summation of utilities (which involved the possibility of interpersonal comparisons of utilities). Instead, the latter was the approach followed by the "Cambridge School" (Alfred Marshall and Arthur C. Pigou) and criticized by Vilfredo Pareto. However, Arrow moved his analysis forward and demonstrated that even Bergson's social welfare analysis was misleading through the axiomatic approach based on Alfred Tarski's logic.[62]

This result, which he labeled the "general possibility theorem for Social Welfare Functions," became the bulk of his Ph.D. thesis and was published in 1951.[63]

Despite Arrow soon turning his attention to other aspects of economic theory, starting with General Equilibrium Theory, his elegant formal argument quickly captured the interest of many young economists. They offered many refinements to smooth the conditions that made social choices inconsistent with individual preferences.[64]

In his 1951 work, Arrow explicitly paralleled the voting and the market mechanism as examples of collective choices. Naturally, then, both voting and the market are regarded as special cases of the more general category of collective social choice.

This parallel notwithstanding, and the references he made to some Political Science literature, Arrow's work did not belong to this discipline. His analysis was as general as possible, and his starting point was the critique of Welfare Economics as a consistent body of tools to develop social policies. (Igersheim 2019) Furthermore, he was not pretending to advance a parallel between political

62 Instead, Bergson's analysis followed a more classic calculus-based maximization. Bergson 1938.

63 The most famous name of this result is the "Impossibility Theorem." However, Arrow pointed out the possibilities of social choices, and therefore, starting with the name, its result was less pessimistic than often implied.

64 Arrow worked extensively on economic theory topics, starting with the General Economic Equilibrium, whose existence Arrow, alongside Debreu (and Lionel W. McKenzie), was the first to provide the mathematical proof. (Kenneth J. Arrow and Debreu 1954; Düppe and E Roy Weintraub 2014a)

and economic action, other than the simple (although fundamental) fact that both can be represented in terms of rational action defined formally.[65] In this sense, even if voting mechanisms represented a clear example of a "Social Welfare Function," they were not the only ones. This point is apparent if Arrow's analysis (and Social Choice Theory in general) are compared with the works of Black, Downs, and those scholars who studied axiomatically voting behavior from the 1960s onward.

He summed up the theoretical problem he was dealing with as follows:

> "In a capitalist democracy, there are essentially two methods by which social choices can be made: voting, typically used to make 'political' decisions, and the market mechanism, typically used to make 'economic' decisions. [...] The methods of voting and the market [...] are methods of amalgamating the tastes of many individuals in the making of social choices. The methods of dictatorship and convention are, or can be, rational in the sense that any individual can be rational in his choices. Can such consistency be attributed to collective modes of choice, where the wills of many people are involved?".(Kenneth J. Arrow 1951b, pp. 1–2)[66]

Famously, the simplest form of this problem is undoubtedly the "paradox of voting" discovered by the French enlightenment writer and politician Condorcet. Given three voters, three alternatives, and the following preference orderings: A is preferred to B, and B to C for the first individual; B is preferred to C and C to A for the second individual; and C is preferred to A and A to B for the third individual (and transitivity applies ever). It is apparent that a majority prefers A to B, a majority prefers B to C, and a majority prefers C to A. As Arrow wrote: "So the method just outlined for passing from individual to collective tastes fails to satisfy the condition of rationality, as we ordinarily understand it." (p. 3). This is also the central issue of Welfare Economics.

Several assumptions simplify Arrow's analysis: individual values, and therefore preferences, are taken as given and not capable of being altered by nature; finally, individuals are assumed to be rational. Furthermore, his analysis did not entail GT, even if the author was aware of the contributions made by von Neumann and Morgenstern, and, most important, he recognized how GT could be used to address this problem.

It could happen in two ways. On the one hand, it could involve strategic manipulation made by individuals to overcome their opponent and secure the favored outcome. "Thus, in an electoral system based on plurality voting, it is notorious that an individual who really favors a minor party candidate will frequently vote for the less undesirable candidates rather than 'throw away his vote.'" (p. 7) Therefore, social choice intended as a mechanism to aggregate

65 As he pointed out, he was not the first to advance an analogy between economic choice and political choice. However, it was the first to employ the exact kind of mathematical formalism and notation that would become customary in Social Choice Theory and the foundations of the theory of consumer choice. Among the authors who advanced such a comparison, he listed the German economist Herbert Zassenhaus, American economist Howard Bowen, and Chicago economist Frank Knight. Besides, one could find curious the missing of any reference to the "Institutionalist" school (for instance, John Commons), except for Veblen's theory of leisure class. Especially Knight also stressed the differences between the two kinds of choices, even if the analysis is more 'socio-psychological' than formal.Kenneth J. Arrow 1951b, pp. 5–6

66 The problem can be also summed as follows: "to construct an ordering relation for society as a whole that will also reflect rational choice-making so that R may also be assumed to satisfy Axioms I and II" (p. 19). For the meaning of R, and the axioms, see below.

preferences would also devise rules so that citizens will actually express their tastes sincerely. This latter issue is also linked to the second possible game-theoretical analysis of this topic: namely that of constructing "games of fair division, in which the rules are to be such that each individual, by playing rationally, will succeed in getting a preassigned fair share." (ibidem).

Despite its resting upon the assumption of individual rational choice, this choice is described using "ordering relations," and the model of rational choice is built up from pair-wise comparisons among different alternatives (or, as he called them, "social states)."[67]Therefore, it does not entail, as in von Neumann's minimax theorem, the existence of a set of pure strategies in the player's 'environment' (namely, into the set of alternatives available), from which the existence of the set of mixed strategy can be derived, and therefore, an equilibrium (i.e., a 'saddle point') exists. (Kenneth J. Arrow 1951b, pp. 19–21)[68]

Furthermore, his viewpoint excludes the interpersonal comparison of utilities, namely the issue of measurable utility, which, as seen, von Neumann and Morgenstern formalized axiomatically. As he summed up their result:

> "They consider a preference pattern not only among certain alternatives but also among alternative probability distributions. Making certain plausible assumptions as to the relations among preferences for related probability distributions, they find that there is a utility indicator (unique up to a linear transformation) which has the property that the value of the utility function for nay probability distribution of certain alternatives is the mathematical expectation of the utility. Put otherwise, there is one way (unique up to a linear transformation) of assigning utilities to probability distributions such that behavior is described by saying that the individual seek to maximize his expected utility. This theorem does not, as far as I can see, give any special ethical significance to the particular utility-scale found. [...] What it does say is that among the many different ways of assigning a utility indicator to the preferences among alternative probability distributions, there is one method (more precisely, a whole set of methods which are linear transforms of each other) which has the property of stating the laws of rational behavior in a particularly convenient way. *This is a very useful matter from the point of view developing the descriptive economic theory of behavior in the presence of random events, but it has nothing to do with welfare considerations, particularly if we are interested primarily in making a social choice among alternative policies in which no random elements enter. To say otherwise would be to assert that the distribution of the social income is to be governed by the tastes of individuals for gambling*. (Kenneth J. Arrow 1951b, pp. 9–10, italics added)

Then, he assumed that an individual's behavior could be better described by utilizing a "preference scale without any cardinal significance." (p. 11)

I cannot explore the details of Arrow's formal argument. So instead, I present Arrow's ideas about the mathematization of social sciences (in general, not only economics). An essay Arrow published, both as a Cowles Commission working paper and a paper in a collection of essays edited by Harold Lasswell on the recent developments of the science of policies, contains a clear-cut review of these ideas. (Lasswell and Lerner 1951; Kenneth J. Arrow 1951a)

In Arrow's view, the application of mathematics to study real-world phenomena (natural or social) is grounded on the concept of "model." A model, Arrow

67 See below
68 See above

stated, is a class of admissible structures about social (in the case of social sciences) relationships. Certain statements can be made about such structures, and mathematics excludes those incompatible with those assertions. Then, mathematics is a language of "superior clarity and consistency" because it can be used to ascertain the components of a model or a theory in a way that overshadows the necessary over-simplifications of mathematical representation of reality. (Kenneth J. Arrow 1951a, 129 et ss.) Thus, mathematical modeling in social sciences also better addressed such long-lasting issues as the dichotomy of "individual versus the collective."

Suppose it cannot be presumed that an individual exists devoid of any kind of influence from the social environment at the same time. In that case, no collective analysis can be really freed from any analysis of individual behavior. Following Koopmans and the postwar development of econometrics (as well as the controversy with Vining over measurement, see the previous chapter), "a full characterization of each individual behavior logically implies a knowledge of group behavior" (p. 133), but, also, empirical analysis can consistently find out individual behavior.

In this respect, the principle of rationality, which is strictly related to the assumption of individual behavior, has decisive importance. Given the possibility of choosing among different courses of action, an individual can list a preference order and, consequently, form a utility index, taking into account all the external constraints, to choose his preferred course of action. The latter is the real meaning of the mathematical concept of constrained maximization of a utility function. Arrow also maintained that the solutions of mathematical models would need to be tested against statistical data to be helpful in social analysis. Therefore the second advantage of mathematical modeling was "the opportunity to tap the great resources of modern theoretical statistics as an aid in empirical validations ." (p. 132)

Arrow's argument is kept on very general lines and does not explore in-depth the issues he is referring to on.[69] This generality notwithstanding, its scope is easy to detect. Rational analysis, and therefore mathematical modeling, lies at the frontier of any expected development in the vast part of fields in social science.

Let us conclude this section by reviewing Downs' theory. Like Black (although never referred to), Downs' perspective is that of an economist wanting to investigate the "rationale of government activity" along the lines offered by economic theory for producers and consumers. He described his model as "a study of political rationality from an economic point of view." (Downs 1957, p. 11) However, his economic approach is defended exclusively through the properties of rationality he listed (not in formal terms), namely completeness, transitivity, maximizing behavior and consistency of choices across time.[70] Consequently, voters are rational when maximizing only their political preferences, and every elected official is rational when he maximizes the chances of being re-elected.

This conception of rationality is related to what Downs defined as "the self-interest axiom ."(Downs 1957, p. 27) His theory aims to provide a positive description of political behavior regarding voting and party actions. Therefore, like in Black's analysis, the assumption of rationality and self-interest serves well this

69 It is contained only a brief review of game theory, as well as a brief discussion about some sociological laws

70 These are technical definitions. In reality, Downs' discussion of rationality and self-interest is purely verbal.

purpose. At the same time, differently from Black and Arrow, Downs did not recognize practically any importance of the problem of cyclical preferences and, more in general, social choice analysis.

Furthermore, his adoption of a logic-deductive analysis contends a functionalist approach to studying institutions (not only political ones). Indeed as Downs explicitly stated, following Joseph Schumpeter, "social functions are usually the by-product, and private ambitions the end, of human action ."(p. 29)[71]

Similarly, the basic logic of voting follows from the self-interest axiom: citizens are rational in politics; therefore, they vote for the candidate they believe will provide them with more benefits. So, if these benefits are defined as a utility, hence each citizen can obtain a "utility income from governmental activity," from the self-interest axiom derived that each voter wants to maximize his utility.

From these brief remarks, it is clear that Downs, contrary to Black, did not address explicitly the unity of economics and politics. Instead, the main difference with Arrow rests in the general setting of the problem he dealt with, namely, not the formal equivalence among different kinds of social choices (voting and the market) but the working of some political institutions in a democracy.

These differences notwithstanding, it can surely be affirmed that the works of scholars like Arrow, Black, and Downs really paved the way for Riker's revolutionary agenda in Political Science and represented one of the main strands from which "Positive Political Theory" emerged.

71 Indeed, Schumpeter famously pointed out that, despite parliamentary activity's function encompassing legislative and administrative measures, democratic politics can be understood only as a "competitive struggle for power and office," and precisely this competition fulfills this social function. This view can be compared to the role of the pursuit of profit in economic activity. (Schumpeter 1942, p. 282)

4

WILLIAM H. RIKER AND THE POSTWAR POLITICAL SCIENCE

IN THIS CHAPTER I am going to address William H. Riker's life and early works, from the late 1940s, until 1962. In that year, Riker published his most ambitious theoretical work, *The Theory of Political Coalitions*[1] that represented the first full-breadth attempt to employ game theory to provide a model of political behavior. The same year, the administrators of Rochester University appointed him as Professor of Political Science and Chairman of the Department. At Rochester, he established the first Ph.D. program in political science, where formal analysis, decision and game theory, and mathematical modeling were central.

Riker was not the first to employ GT in politics. For example, the economist Shubik was the editor of a brief volume, *Theory of Games and Political Behavior*, that collected pieces of essays, papers, or general work that showed the potential fertility of a game-theoretical approach in political science. (Shubik 1954) Shubik was the author of a fully-theoretical brief paper applying one cooperative game-theory solution to political issues. (Shapley and Shubik 1954)[2]

Probably the most famous work where the theory of games was used to address political issues was Thomas Schelling's *The Strategy of Conflict* (Schelling 1980). Schelling was an economist and eventually won the Nobel Prize in economics in 2005, together with Robert Aumann.[3] However, whereas the latter was a mathematician and his contributions extremely formal, Schelling did not develop new solution techniques but instead provided innovative, valuable insights, especially about International Politics. He employed GT to address the classical coordination problem among players who do not know what the other is doing. In other words, he explored the fact that some Nash equilibria are better than others or even that some Nash equilibria are not Pareto-optimal. But also the problem of multiple NE and how to choose among them. However, Schelling was an economist and not a political scientist. Then, Schelling did not join either the methodological debates in the 1950s or he advanced a "reformist agenda" within the discipline.[4]

Anatol Rapoport also offered pivotal contributions to investigating international conflicts and their resolutions in the same fashion. However, again, despite the bold inter-disciplinarity, he did not address the methodological and disciplinary issues political scientists in the 1950s were interested in, and they do not fit the narrative I am setting forth. Because they did not affect political science definitively and durably, nor did they contribute to developing a methodology-driven and a theory-driven subfield of the discipline.

1 Henceforth referred to as TPC

2 See below

3 Theirs was the second prize awarded to Game Theory, after the 1994 prize, to John Nash, John Harsanyi, and Reinhardt Selten.

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

A similar discourse holds for other scholars who presented formal contributions or discussed game theoretic techniques in their early works. I am referring especially to Herbert Simon, Morton Kaplan, and Karl Deutsch.

In particular, Simon, perhaps the most famous, as seen, studied in Chicago in the late 1930s-early 1940s and was socialized as a professional political scientist. (Simon 1996) He also was among the first reviewers of TGEB (Simon 1945).Yet, in his review, he advanced some considerations on how scholars of politics could extend von Neumann and Morgenstern's analysis into Political Science.[5] Furthermore, Simon presented his most famous contribution to the study of rational decision-making, namely the idea of "bounded rationality," in a work where he explored, from the perspective of organization and policy science, and not economics, the analysis of organizational behavior. (Simon 1947) However, it could be said that Simon's contributions, spanning from econometrics to system theory and computer science, were too broad to have a real influence on Political Science. [6]

Karl Deutsch was another political scientist who foresaw the possibility of employing game theory in international relations. (Deutsch 1954) Even in his case, he did not explore further the theoretical and formal aspects of the theory, and therefore his analysis cannot be regarded as game-theoretical.

Similar is the case of Morton Kaplan. In one of his early works on International Politics, *System and Process in International Politics* (M. A. Kaplan 1957), Kaplan devoted an entire chapter to discussing the Theory of Games. In his view, GT had little to say concerning systematic choice patterns in international politics. However, it had a lot to say about the problem of strategic choice. Then he, similarly to Schelling, extrapolated the strategic analysis of conflict from the more general study of international politics. Kaplan framed his analysis within the ostensibly more general approach of systems theory. Indeed, to him, game theory is not a good tool to address any political problems. It is not a substitute for all other kinds of social and political theories but can be used to better understand the working of a system. (M. A. Kaplan 1957, p. 220)[7] He categorized the possible systems in International Relations in a way that, as a prestigious reviewer, the economist Kenneth Boulding, noted, resembles the states of market competition, ranging from the perfect competition (a Balance of Power system with many actors) to monopoly (International hierarchy) passing through monopolistic competition and oligopoly. (Boulding 1958)[8]The taxonomy of actors is even more complicated and involves different types and patterns of choice. However, Kaplan focused on 2PZSG and Minimax solutions, briefly presenting von Neumann and Morgenstern's cooperative solution.

These scant references to these authors and works show that in the 1950s, some political scientists tentatively employed GT in many ways. However, these

5 See the previous chapter

6 He was also awarded the Nobel Prize in Economics in 1978. On Simon's extraordinary intellectual career see his autobiography: Simon 1996

7 In a nutshell, system analysis aims at treating politics as systems of action. A system of action is a set of variables so related with regard to their environment that describable behavioral regularities characterize the internal relationships of the variables to each other and the external relationships of the set of individual variables to combinations of external variables.

8 Kenneth Boulding himself was deeply interested in the study of conflict resolutions, other than a resolute pacifist. He helped to establish the *Journal of Conflict Resolution* as well as the Center for Research on Conflict Resolution at the University of Michigan, an interdisciplinary research place that lasted from 1958 to 1971, to which Schelling and Rapoport also contributed.Erickson 2015

analyses and their proponents did not use the theory of games to enforce the creation of formal modelings on the path of Economics. Nor their investigations (with the partial exception of some of Schelling's intuitions) cross-fertilized the development of the theory itself. Different was the case of Riker and "Positive Political Theory."

Indeed I don't want to show the story of the development of some formal techniques or economics-like notions to address political issues. Instead, this work is the story of how a particular set of mathematical techniques and the commitment to theory-driven research spread up into American Political Science. In doing so, the works belonging to the new field of formal political theory were also influenced by the co-eval high-ground formal development of economics and game theory. Riker was the main character in this process, and "Positive Political Theory" developed at Rochester University mainly thanks to his efforts.

This chapter reconstructs Riker's early life and career until his appointment at Rochester. It will also focus on the methodological and philosophical papers he wrote and his first paper on game theory, a somewhat empirical test of Shapley and Shubik's power index.

4.1 RIKER IN THE 1950S: FROM HARVARD TO ROCHESTER

RIKER'S LIFE intertwined the narrative I am exploring in this work. Therefore, this section aims to present his early and formative years before establishing "Positive Political Theory." By discussing his training as a political scientist and early scholarly enterprises, some insights on the general condition of Political Science in the 1940s and the 1950s will complement the historical reconstruction before.

A historian can derive some information on Riker's life from the brief biographical memoir written by Kenneth Shepsle and Bruce Bueno de Mesquita for the biographical series of the National Academy of Science. Indeed, Riker became a member of it in 1974, the first political scientist ever admitted. (Shepsle and Bueno de Mesquita, 2001).[9] This paper offers some fascinating accounts of Riker's personality and family life, as well as of his role as teacher and mentor. However, due to the nature of the series, the general tone is often acquiescent and celebratory. Therefore, the most important source for reconstructing Riker's life is 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 significant figures in the development of American political science, to the benefit of future historians but also the practitioners of the discipline. (Riker and K. Shepsle 1979)[10] Riker's 150 typed pages interview spanned from reminiscences on graduate and undergraduate education to theoretical and methodological issues.

Riker also offered some other historical accounts of his intellectual journey, for instance, in the paper discussed at the first academic conference on the history

9 Shepsle was a Graduate Student at Rochester, in the Ph.D. program, focused on Rational Choice and Game Theory, which Riker established there, starting from 1964. Instead, Bueno de Mesquita arrived at Rochester in 1972 and remained there until 1986, becoming close to Riker.

10 There are inherent risks of resting excessively on oral history and personal reminiscences. Still, given the nature of the topic (i.e., contemporary intellectual history), these provide an essential source. For an interesting methodological discussion on Oral History about the history of Contemporary Economics, see Düppe and E Roy Weintraub 2019

of game theory in 1992. (E. Roy Weintraub 1992; Riker 1992; Riker 1997) He reprised some themes but did not generally provide new information. Besides, his narrative is often generic and neither detailed nor precise from a historiographical point of view. Therefore, my primary source remains his interview with Shepsle.

4.1.1 *Harvard and Lawrence College: graduate years and early works*

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

In his interview, he made some reflections on the state of political studies of that time. In the late '30s and early '40s, according to him:

> "[...] There was not clear conception of what the field was, in my impression. It was hard to tell the people who studied political parties and American politics from historians, and indeed they were often the same people. And it was hard to tell the people who studied constitutional law and things like that from lawyers and indeed they were often the same people. And it was very hard to tell political philosophers from historian of ideas or from people in philosophy departments, and indeed they were often the same people. So that the main activities that one associated with departments of political science [were] just very difficult to distinguish them from other fields, though that is equally true of the people who taught about public affairs." (pp. 32-3).[11]

This situation, in his view, was common both in undergraduate studies and graduate school. His bachelor's in Economics could advance some speculation about his future interest in formal methods. In reality, it is likely that the general undergraduate education in Economics at the time was of low interest and devoid of any theoretical inclination, especially for what concerns mathematical analysis. Riker, in his remarks, attributed influence to these undergraduate studies in economics only for what concerns the "mindset" of Economics and not for specific training in the discipline: "I [...] believed that the traditional study of constitutions which political scientists have engaged in, was a kind of study of purpose in behavior [...]." (Riker and K. Shepsle 1979, p. 21)

Despite this poor perception of scholarly research, he decided to apply for Graduate School in political science. Riker recollected that his range of choices comprised Harvard, Columbia, and Chicago. These were "[t]he three schools that were producing substantial numbers of political scientists at the time." (p. 36) The latter was associated with Merriam and the "Chicago School of Political Science," whose members emphasized empirical methods and quantitative

11 Another similar account is that provided by Charles Lindblom, who, following Daniel Bell's analysis of the second postwar American social sciences, defined political science in the 1940s and 1950s as "a weak discipline, hardly worth explicit comment in an account of the great and exciting issues in social sciences of that period." (Lindblom 1997, p. 229

analysis. Indeed his political science professor at DePauw, Harold Zink, advised him to apply to Chicago.[12] However, at the time, Riker was influenced by the works of E. Pendleton Herring, a professor at Harvard. Therefore he decided to enroll there in 1945. Herring, a generation younger than Merriam, was close to the latter in advocating scientific methods in social sciences and had a pivotal role in the development of the "Social Sciences Research Council" in the late 1940s and 1950s and, overall, of the "Committee on Political Behavior"[13].

At Harvard, Riker studied under Herring for two years before the latter's appointment at the SSRC. He remembered him as "an excellent person to work with, although ultimately I found what he was teaching was not terribly interesting." (Riker and K. Shepsle 1979, p. 38) Indeed Herring's approach to political analysis was that of case studies, focusing mainly on public administration. Almost thirty-five years after his graduate studies, Riker was particularly dismissive of this method, defining it as "simply artistic investigations of events," devoid of any attempt to provide generalizations and therefore to elaborate a political theory (ibidem). Such harsh judgment, he stated to Shepsle, was already well defined even in his graduate years:

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

This criticism notwithstanding, Herring was the teacher Riker felt closest to. However, the person "who was the dominant figure in that department at that time and who certainly influenced all the graduate students" (p.41) was the German scholar Carl J. Friedrich. Friedrich came to the United States in the 1920s and remained there after Adolf Hitler's rise to power. His main research fields were Constitutional theory, focusing on federalism and comparative analysis, and the history of political ideas. Furthermore, Friedrich was among the few scholars in American political science during the 1930s interested in setting empirical research in an original theoretical framework by establishing a theory of power. (Easton 1951)

Riker's relationship with him was pretty turbulent. This was due to the German professor's personality ("he was an extremely opinionated man") and his scholarly activity. Later he recalled: "He may have had, [at] an earlier period of his life, an interest in political science as such; but, by the time that I was around there, his sole interest was in teaching about the history of political ideas." (Riker and K. Shepsle 1979, p. 42) In reality these words are too severe because Friedrich never ceased to make Political Science research and some of his most important and well-received works were published still in the 1950s, 1960s, and 1970s.[14]But in Riker's remarks, Friedrich came to symbolize the general attitude

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

13 As seen in chapter 1

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

of Harvard Political Science department as well as of American discipline as a whole. Therefore, although Friedrich was hostile, like Riker, to such empirical primitive methods as case studies, this hostility was mainly due to the opposition toward the very concept of the description of political events and consequently was, in Riker's eyes, without any usefulness in establishing a science of politics. (p. 43)

Such remarks are attractive because they offer a view of the disciplinary state of Political Science as perceived by a young practitioner in the 1940s. Thus, on the one hand, there were people like Herring who pursued the aim of a scientific discipline but adopted the wrong methodologies. By contrast, Friedrich defended the need for a theoretical approach at the expense of any practical purpose. At least, according to Riker. One consequence was that the Graduate School experience was far from intellectually satisfying to Riker. In general, indeed, graduate education at Harvard was deeply compartmentalized, and the faculty emphasized historical studies. This compartmentalization allowed the co-existence of different approaches, like Herring's and Friedrich's mentioned above, but at the same time did not favor intellectual exchanges even within the same discipline. A consequence, Riker observed, was that "nothing that anybody studied in my group, at least nothing anybody studied in graduate school, had any significance for their subsequent intellectual development, which is probably a pretty good picture of the state of Harvard at that time." (p. 44)

The "Behavioral Revolution," which occurred in American political science from the late 1940s onward, originated from the same intellectual and methodological concerns Riker and other young scholars had. Take, for instance, David Easton, a student of Harvard Graduate School too. Easton's first works contained an inspired critique of modern political theory's historicist attitude (Easton 1953). Furthermore, in an interview given for the "Political Science Oral History Program," Easton remarked that "by the time I left Harvard, I just didn't know what political science was all about." (Baer, Jewell, and Sigelman 1991, p. 199) These words are extraordinarily similar to those of Riker, who stated that "people go out of Harvard without having any sense of doing anything in political science" (Riker to Shepsle, cit., p.48) and he "had no sense of what one did as a scholar in political science when I got through and finally [got a] Ph.D. [at] Harvard." (Riker and K. Shepsle 1979, p. 44)

Finally, in Riker's perception, even dissertations were not "a real investigation to discover truth or anything of that sort" but instead, "simply an exercise without any real expectation of scholarly achievement." (p. 40) Riker chose as supervisor Pendleton Herring and worked on the relationship between the "Congress of Industrial Organizations" (a federation of unions in Canada and the United States) and political organizations in the late 1930s and early 1940s, adopting a case study approach. The thesis was submitted and successfully defended in 1948, with the title *'The CIO in Politics. 1936-1946'* under the supervision of Merle Fainsod (a change due to the new appointment of Herring out of the department).

The years immediately after the conclusion of the Second World War saw a massive amount in undergraduate and graduate education, thanks also to the action of the Federal Government which provided de facto free education to the veterans of the Army (by the Servicemen's Readjustment Act of 1944, commonly known as G.I. Bill). Consequently, the rise of the demand for higher education pushed the universities to raise their supply by hiring new professors and lecturers and expanding the number of graduate students. However, in the

late 1940s, the demand for teaching began to decrease, and a poor hiring climate lasted until the mid-1950s. Riker, who was married in 1943 after an unsuccessful job application at Swarthmore College (PA), was hired by Lawrence College in Appleton (WI). There he spent almost fourteen years before his definitive move to Rochester.

Riker recalled that the intellectual atmosphere at Lawrence was quite stimulating, thanks also to a more effortless teaching requirement, which allowed more time for scholarly activities. These consisted, during this period, mainly in writing a textbook on the American political system, *Democracy in the United States* (Riker 1953), primarily based on his teaching course in American politics. (Riker and K. Shepsle 1979, 50 et ss.). He attributed great importance to the years spent working on this book, particularly to exploring the foundations of Political Science. Indeed, in his vivid account, he started to realize, just after its publication, that "it would be hard to say that any sentence in it was true" (p. 60). So then, in his mind, the issue at stake became that of what political science was and whether or not to utter true sentences.

Riker later labeled the development of political science in the 1950s as "the ferment of the 1950s" (Riker 1997). Reformist goals and practical interest in public affairs were essential in this ferment. However, in his case, the most substantial incentive was the perceived need for a rigorous foundation of the methodological premises of the discipline. He started reading science philosophy to pursue this ambitious aim, especially logic. Nevertheless, he soon realized that logic bore more on the argument's validity than its truth content. Therefore, he paralleled these studies with more applied mathematical courses (linear algebra and Calculus) before discovering von Neumann's and Morgenstern's work around the mid-fifties.

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 acquaintance started with his reading Lloyd Shapley and Martin Shubik's pivotal paper, in the *American Political Science Review*, about the power distribution in a committee system. (Shapley and Shubik 1954; Shubik 1954)[15]This work was a strictly theoretical paper (albeit devoid of excessive technical difficulties) based on mathematical solutions for n-person cooperative games developed by Shapley. Together with this paper, he also read Kenneth Arrow's work on Social Choice, and these two works led him back to von Neumann's and Morgenstern's outstanding opus. As he recalled: "There I discovered what I thought that political science needed for constructing theory" (Riker 1992, pp. 207–9)

Thus, Riker mainly devoted the second half of the 1950s to expanding his knowledge of game theory, especially cooperative game theory, to experimentally test Shapley and Shubik's conclusions and further investigate the social choice theory. (Riker 1959a; Riker 1961). To these, he also added two philosophical papers, where he dealt with the issues of how to circumscribe the events to provide descriptive generalizations of political events. (Riker 1957; Riker 1958a)

Riker quickly became "something of a publicist" for game theory in Political Science. For example, he insisted that the political theory panel at the "Midwest Conference of Political Scientists" at the University of Michigan, Ann Arbor (April 1958) was devoted to Game Theory, paralleling another on more traditional

15 See below

issues. (Harry Davis to Riker, January 10, 1958, WHRP, Box 18, Folder 2) On that occasion, Riker presented a brief working paper where he talked to fellow political scientists about game theory and politics. ("Contributions of Game Theory to Political Theory" (mimeo), Riker 1958b)[16]

Riker started his presentation by asserting that the main difficulty of explaining GT to political scientists lies in their lack of mathematical training. "[...] [P]olitical scientists are not usually trained in mathematics and are somewhat afraid of or at least diffident about pursuing it. Hence they have been diffident about pursuing the relationship of game theory to politics." (Riker 1958b, p. 1) Straightforwardly, game theory concerned "a series of theorems about how to play particular categories of games most profitably." (p. 2) A game is a set of rules made up of different moves by the actors involved in it.

That game theory has much relevance to politics was Riker's deep conviction:

> "The category of zero-sum, two-person games is clearly a model for those political situation in which two persons are each trying to do the other in. The cooperative two-person games, in which the players can, by cooperating, obtain a greater payoff than by opposing each other, bear an obvious resemblance to, e.g., oligopolistic situations so often found in economics. The n-person game is clearly a model of the contemporary nation-state system or of the free market of classical economy or of legislatures with undisciplined parties. etc.." (Riker 1958b, pp. 2–3)

In Riker's eyes, GT was primarily normative and therefore different from the descriptive model employed by Political Scientists. However, it "differs notably from the kind of normative theory heretofore found in political science. Most normative political theory is concerned with distinguishing between the just and the unjust. [...] Not so game theory. It is concerned rather with distinguishing between the smart and the stupid. It establishes and justifies standards of rationality and then uses the standards to separate wise from foolish behavior." (p. 4) Besides, "a verified normative theory might conceivably lead to a political engineering." (p. 5)

To strengthen his argument, he advanced a simple example concerning the "balance of power system" in international relations. A feature of this system is that each member must oppose anyone who assumes a predominance position. Otherwise, the balance of power will be destroyed. Riker showed that game theory could carry this step further. His straightforward model has three coalitions, two opposing and a neutral one. Using an elementary version of Shapley and Shubik's power index, Riker looked for situations where the neutral's interest was to join the most potent coalition, therefore destroying the balance, and those where it was dominant chose otherwise.[18]

16 The chairman of the session where Riker presented his paper was the Political Scientist Ralph M. Goldman (Michigan State), and the discussants were Stanley Gabis, Theodore Mitau, James M. Roherty, and Glendon Schubert.[17] However, none of them was a game theory expert or joined Riker's commitment. Goldman and Roherty were experts in war and international politics. Stanley Gabis studied under Leo Strauss in Chicago and was a political theorist. Finally, Theodore Mitau was a scholar of public affairs and educational policies.

18 Riker's oversimplified model assumed two opposing coalitions and a neutral one. Therefore there are two possible outcomes: the neutral coalition joins the weakest opposing coalition, and the balance of power is maintained; otherwise, the neutral coalition merges with the strongest one, and the balance system is broken. By using the Power Index, he computed the power of each coalition. Then he showed how much each coalition should offer to the neutral one to ally with it. Besides,

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

Riker noted that this model suffered several defects, starting with being too imprecise and not general. Besides, there was also a more technical problem, i.e., the author rested on the much-debated notion of "interpersonal comparison of utility" that economists and formal theorists were attempting to get rid of. Besides, he was also aware that the capability of the theory to provide the kind of normative analysis he suggested was long to be reached. Therefore, the contributions he mentioned were "better thought of as potential contributions rather than actual ones until bot game theory is improved and the models are more carefully fitted to political applications." (p. 5)

Despite these critical points, the general mood of Riker's presentation was quite optimistic, and it paved the way for most general works and ambitious research. Still, as he later remarked to Shepsle, he received much praise but not real feedback during the conference. (Riker and K. Shepsle 1979, p. 8) To find a more suitable intellectual environment to pursue his research agenda, he joined the "Center for Advanced Study of Behavioral Sciences" at Stanford (1960-1).

4.1.2 *Research Fellow at the CASBS. (1960-1)*

In 1954, THANKS THE FUNDING of the Ford Foundation, it was established the Center for Advanced Studies in the Behavioral Sciences (CASBS) at Stanford University. It evolved from a previous Ford Foundation's program, the "Behavioral Sciences Program" in Pasadena, California, and quickly became part of the set of non-academic institutions that shaped the "Cold War rationality." (Amadae 2003, pp. 78–9; Erickson et al. 2015) However, Unlike RAND and Cowles, the CASBS was more devoted to effective interdisciplinarity among social sciences, psychology, and behavioral sciences. Indeed, the Center attracted "many psychologists of a less hawkish persuasion than your typical RAND fellow, but the same tools (optimization, Bayesian statistics, game theory) we to be found in its offices as well." (Erickson et al. 2015, p. 14) Among the lists of fellows, we found political scientists, sociologists, economists, psychologists, historians, jurists, philosophers, etc. [19]

The Center had considerable financial support from the Ford Foundation (it consisted of several millions of dollars). His purpose was twofold: to contribute to the development of the behavioral sciences and the development of individual behavioral scientists. Furthermore, to contribute indirectly to improving the

Riker also attempted to show the best strategy for the neutral coalition. However, his model is far from being detailed and general, and even its employment of the Power Index is a very special case, with arbitrary values.

19 The CASBS is still an active research center. On his site, one can consult a comprehensive list of fellows. Among the names of the first half-decade, eight future Nobel prizes in Economics

quality of faculties in these fields. (CASBS, Plans for continuation of the Center, Advanced Studies in the Behavioral Sciences 1959) Therefore, the staff of the Center, who mainly belonged to the close Stanford University, assisted the fellows in their studies and arranged seminars and conferences to review the recent debates in many areas of social science research.[20]

In the late 1950s, Riker was looking precisely for such research opportunities. Indeed, he got in touch with Ralph W. Tyler at the beginning of 1958. Tyler, a renowned educator who had a significant impact on the progressive reforms in the American high school system, was the first director of the Center. Unfortunately, the slots for the academic year 1958-9 were complete. Therefore Riker was able to join the Center only the following year. (Ralph W. Tyler to Riker, October 15, 1958, in WHRP, Box 10, folder 1)

In the exchange with Tyler, Riker briefly described his work as follows:

> "[...] My published work (aside from a textbook on American government) is largely concerned with federalism. [...] While, I am sure, I will continue to have interest in federalism - I have a small research project in process on this subject now - I have for the past several years been developing an interest in a formal sort of political theory. The essay on the paradox of voting is directly the product of this interest and the essay on disharmony in federal government combines my two interests. If I were to be granted a fellowship by the Center, I would like to devote it to the latter interest, especially the analysis of coalitions, partly, at least, from a formal point of view. As indicated by the two essays in the Journal of Philosophy I also have a continuing interest in methodology which I might possibly pursue in part of my time at the Center."(Riker to Tyler, March 21, 1958, WHRP, Box 10, Folder 1, underlined in the text)[21]

In another letter, the political scientist added that he wanted to work on the "new formal or mathematical political science," and therefore, he wished "to attempt to formulate some mathematical statements about coalitions and to devise tests of the adequacy of these statements." (Riker to Tyler, June 22, 1959, WHRP, Box 10, folder 1)

Finally, Riker joined CASBS in 1960. As he recalled to Shepsle, the group of political scientists there was extremely heterogeneous. However, it did not undermine the importance that this period had for his training in quantitative methods and formal analysis:

> "It was a very strange bunch the year I was there. On the one hand there was Schubert and me, and on the other hand, there was Marty Diamond and his teacher, Leo Strauss, and a student of Strauss, the then-current student of Strauss. It was a very strange group of political scientists. We had nothing to say to each other [...] [T]he thing that was very nice for that year was that I got to know some people at the Center itself who read a good portion of what I wrote and it was their criticism and help that encouraged me to go on with the Coalitions book, especially a man named David Wallace who is a statistician at Chicago and he was quite encouraging. An anthropologist named Nur Yalman, who is now at Harvard, was extremely encouraging also. So that I got some real help from people who knew more about

20 One can find a detailed description of Center's activities, scopes, and the selection process of every year's cohort of fellows, in an article Ralph Tyler, his first director, published on *Science*, in the spring of 1956. Tyler 1956

21 I will examine these two works below

formal matters than I did, especially David Wallace. David Wallace was a collaborator and probably a student of Mosteller's [sic] [...] And Glen Schubert was helpful also, although in his case it was the blind leading the blind." (Riker and K. Shepsle 1979, pp. 12–3, underlined in the text)

22

These words offer for sure a glimpse of the solid interdisciplinary attitude at this Research Center. Indeed, among the other fellows, in Riker's cohort, we found the name of mathematical economists Gerard Debreu and Robert Dorfman, other than non-mathematical economic theorist Abba P. Lerner.[23]

It was at Stanford that Riker wrote a significant part of his book on political coalitions and a detailed review article about social choice and votes paradoxes published in the *American Political Science Review*.This essay, in particular, represented perhaps the first systematic exposure of Social Choice and formal voting theory to the community of political scientists. Indeed this community mainly remained indifferent to Social Choice analysis, both Arrow's work and the vast literature that came out from it.[24]

The publication of Duncan Black's *Theory of Committees and Elections* (1958) gave Riker the occasion to write this review. (Black, 1958)[25] In particular, he was upset by the lacking of attention to Black's work, which received some favorable reviews (for instance, in the *Journal of Politics* and in the *Midwestern Journal of Political Science*, but only a brief note in the leading journal, namely APSR. As Riker wrote to professor Avery Leiserson of Vanderbilt University, Nashville (TN), the editor of the book review section of the APSR:

> "I think this book is one of the half-dozen most important books on political theory to be published in this century (H. Eulau agrees); yet the review relegated it to a footnote in a manual of parliamentary law. If Black were American, I wouldn't be so upset, for I'd expect his work to get known by friendship; but since he is English,[26] I'm afraid his work may simply be ignored and this I would regard as a great loss to the discipline." (Riker to Leiserson, January 31, 1961, WHRP, Box 5, Folder 1)

Therefore he proposed to write "a bibliographical article on developments in the theory of voting and summation of preferences from 1950 to 1960." (ibidem)

22 For what concerns the people mentioned above: David Wallace is known for his authorship, together with Moesteller, of a comprehensive statistical analysis which permitted to discover the author of 12 out of 85 *Federalist Papers*. Besides, he was one of the forerunners of computational statistics. A resume of Wallace's career and academic accomplishments can be found in the obituary outlined by the University of Chicago after his death in 2017. Nur Yalman was a social anthropologist whose research focused on Middle-Eastern Politics and Social Culture. Schubert was instead among the founders of studies regarding Judicial Behavior. In particular, he showed a strong interest in the study of judicial decision-making, both from a cultural and quantitative perspective.

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

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

25 On Black's life and work, see the previous chapter.

26 Here Riker is mistaken. As seen, Black was from Scotland

It would also complement Black's work, whose second part was devoted to a somewhat careful and original reconstruction of the history of mathematical voting theory.[27]

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

The new editor of APSR, Thomas Eliot of Washington University in St. Louis (MO), swiftly accepted Riker's project. Riker then sent a copy of his planned project to Duncan Black, asking for bibliographical advice. The latter's response was highly supportive, even if the Scottish economist could not provide further references for Riker's bibliographical research. (Black to Riker, March 13, WHRP, Box 5, Folder 1) Black also stated to Riker that he "began looking into the Theory of Games to try to get a link-up with Voting and made a collection of some of the literature." (ibidem)

Consequently, in the spring of 1961, Riker wrote an "interpretive bibliography" to expose the significant developments of formal analysis of voting and social choice after Arrow's work without mathematical sophistication.

The paper is made up of five parts. The first three are devoted to "paradoxes of voting," recently re-discovered by Black, the examination of Arrow's theorem, and a verbal review of the discussions upon it among economists, mathematicians, and social scientists. Finally, Riker explored the relevance of Arrow's theorem and Social Choice for political theory in the last two parts. Besides, the author also offered some insights on what discipline he perceived political science should become. Therefore, even if lacking theoretical originality, this essay occupies an important page in the author's intellectual development and is worth examining.

As briefly outlined in the last chapter, Arrow formally generalized the problem of social choice (the problem of the consistency between individual choice and collective choices). He did so by reshaping it, using Tarski's relational logic. Then, Arrow's starting point was the two axioms that define the notion of preference

27 Indeed, Black presented a detailed history of developing some mathematical theories of elections, from Jean-Charles de Borda to Charles Dodgson (better known by his literary name, Lewis Carroll). In this second part, the Scottish economist showed great sensitivity toward the historical reconstruction of ideas. Through the discovery of his fragmentary pieces regarding the mathematics of voting, he contributed to the scholarship about Carroll.

as ordering (P, strict preference, and R, a combination of indifference and strict preference). An ordering has two properties, intuitively acceptable: is connected, that is, for any pair of alternatives, a and b, it is possible to compare them by R, to obtain aRb or bRa (without any specification of $a \neq b$); an ordering is also transitive (then $a > b$ and $b > c$ implies $a > c$) Thus, given a society formed by two or more persons, each with an ordering, the social problem is to sum the individual orderings to produce a unique social arrangement (or, in Arrow's and economists' terms, a Social Welfare Function).

Another way to think of SWF is, in Riker's words, a "[...] set of instructions fed into a vote-counting machine to inform the machine how to select a victor from the set of ballots. The goal of the theory of summation of preferences is to discover a social welfare function which, from a set of weakly ordered preferences of individuals, produces a unique weakly ordered preference for society." (p. 902). Arrow showed that no such function exists if some reasonable conditions exist.

The most usual reply to Arrow's theorem involved restricting at least one of the conditions an SWF has to satisfy or advocating a sort of "interpersonal comparison of utility" procedure.[28] For instance, a famous argument, advanced by Leo Goodman and Harry Markowitz and briefly referred to by Riker, was about both interpersonal comparison and the restriction of Arrow's notable condition of Independence from Irrelevant alternatives. (Goodman and Markowitz 1952) In fact, according to the two authors, if a person has a robust preference for a over b (in a choice between a and b), and a second person has a very weak preference for b over a, then it is reasonable for a society to choose a over b. Society can discover these by introducing "irrelevant alternatives" if, for example, the first person's ordering is a, c, d, e, b, and the second person's ordering is b, a, c, d, e.

Riker criticized this position, defending Arrow's original Independence condition as a heuristic device to avoid strategic misrepresentation. This argument, as Riker admitted, was not dissimilar from that of decision theorist Duncan Luce, contained in his work *Individual Choice Behavior*, 1959. Luce's argument shows that if one can make each decision among different and different partitions of the decision set, the final decision could depend on the type of partition adopted.

Another famous reply to Arrow briefly reviewed by Riker is that of James Buchanan, contained in his famous papers *Individual Choice in Voting and in the Market* and *Social Choice Democracy and Free Markets* (James M. Buchanan 1954b; James M. Buchanan 1954a). Buchanan opposed Arrow's analogy between social choice and individual choice, especially that the market should be expected to provide transitive social orderings. This view, in Riker's eyes, was not the case for Political Science, for which "[...] [w]ith respect to voting systems, as distinct from other methods of summation, it seems that transitivity of outcome is an essential requirement." (Riker 1961, p. 905)

The relevance of these results for political theory seemed apparent to Riker, starting from the fact that also Arrow made some remarks on this point. Indeed, even if philosophers and political scientists often thought that one could bypass the summation of individual preferences using such concepts as Rousseau's "General Will," in reality, this resurfaces every time the citizens are called to vote.

In particular, Riker shared the analysis set forth by one of his fellow political scientists at the CASBS, Glendon Schubert, in *The Public Interest* (1960). There,

28 But it must be noted that Arrow, at the beginning of his essay, strongly refuted this approach, followed instead by traditional welfare theorists.

Schubert divided political theorists into three groups: rationalists, who assume that society can discover public interest through the summation of preferences; idealists, who believe that public interest cannot be necessarily related to individual preferences; realists, who assume that a substantive public interest cannot be adequately defined. It is apparent that for "realists" and "idealists," the problem of the summation of preferences does not arise. Instead, it is essential for the so-called "rationalists." "Rationalists [...] who play the same role in political science as welfare economists in economics should be intensely concerned with the work of Black and Arrow. (Riker 1961, p. 906) Thus, political scientists have to face the same problems as economists, namely that of avoiding the impossibility results, and for this, they have to make some restrictions on Arrow's conditions.

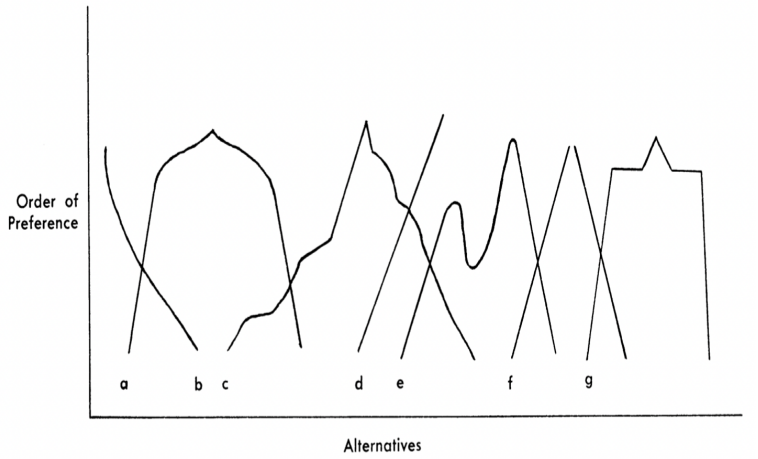

FIG. 1. Examples of preference Curves

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

Figure 1: Preferences curves. From Riker, 1959, p. 907

Riker for sure rebuffed the "idealist position." However, it is complicated to ascertain where he posited himself between the "realists" and the "rationalists." He started as a rationalist regarding the analysis methodology but evolved into a "realist." Indeed, in the late 1970s-early 1980s, he famously advocated a somewhat radical "realist" position as a sort of extreme dead-end of Arrow's theorem and the impossibility literature derived from it (even if he maintained the necessity of a rational-choice understanding of the premises of political action). (Riker 1982) However, as Riker stated in his 1961 essay, "until political theorists avail themselves of the appropriate tools for discussion [...], It is inappropriate to conclude that the discussion can have no worthwhile outcome." (Riker 1961, p. 906) In the early 1960s, some powerful "impossibility results" were still far from being discovered in formal political theory. Therefore a scholar like Riker could have still presumed that some restrictions of Arrow's conditions could work the paradox out.

As seen previously, Duncan Black offered one of the most successful attempts to solve the voting paradox by restricting the admissible set of preferences. Indeed, in his works, the Scottish economist discovered and proved that simple majority voting produces a unique transitive ordering if the whole set of individual ordering is single-peaked. By "single-peakedness," Black intended a curve

with only one optimum point. Since each curve, in his analysis, represents each committee member's ranking of preferences, this means that "the further a motion departs to the right (or to the left) of a member's optimum, the less he desires that that motion should be adopted." (Black 1958, p. 10) Riker summed this notion as follows: "By "single-peaked" is meant that all the individual orderings can be represented on a graph with the rank of preference on the ordinate and the alternatives themselves on the abscissa such that each ordering (or preference curve) appears as a curve with one and only one peak." (Riker 1961, p. 906). Single-peakedness is a necessary condition of Black's "median voter theorem".[29]

Thus, it is apparent that Black's result is important for political science. As Riker pointed out: "Black's notable discovery may be epitomized by saying that social welfare functions exist without inconsistency among the conditions if some sort of inner harmony exists among the persons in the society." (Riker 1961, p. 906) Then, the failure to agree on standards (that is, the lacking single peakedness) implies the presence of cleavages in society, which are challenging to reconcile. This latter point also involves the problem of social norms and agreement upon them. On the contrary, single-peakedness means at least an agreement on which alternative is not the worst.

Black also tried to compute the random apriori expectation of cyclical majorities without obtaining a general rule. The main result, although far from being complete or definitive, is that an apriori expectations of the existence of a majority decision, for $n \geq 5$ (n is the number of alternatives) is tiny (Riker briefly reviews many different results in pp. 908-910). This latter aspect points directly to the central problem of political science. Namely, apart from all the low chance of obtaining a majority decision, "the surprising fact is that majority decisions occur at all." (Riker 1961, p. 909)

Applied to the study of American political institutions, mainly the Congress, the social choice analysis can offer, for instance, a theoretical explanation of the significance and scope of classic strategies of parliamentary action. A prospective loser can transform the opponents' preferences by introducing a new alternative, transforming a single-peaked curve into a double-peaked. These tactics are persistent in American congressional history (Riker's example is the debate concerning the 17th amendment of the U.S. Constitution). Besides, one voter or a committee member can also misrepresent the preferences to obtain a more preferred outcome.[30]

Riker concluded his essay by presenting what he regarded as the main points of his analysis, as well as why the social choice analysis was important for political science:

> "The main point of this bibliographical essay has been to suggest that the work of Black and Arrow is of great importance both for political theory

29 Arrow used a different formalism to clarify the intuition behind this idea: he introduced the relation of "betweenness": y is between x and z, namely $B(x, y, z)$ (the same holds for x and y). Therefore, single-peaked preferences simply means that there is at least an alternative that is not judged as the worst outcome by any voter. Besides, a weakening of Black's condition has been attempted by Michael Dummett and Robin Farquharson, (Dummett and Farquharson 1961), according to whom, a sufficient condition for stability is the existence of at least one outcome x such that, for every other outcome y, some majority regards x as at least as good as y. They defined a situation is defined as stable if no groups of voters can, by voting differently, obtain a different result which the group prefers.

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

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

The italics above confirm what was said previously about Riker's position in Schubert's taxonomy. The formal analysis of politics can be interpreted as a "positive program," looking forward to empirical evaluation of policies and theoretical ideas. As mentioned, Riker's expectations somewhat froze in the 1970s. However, it does not undermine the importance of the ideas and theoretical concepts he presented for the establishment of a consistent and comprehensive political theory:

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

Riker sent a copy of his article to Black, who favorably read it. Then, in the autumn of 1962, Black led a graduate seminar at the Virginia Polytechnic, where he met, among the others, Buchanan, Gordon Tullock, and Ronald Coase. (Black to Riker, May 14, 1962, WHRP, Box 5, Folder 1) The following year Black joined Rochester for a semester. Riker's activity in reshaping the political science department was about to start.

4.2 THE FOUNDATIONS OF A "FORMAL, POSITIVE POLITICAL THEORY"

RIKER DESCRIBED the work he aimed to pursue at Stanford as "formal, positive political theory" in a letter he exchanged with the Center's staff. ("Supplementary Statements," Riker to Tyler, December 4, 1959, WHRP, Box 10, Folder 1) In his words, "formal" meant that the theory was to be expressed in algebraic rather than verbal symbols. Instead, "positive" refers to the descriptive, rather than normative, nature of his analysis.

Besides, in an inventory of personal interests each Center's fellow was asked to fulfill, he outlined his current interests and activities in the following way:

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

> neo-classical theory of value acquires most of interest from the fact that it seems also in some ways to be descriptive, so also a thoroughly deductive political theory would be much interesting if had some descriptive validity."("Inventory of Personal Interests," Riker 1960)

Well before he arrived at the CASBS, Riker had begun working on some of the abovementioned issues. Those lines, then, encompass in a nutshell the outcome of some years-long period of reflection and study that pushed Riker into a more robust commitment to formal analysis.

Letting apart his relationship with the scholar community of game theorists (which will be the object of a different section in the next chapter), Riker's interest in game-theoretical applications in political science started from Shapley and Shubik's paper on the measurement of power in a committee. (Shapley and Shubik 1954) Then, he tried to compare it against empirical data. (Riker 1959a) This attempt shows how Riker tried exploiting game theory and working with its basic idea, namely individual rational action. Besides, Riker exhibited a strong interest also for general ideas related to the methodology of social science (despite sometimes showing a derogatory nuance toward these kinds of debates). He also published two brief but dense papers on such a philosophical deal. (Riker 1957; Riker 1958a)

4.2.1 *Squabbling over methodology: Riker's philosophical papers (1957-1958)*

DUE TO THE FRUSTRATION with the state of coeval Political Science, Riker decided to join the methodological debates set forth by the American practitioners. However, he did so "in a spirit of reluctant temerity," given his somewhat skepticism toward the utility of such debates. Indeed, at the very beginning of his first philosophical paper, he wrote: "The social sciences are today so beset with squabbles over methodology that it seems we are more intent on talking about learning than we are on learning itself." (Riker 1957, p. 57) Moreover, he did not aim at a single target, author, or issue. Instead, in the two papers published in the *Journal of Philosophy*, he assumed a strong position regarding fundamental problems like the ultimate purpose of social science and how to reach it.

Riker did not address those topics which were to be discussed extensively in other works (starting with the first chapter of *The Theory of Political Coalitions*) and which represented the main methodological features of "Positive Political Theory:" i.e., the role of models, the use of mathematics, or the rationality assumptions. Further, he did not explicitly advocate an "economic theory" of politics. Then, his arguments appear to differ from Black's unity of political science and economics or Downs' defense of the "self-interest" axiom. (Black 1950; Downs 1957)

In the yet-referred interview with Shepsle, Riker remarked that in his attempt to make political science scientific, he started with the extensive study of the philosophy of science and logic before devoting himself to mathematical training. (Riker and K. Shepsle 1979). To him, the most critical questions regarding the status of political science encompassed if some sort of exact sentences about the real world were possible and how to conciliate positive aspirations with normative judgments. In this view, the starting point in dealing with these essential questions entails defining the objects under investigation and their relations, namely, "events" and their causes in Riker's vocabulary.

From the philosopher of science's point of view, Riker's argument could exhibit some weaknesses, other than misrepresenting the kind of scientific knowledge embodied in natural sciences. So, these two works (especially the first, *Events and Situations*) can be interpreted broadly as a sort of "philosophical manifesto" for his approach and not a well-defined theoretical work. Eventually, their importance lies more in presenting Riker's commitment toward a more precise way of studying politics than their importance for the philosopher of science. Besides, these papers represent an important step toward establishing true and positive political science, often referred to in all the methodological discussions Riker engaged in defending his view, even in the later years of his career.

In the first paper, Riker focused on the importance of carefully circumscribing the events which define a scientific study. Therefore, he challenged such social science entailing the development of universal theories of society, which he ascribed to the "poetic tradition in the social sciences." (Riker 1957, p. 70) Furthermore, he was equally hostile toward such approaches that focus on studying idiosyncratic details of rare events, as confirmed by his hostility toward the case studies approach in his graduate years. Given what he explicitly addressed as the ultimate purpose of social sciences, namely the scientific explanation, both these views are inadequate, mainly because they are unclear about what is to be explained.

Riker's argument goes as follows: to him, the starting data in analyzing social facts are "motions" and "actions," which the researcher can interpret as agents framed by a surrounding context and operate within it. The unit of motion and actions, i.e., the central concepts in human and social studies, are labeled as "events," namely "subjectively differentiated portion of motion and action." (p. 58)[31] Human events can be delineated as a beginningless and endless continuum of these motions and actions. These can be arranged as a consequential order, where the end of a segment of motion (or action) is the beginning of another. Despite this continuum, individuals cannot comprehend the whole of this continuous reality. Then this must be divided into pieces, viz., imagining starts and stops in continuous motion and actions. Therefore, the events are "what lies between the starts and stops" (p. 59). By imposing some boundaries, the separation of events is exceptionally subjective. "The motion and action in an event are objectively existent, but the boundaries are subjectively imposed." (p. 60) Riker calls the boundaries imposed by humans to continuous reality "situations" (an "initial situation" and a "terminal situation"). Contrary to events, these do not contain a portion of reality but "may be regarded as instantaneous" photographs of reality.[32]

A "form" characterizes a situation, i.e., the condition of movers and actors, by an arrangement, the spatial relationship between actors and movers, and the situation itself. However, "movers" and "actors" are also influenced by their previous history ("the condition" of the situation). Any situation can serve as the initial or terminal situation of an infinite number of events (for instance, the same situation can be the initial of an event and the terminal of the other).

31 A more precise definition of an event, which also encompasses that of situations, is offered near the end of his paper.

32 Riker's example is that of a film, where the continuous picture between two frames can be regarded as an event and the two frames as a situation. More precisely: "*A situation is an arrangement and condition of movers and actors in a specified instantaneous and spatially extends location*." (Riker 1957, p. 61)

Consequently, events can be more precisely defined as follows: "*the motion and action occurring between an initial situation and a terminal situation such that all and only the movers and actors of the initial situation (or the component into which they are formed in the course of the event) are included in the terminal situation*". (p. 61 italics in the text). If some actors and movers are in the same initial situation but not the terminal one, their event is different. The "history" of an event is how the movers and actors proceed from the initial situation to the terminal one. Furthermore, one must conceive an event to include all the movers and actors who explain it. Otherwise, according to him, it is "ambiguous" and illogical.[33]

Riker offered a historical example to convince the reader, namely the First World War and its outbreak. As he wrote: "The initial situation sometimes included only the Austro-Serbian controversy: sometimes, however, it included the state of affairs among the Allied and the Central powers, sometimes it even includes the whole world, etc." (p. 63). In Riker's terminology, we have an unambiguous event A, which includes the beginning and the conclusion of the war, an ambiguous event a which has the same terminal situation of A, but three different initial beginnings, the whole world, the Austro-Serbian controversy and the relations between allied and central powers. Therefore, to better ascertain such an event as the world war, all the other (and "ambiguous") events that share some features must be considered.

The central thesis Riker is defending is that the analysis in social science must be carried forward only after circumscribing the proper object to be investigated, i.e., considering all the situations that determine an event. It means devoting much attention to the small events instead of the larger ones because the latter are more prone to be "ambiguous events." To him, such a methodology mirrors that of natural science:

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

33 To complete his discourse about ambiguous and non-ambiguous events, Riker also lists a taxonomy which comprises some "general canons" for determining the ambiguity of events:

1. Some events are so complicated that they are inherently ambiguous (for instance, where there is a vast number of actors and movers, like in the "world histories")
2. Some events with no clear initial situations are probably ambiguous
3. Some events with no clear terminal situations are probably ambiguous
4. Events that are presently occurring and then without terminal situation are probably ambiguous
5. Large events with many movers and actors and great extent and duration are likely more ambiguous than small events with few movers and actors. This point is important because, in Riker's view, is one of the chief lessons that social scientists can learn from natural scientists. (Riker 1957, p. 65)

> nated from events, or at least they are identified and their motion and action described. (Riker 1957, p. 68)

Most notably, Riker advanced a scant reference to von Neumann's work at the end of his paper. To him, this has been one of the most successful attempts to restrict attention to smaller events, although isolated. This reference can suggest that his reading of TGEB could have influenced some of his arguments. Indeed, in the first chapter of TGEB, other than a verbal description of the problems they were dealing with, von Neumann & Morgenstern devoted a brief section to the "necessary limitations of the objectives" of their analysis. (Neumann and Morgenstern 1944, 5 et ss.) Then, we found some phrases which present a striking resemblance with Riker's argument, as the following passage:

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

The same standard of modesty, which characterized the development of physics, namely the study of simplest phenomena, should also be applied to Economics. "The sound procedure is to obtain first utmost precision and mastery in a limited field, and then to proceed to another, somewhat wider one, and so on." (ibidem) Besides, this could also represent a better approach to social and economic policies. Eventually, however, despite similarities, there are also some differences between Riker's and von Neumann $ Morgenstern's pages. Namely, von Neumann & Morgenstern's focus on small events was about" the behavior of the individual and about the simplest forms of exchange." (ibidem) Riker's delimitation of events, at least in this philosophical paper, instead did not directly entail a theory of human agency.

The second issue Riker dealt with was the causal relationship between events. This issue is the main topic of his second philosophical paper, *Cause and Events*. (Riker 1958a)

The cause of an event can be defined comprehensively as the necessary and sufficient condition of its occurrence. However, to Riker, the most questionable aspect of this definition concerning the social sciences lies in the notion of sufficiency, namely that a cause is the only one for any event. Therefore, he attempted to develop a standard definition of causality more apt to the social sciences.

Thus, the author defined the concept of "cause" as follows: "One event causes another if and only if the terminal situation of the causing events is identical with the initial situation of the caused event." (Riker 1958a, p. 282) By "identical," Riker means a time-space location (precisely like a "situation" is). Then, assuming that actors and movers can occupy only one space-time location, location identity necessarily implies that movers and actors are the same. Moreover, since movers and actors cannot simultaneously have two "histories" or "arrangements," the

identity of movers and actors in two situations is a sufficient guarantee of the sameness of two situations.[34]

Riker also tried to prove that the definition of cause entails both necessity and sufficiency. Two parts make this proof: 1) when the location of the terminal situation of event A (the cause) is identical to the location of the initial situation of event B (the effect), then A "at least contains" a necessary and sufficient condition of B; 2) if this is the case, A is no more than a necessary and sufficient condition of B. Therefore, he stated the following "theorem" of the identity of contiguous situations: "the statement A causes B if and only if the location of the terminal situation of A is identical with that of the initial situation of B is equal to say A causes B if and only if A is a necessary condition of B." (p. 288)[35]

Having shown that A is the cause of B, he also stated that A is no more than a necessary and sufficient condition of B. Namely, to demonstrate that conditions of the identity of time locations, space location, and movers and actors between the terminal situation of A and initial situation of B are essential to the theorem mentioned above.

The advantage of this approach for the study of events in social sciences is that it can offer a standard and helpful definition of causality by separating two events A and B such that the location of the terminal situation of A is identical to that of the initial situation of B. Besides, suppose a situation can be the terminal situation of an infinite number of events. In that case, any given situation, which is the initial situation of an event B, can serve as the terminal situation of events $A_1, A_2, \ldots.., A_n$ each one is then a cause of B. Therefore, even if infinite, all the possible causes must obey the following conditions:

1. All causes of B have identical terminal situations
2. The movers and actors of all causes of B are identical with that of B
3. Each cause of B includes all and only the movers and actors of each other cause of B
4. Each initial situation of the causes of B includes all and only the movers and actors of each other initial situation of a cause of B.

In the concluding part of his paper, Riker also responded to some possible criticisms against his argument, namely the overlapping between causality and

34 Riker advanced a series of assumptions, which determine the more general definition of cause in the context of social sciences. Thus:

1. Two situations are identical if and only if their locations are identical
2. Two situations are identical if and only if their movers and actors are identical
3. Two locations of situations are identical if and only if the movers and actors of the situations are identical
4. If two locations of situations are identical, then two situations are identical (cf. Riker, 1958, p. 285 et ss.)

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

antecedence. Then, if it is true that the first implies the latter, the real problem, to escape logical fallacies, is to restrict the kinds of antecedence that are permitted, as implicit in the definition of causality that he set forth.

However, some part of Riker's arguments seems questionable.

First, in both papers, he is not defending a particular view of science based on the importance of deductive arguments over inductive, or vice versa. Therefore, he did not discuss such issues as the role of models, their foundations, or the different degrees of explanations. Furthermore, the main difference between social and natural science seems to lie more in the distinct methodologies employed than in substantive differences regarding the object of analysis. The development of natural sciences, to which neither comparable has happened in the social sciences, can be explained, for the vast part, by the fact that natural scientists have restricted their analysis only to these aspects that show a similar pattern. These correspond to small events, which are "less subject to ambiguity than large events: small events can often be precisely bounded; and, failing that, statistical techniques can often be used to resolve such ambiguity as remains." (Riker 1957, p. 69)

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

From the historian of science's (or philosopher's) point of view, Riker's arguments can display a simplistic knowledge of natural disciplines and their methodology. For instance, even if some natural sciences have indeed started by focusing on small events, in the most advanced fields, like non-classical Physics, in those years, the general framework was that of the general analysis of phenomena to be explained (think of relativity and quantum physics).

It is most interesting to relate Riker's analysis with the coeval developments of Economics, especially given the later references to it as a role model.[36] Again, Riker's view seems to miss the point of what concerns this discipline, too. In his 1957 paper, he seemed to treat Economics implicitly as part of the social sciences. Therefore, the same methodological recipe could apply, i.e., to properly define the events to be investigated and start with the small events. However, in the same years Riker was writing, the methodological revolution in Economics, provided by mathematical modeling, was occurring. Contrary to Riker's recommendation, the most important theoretical results obtained in the decade encompassed the existence of General Economic Equilibrium, a result close to the unified generalizations of physical phenomena for many aspects. As the further development of economics will show, such a result does not make the analysis of small-size phenomena useless. Nevertheless, at the same time, its validity rests on its internal logical consistency and not only on some superior and precise explanatory power.

This discussion could suggest two possible interpretations of Riker's attitude toward Economics. First, as he was not an economist and was unaware of theoretical developments in that discipline, he had a simplistic view of them. Such a view could be confirmed, I will show, by leaning toward the kind of instrumentalist approach, a philosophical argument far from representing the mainstream

36 see below

view among those economists interested in the philosophical foundations of their discipline.

However, the second interpretation should underline Riker's interest in GT. Then, when referring to Economics, perhaps what he had in mind, as an outsider, was the mixture between the undergraduate education he received and what he understood reading TGEB. Then, the focus of the discipline should start, as in Morgenstern's expectation, with the analysis of small-size phenomena. Apparently, even some decision theorists shared this view in the 1950s, especially those involved in the experimental and psychological study of utility theories.

4.2.2 *"Does the Political Man seek to maximize 'power'?"*

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

THIS IS THE ABSTRACT of the first paper Riker published about GT in Political Science. In the author's words, this paper represented "an attempt to estimate the adequacy of an important assumption in the new formal or mathematical political science." (Riker to Tyler, June 22, 1959, WHRP, Box 10, Folder 1) This assumption pertained to the so-called "Shapley and Shubik's power index," a theoretical result that, as seen, he alleged had a great influence on him. (Riker 1992; Shapley and Shubik 1954). Therefore, perhaps not surprisingly, his first work devoted to GT aimed to test their method using the analysis of a legislative body's effective working. (Riker 1959a)

Empirical testing of GT concepts started at RAND yet in the early 1950s. For instance, an intensive round of laboratory experiments involving different individuals and concerning Cooperative games, with a focus primarily on bargaining, negotiation, and coalitions, was conducted in the summer of 1952 by a group of RAND scholars, which also comprised John Nash. (Kalisch et al. 1952) Other laboratory experiments focused on Non-cooperative games, for instance, the Prisoner's dilemma or opposition of interests.[37] Furthermore, these studies were paralleled by those conducted in psychology to test the assumptions regarding the new-coming axiomatic theory of utility and decisions. (Moscati 2018)

Riker was aware of such empirical testing in the second half of the 1950s. He also tried to experimental test some Cooperative games regarding coalitions, even in the 1960s. However, only after his appointment in Rochester was he able to find the necessary resources to conduct these tests properly. (Riker 1970)[38]

37 More in general, on the history of Experimental Economics see: Maas and Svorencik 2016; V. L. Smith 1992.

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

However, his empirical testing of Shapley and Shubik's index was not a laboratory experiment but a test conducted against a set of data he could collect regarding the behavior of the French National Assembly legislature (since his teaching duties at Lawrence also comprised a course in Comparative Politics. Riker and K. Shepsle 1979, p. 54) Besides, contrary to set-valued solution concepts, like the stable set, which was difficult to detect, Shapley's and Shubik's was simply a numerical value, relatively easy to compute.

Neither Shapley nor Shubik was a political scientist. Lloyd Shapley was a Princeton mathematician, and he was personally and intellectually very close to John Nash, other than Martin Shubik. He spent the vast part of his career at RAND, mainly contributing to the development of GT, mainly Cooperative GT.[39] Among the impressive list of his contributions to GT, Shapley related his name to the value named after him, a general solution for n-person games, with transferable utility and binding agreements. Unlike von Neumann and Morgenstern's "stable set" and the "core" (see the previous chapter), which also applies to similar games, the Shapley Value is not based on stability considerations. Instead, it entails the players' "reasonable expectation of reward," based on an apriori evaluation of the entire game. Namely, the value added to every coalition by a player is multiplied by the a priori probability that the coalition will form. The players' Shapley values constitute a unique payoff vector as the game's solution. (Shapley 1953; Roth 1988; Taylor 1971)[40] Martin Shubik was instead a Princeton Ph.D. economist. Together, he and Shapley offered many results and applications of Game Theory to economic problems, whereas Shapley was the mathematical mastermind and Shubik was the social scientist. (See: https://www.informs.org/Resource-Center/Video-Library/H-T-Videos/INFORMS-History-and-Traditions-Interview-with-Martin-Shubik)

Among the first issues they addressed together, they dealt with the everlasting political issue of power. However, they did it innovatively, focusing on a narrow and "technical definition" derived from Shapley Value, and their brief (and not formal) paper was readily accepted and published in the leading journal of political science, *The American Political Science Review* in 1954. As Riker described their result:

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

Indeed, they defined "power" as the chance each member of a committee has of being critical to the success of a winning coalition. (Shapley and Shubik 1954, p. 787) The authors conceived their method as a first step to addressing such

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

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

problems as designing the size and type of legislative bodies, protecting minority interests, and even finding a criterion for "fair representation." Their analysis does not consider any sociological or political superstructure in a legislature. Nevertheless, this can be helpful in setting up norms or standards, "the departure from which will serve as a measure of, for example, political solidarity or regional or sociological factionalism in an assembly." (p. 791) Indeed, this index represents the apriori chance for each committee member to be pivotal for a minimal winning coalition. It also means that the marginal person in a coalition may expect a significantly greater portion of the total payoff of that coalition than the ordinary member. One can interpret Shapley and Shubik's power index as a "technical definition of power" compared to the popular analysis of power, namely, the ownership and use of resources or the act of domination or influence of some individuals over others.

In a formal, although not axiomatic way, their model can be summed in the ensuing terms:[41] given a voting body of n-members and a rule to define victory in a vote (for instance simple majority rule, $\frac{(n+1)}{2}$ if n is odd, or $\frac{n}{2}+1$ if n is even), members vote according to a sequence. Letting $n!$ the total number of these sequences[42], and defining a Minimal winning coalition, that is a coalition such that, if one member is subtracted, then it is not winning anymore, the member who transforms a coalition from losing to winning in a sequence is said to be a "pivot." This means that the marginal value of the vote after $\frac{n}{2}+1$ is zero.[43]Therefore, concerning each member i, the power index P_i is the ratio between the number of sequences in which i pivots and $n!$. Furthermore, $\sum_{i=1}^{n} P_i = 1$.

Think of a group of individuals who must vote for some bill in a given order. The bill is passed as soon as a majority is reached, and the last member who voted is given credit. If the order of voting is chosen randomly, one can compute the frequency with which a member belongs to a group of voters, and the frequency of a member is pivotal. Then an index can be construed, which measures the number of times the individual's action changes the state of affairs.[44]

Shapley and Shubik also delineated some properties of their power index, even if, perhaps given the expected audience (namely political scientists and not mathematicians), they did not offer formal proofs: first, in a pure bicameral system using simple majority voting, each chamber has the $\frac{1}{2}$ of the power, so then power is inverse to the size, and the smallest bodies are most powerful; second, the power division in a multi-cameral system depends on the type of majority required to pass a bill. Raising a majority in one chamber increases the relative power of that chamber. On the other hand, if unanimity is required in every house (i.e., each individual has veto power), each individual is equally powerful to the others, and the power index of his chamber is proportional to his size.For instance, take system consisting of a governor and a council, which have to approve a bill before it can pass. If the council needs unanimous voting, the

41 This is the summary made by Riker in his 1959 paper.

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

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

44 If this formal model is applied to a committee chairman's tie-breaking function, in an odd committee, he is pivotal as often as an ordinary member, in an even committee, he is never pivotal. Then, for instance, applied to the case of the US Senate in the 1950s, the power index of the US vice-president was equal to $\frac{1}{97}(96+1)$

governor has no more power than a simple member. If a simple majority rule is adopted, the governor has n-times the power of each councilman (where n is the number of members). If only one member's approval is required, each member's chance to be pivotal is reduced, and the power turns out in favor of the governor.

The two authors explicitly stated that this index could be empirically tested (for instance, by looking at the voting record of each member of a committee), weighted for all votes, and the results compared with the theoretical expectations. This test is what Riker tried to do in the paper published in 1959 on *Behavioral Science*. Then the index was used to study changes in party affiliations in the second legislature of the French National Assembly and tested against a strong hypothesis regarding the behavior of the members of a legislature.[45]

In this paper, the fundamental hypothesis was that of maximization of power. The members' behavior who changed sides in a legislative assembly was determined by the attempts to raise their power. Then, Riker expected Shapley and Shubik's index to rise after migration from one party to another.

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

Therefore, Riker strongly associated rational choice with the assumption that a rational political man seeks to increase his power. However, neither Shapley's solution nor Shubik and Shapley's index envisaged such a strong individualistic assumption regarding rational choice. Indeed the Shapley Value has been derived axiomatically, without "commitment to the assumptions regarding rational behavior embodied in the von Neumann & Morgenstern notion of "solution." It refers instead to the abstract games, that is games "played by *roles*-such as 'dealer' or 'visiting team' - rather than by *players* external to the game." (Shapley 1953, pp. 31–2) Shapley and Shubik did not explicitly assume a power maximization hypothesis. Instead, their analysis was just a convenient conceptual device; therefore, the power index they computed represented an essential element of the committee system itself.

To address the issue of rational behavior as power maximization, Riker built a model based on weighted majority games, namely games where voters' power and weight differ.[46]Then, he investigated whether or not people in such a

45 This assembly had three features which were fit to Riker's hypotheses: more than two parties; the parties have a strict discipline; there are relatively frequent migrations among parties by a substantial number of members.

46 Riker's example is the following: a 3-person game where the players a, b, c are weighted respectively 50, 49, and 1. Given six possible voting sequences, the difference with simple majority games is that a majority of 2 out of 3 represents a winning coalition in the latter. The second voter in the voting sequence is ever pivotal. Therefore, P_i for each member corresponds to $\frac{1}{3}$. However, for a weighted majority, where the minimal winning coalition needs 51 votes out of 100, c and b are pivotal only once, whereas a is pivotal four times. Then, $P_a = \frac{2}{3}$, $P_b = \frac{1}{6}$ and $P_c = \frac{1}{6}$.

weighted majority game try to increase their power index. It does not entail people's effective ability to compute their power index. However, the author presumed that if they "sense gains and losses in bargaining opportunities, [...] note with envy or satisfaction that their colleagues have more or fewer opportunities and [...] behave in such ways as might increase or maintain their opportunities." (Riker 1959a, p. 122)

More specifically, the hypothesis to be tested was to confirm if the power indices of the members who changed their coalition (namely party affiliation) in the legislature, raised. Assuming that the power of each $1, \ldots, m$ member of a party A is $P_i = (\frac{P_A}{m}, G_\alpha)$, that is, a function of the total power of that party, divided the number of members, and the weighted majority needed to win, if he decides to change its affiliation, joining party B, its power becomes now $(\frac{P_B}{m}, G_\beta)$. Then, its eventual gain, or loss is $R_j = (\frac{P_A}{m}, G_\alpha) - (\frac{P_B}{m}, G_\beta)$ ($j = 1, \ldots n$ is the total number of legislators who change their parties). Therefore there are at least two main cases:

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

Given at least four possible hypotheses:

1. Legislators increased their power consciously
2. Legislators increased their power by chance
3. Legislators consciously decided the reject favorable outcomes (for instance, changing sides for ideological reasons)
4. Legislators decreased their power by error

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

These hypotheses were tested against data from the second legislature of the French National Assembly, focusing on two years (1953 and 1954), totaling 34 party changes involving 61 individual changes and 46 members. (Riker 1959a, pp. 124–8) Unfortunately, the final result was ambiguous at most.[47] He advanced three possible interpretations for this outcome.

The first and perhaps most destructive is that the apriori index is irrelevant to the case under examination because it is a simple algorithmic procedure and does not entail strategic manipulations. For instance, a party could belong to a "quasi-permanent" winning coalition. Then, the index does not consider the real-world dynamics of party politics and the institutional arrangements that rule a parliamentary assembly. Thus, "whether or not this explanation is appropriate for the results obtained depends upon empirical investigation to determine the existence or non-existence of quasi-permanent coalitions." (Riker 1959a, p. 129) Even Shapley and Shubik, in their short paper, conceived such a possibility.

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

The second explanation is that politicians in a legislature, even if willing to increase their power, cannot do so since they cannot compute advantages and disadvantages. This idea resembles the critiques of the notion of substantive rationality, based, for instance, on the impossibility of setting forth the kind of advanced computations required for this aim. Finally, Riker also stated that people who change their party affiliation are "truly indifferent to power considerations." Then, if, as Riker did, although for explanatory reasons, rationality is related to power maximization, then "behavior motivated by ideology must be regarded as irrational." (Riker 1959a, p. 131)

However, the final result of his paper was not disappointing in the author's eyes. Although the Power index did not explain the specific case Riker took under exam, it offered a solid basis for the scientific explanation of politics, starting from rejecting such hypotheses that did not conform to data. Then, for instance, even the lacking of "rational behavior" that fits that envisaged by the index could be considered a fruitful analytical result, at least in the study of large legislative bodies with many parties.

Furthermore, Riker was aware that his empirical testing was not sufficient to discard the Power Index at all. Indeed, he returned to this topic in different papers, working on "weighted voting" with Lloyd Shapley in 1966 (Riker and Shapley, 1966) and applying the power index to a model of coalition formation in a paper written with political scientist Steven J. Brams. (Riker and Brams 1972)

Besides, this early work on Shapley and Shubik's result had at least a twofold effect on Riker. First, in the work of political coalitions, he rested on the original solution concept of von Neumann and Morgenstern. Moreover, he rejected the idea that rationality in political theory could be built upon the notion of maximization of power, preferring instead to focus on the preference for winning. On this fundamental assumption, Riker developed his theoretical argument in *The Theory of Political Coalitions*. (Riker 1962b)

5

LOOKING FOR "A GENUINE SCIENCE OF POLITICS": RIKER'S 'THE THEORY OF POLITICAL COALITIONS'

RIKER'S *The Theory of Political Coalitions*, published in 1962 by Yale University Press, was the foremost product of the author's intense commitment to game theory and formal analysis.[1] Only in the 1980s Riker published other such general works, although his focus shifted from game theory to social choice analysis, political theory, and American history.

TPC was an extraordinarily ambitious enterprise that aimed to construct, using an "existing general theory of coalitions (the theory of n-person games)," a theory of coalitions useful in studying politics and that rested on exact and verifiable assumptions. (Riker 1962b, p. viii) It is also a work hard to read, both for the modern reader and perhaps even more for the coeval reader. Indeed, its formal analysis is too much verbal, and for this reason, the proofs of Riker's statements are difficult to be grasped. By comparing it with von Neumann and Morgenstern's pages, which are much more demanding to the reader, in Riker's, there is little similar to the excitement a young scholar, comfortable with mathematical analysis, could have felt following von Neumann's exact reasoning. Instead, the former's treatment rests on a poor and sometimes superficial employment of game theoretical notions. A point that was apparent yet to a reader like Morgenstern, who, despite being the co-author of TGEB, was, as seen, far less mathematically inclined than the first cohort of Princeton game theorists.[2]

Nevertheless, Riker's attempt deserves much praise. 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. It is the notion of "minimum winning coalitions" (from which he obtained the "size principle"), 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 many disputed Riker's results on theoretical and empirical grounds (see the next chapter). 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 created. In this part, the author's theoretical ambitions are even less fulfilled than in the first part, mainly because the points addressed were far beyond his technical capabilities. However, even there, one can find valuable insights into the effective working of political systems. Moreover, the first chapter is entirely devoted to exploring some methodological implications of his analysis and presenting the main features of his model to the reader. Then, its technical limitations notwithstanding, TPC represented the first significant attempt to develop a full breadth of game-theoretical analysis in political science.

Riker was very explicit about the nature of his work. His analysis systematically exploits von Neumann and Morgenstern's theory of n-person games. Still, it is not ("most emphatically not." Riker, cit. p. vii) a book about mathematics because

1 From now on TPC

2 See the second chapter of this dissertation. Moreover, On Morgenstern's authentic contributions in the development of the Theory of Games, see R. Leonard 2010; Schotter 1992; Morgenstern 1976b

he limited himself to employing and adapting some mathematical notation without offering formal proof of his statements. 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). Therefore, it does not occupy a central place in the history of the development of GT *qua* theory. However, it had an essential role in the history of how game theoretical ideas crossed domains different than economics.

In this chapter, I will discuss the content of TPC, focusing mainly on how Riker employed GT and reconstructing the relations between Riker and Game Theorists at that time. My conclusion will be that Riker's high theoretical ambitions were not entirely fulfilled, especially given his analytical limitations. Indeed, even if he had some exciting intuitions regarding the possible developments of game theory in political science from a strictly theoretical point of view, Riker did not provide new formal theorems. He shared this feature with other scholars that employed GT in the same period, like, Schelling (see the previous chapter). Besides, unlike the latter, Riker's use of GT was occasionally based on misconceptions, and the tools he needed to fulfill his theoretical ambitions were sometimes far beyond his reach.

However, as I will explain, this does not undermine the importance of this work. Riker's adoption of Cooperative game theory, namely von Neumann and Morgenstern's original idea of the "stable set," was outstandingly original. Furthermore, his attention to the size of coalitions paved the way for an entire class of models adopting that "size principle." Finally, he assessed some weaknesses of von Neumann and Morgenstern's theory.

5.1 THE GENESIS OF THE WORK

5.1.1 *Riker and Game theorists*

IN THE PREVIOUS CHAPTER, I discussed Riker's academic training as a political scientist, his disaffection with the poor state of the discipline in his time, and finally, how he became acquainted with GT. This section will complement that narrative by highlighting Riker's early relationship with the community of game theorists.

As seen, Riker's only connection with the kind of high theory in social sciences that he was trying to elaborate on was the CASBS at Palo Alto, where he spent a one-year fellowship. In California, he met Arrow, Clyde Coombs, and he had exchanges with other companions, who helped him with some formal aspects of his analysis (like the statistician at the University of Chicago, David Wallace).

With very few exceptions (Riker's being the most significant one), those of political scientists and game theorists remained disjoint communities, with no element in common among them, despite attempts to employ game theory in political analysis starting yet in the first half of the 1950s. In a certain sense, as seen, this kind of approach is entailed in 1944 von Neumann's and Morgenstern's work, too. (Simon 1945; Shubik 1954) However, even Riker's membership in their intersection can be questioned, at least for the 1950s. Focusing on that decade, indeed, if he certainly was a member of the scholar community of political scientists, it is highly debatable that he belonged to game theorists due to his intellectual formation and professional activity. Furthermore, despite his theo-

retical inclinations and methodological concerns concerning political scientists, Riker did not join the Behavioral Revolution, the transformative movement in American Political Science, preferring to pursue a theoretical agenda on his own.

The game theorists community was made up, in the 1950s, of young mathematicians interested in the most conceptual developments of the latter more than in its practical employment. In places like RAND Corporation, the emphasis was allegedly on the applied strand of GT, e.g., its adoption in strategic and international political issues. In reality, RAND executives quickly funded even pure theoretical research. Unsurprisingly, Riker was an outsider in that community, lacking the advanced mathematical training to produce new theoretical developments. Riker was not a visiting member at RAND, where the few 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). Then, among the institutions which contributed to shaping the so-called "Cold-War Rationality," Riker was only a fellow of Stanford's CASBS.

One could partially explain the distance of Riker from game theorists with the earlier focus on a well-defined issue, coalition formation in politics, and within a well-defined framework, namely "Cooperative game theory" instead of "Non-cooperative." The latter was instead mainly adopted, especially in international relations.[3] One could also add that Riker was advancing his theoretical agenda in advocating GT. This agenda differed considerably from traditional approaches in Political Science and the most recent Behavioral revolution. Meanwhile, it was challenging to integrate GT developments due to Riker's mathematical difficulties. Therefore, it perhaps deepened his sense of intellectual estrangement among the political scientists of the 1950s.

Riker was not alone in explaining political phenomena through formal analysis (but not necessarily game theory) derived from Economics. Indeed, as seen, the same period saw the works of such authors as Duncan Black, Anthony Downs, James Buchanan, and Gordon Tullock. They were followed in the 1960s by the outstanding community of scholars who attended the political science graduate program at Rochester and in such places as Carnegie Mellon, where Otto Davis and Melvin Hinich worked extensively upon the Downsian notion of Spatial Electoral Competition. With all of them, Riker came to develop a robust intellectual bond. Still, the growth of this scholarly community dates back years after Riker's early commitment and began after his appointment at Rochester (see the next chapter).

His reminiscences in the yet-mentioned interview with Shepsle can offer insights into his relationship with the game theorists' scholar community. Thus, asked if he ever sent any of his ideas to contemporary game theorists, Riker remembered only Duncan MacRae, whose response filled with detailed criticisms is not, unfortunately, among Riker's papers stored at Rochester.[4] However, the historian can find interesting details of his being an outsider among the community of game theorists in Oskar Morgenstern's papers at Rubinstein Library (Duke University). (Morgenstern n.d., Box 83), Although Riker did not mention it in the interview with Shepsle, he sent his manuscript of TPC both to the Princeton University Press and the Yale University Press. While YUP accepted it and sent it for anonymous refereeing to Martin Shubik, a former Morgenstern student at

3 See the introduction to the previous chapter

4 https://rbscp.lib.rochester.edu/finding-aids/D262

Princeton, Morgenstern was highly critical, rejecting his publication. (Shubik n.d., Box 8)

In a letter he sent to Gordon Hubel, the press editor of 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) To remark on his point, he concluded that: "I am sure that anyone else who is at home in Game Theory and who would see this manuscript, perhaps given to him by some other publisher, would come to the same conclusion." (Morgenstern to Hubel, cit.)

Despite Morgenstern's harsh criticism and last remark, Shubik's referee was more supportive. In the end, the manuscript was published. As it will be apparent in the course of my narrative, this did not enhance Riker's reputation among game theorists, lacking those features that appealed to them. However, his analysis clarified to political scientists that a new approach to model political phenomena and develop a new kind of political theory was possible.

5.1.2 *Martin Shubik's referee report*

SHUBIK'S papers, stored at Rubinstein Library, also contain the original referee report of the manuscript of Riker's TPC, which the economist was asked to write. Although this note is not very detailed from a theoretical point of view, it still has valuable remarks.[5] Besides, also, what is missing can help us to elucidate some aspects of Shubik's and Riker's thoughts about political GT.

Yale University Press contacted Shubik at the end of June with the proposal of reading and reviewing (anonymously) Riker's manuscript. (Marian Neal Ash to Shubik, 30th June 1961, MSP, Box 8)[6] For many aspects, the request made by YUP was not surprising. Indeed, Shubik was one of the few people involved in elaborating n-person Game Theory. Furthermore, although he divided his research activities between RAND and his work as an applied economist for important corporations (especially IBM and General Electric), he was also associated with Yale University. Indeed in 1961, he was visiting there, and he became a full professor in 1963, remaining at Yale for all his career. Furthermore, Shubik was also known for his work with Lloyd Shapley about the Power Index.[7] Finally, he also edited in 1954 the brief volume that was a collection of different essays, or part of them, by people like Marshak, Wald, Black, and Arrow about game-theoretical analyses of political behavior. (Shubik 1954)

5 Indeed, it consists of only four typewritten pages, with a general introduction, some minor comments, with few exceptions, about style, and finally, a summary.

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

7 See the previous chapter

In the exchange with the staff of YUP, Shubik did not decline the offer, stating even that he probably knew yet part of Riker's work and perhaps even some earlier drafts of the manuscript. (Shubik to Marian Neal Ash, 17th July 1961, MSP, Box 8) However, on this latter aspect, Riker's interview does not offer any reference. Despite some travel necessities, he sent his referee's report on 9th August. ("Referee's report on 'The Theory of Political Coalitions' by William H. Riker," 9th August 1961, MPS, Box 9) As explained in his earlier reply to YUP, his swift response was facilitated by his being yet acquainted with some parts of Riker's work.

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

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

Focusing on the detailed comments on manuscripts, the only substantial analysis advanced by Shubik is about what seems to him too much emphasis on the zero-sum model. To the economist, much of Riker's analysis would also hold in a non-zero-sum game, mainly because the indeterminate temporal length is the most crucial feature of his game-theoretic analysis. In contrast, zero-sum is well defined only for a small finite part of the game. Instead, the main feature is the opposition of interest, which is a prerogative of a zero-sum (and constant sum) game and in a "more or less strictly competitive non-constant sum game."(Shubik 1961, p. 2)

Other comments are less substantial, although still interesting, like contesting that "Side Payments" has an underlying reference to money, as stated by Riker (although Shubik admitted that this is a common misconception of Game Theory). Still related to this is the remark that "to include threats as a form of side payments leads to a rather poor modeling of human affairs." Because "the result of a threat may be a rather low personal payoff to an individual. However, the threat itself is more properly a part of a strategy". (Shubik 1961, p. 4)

Shubik recommended Riker's manuscript for publishing, even with only editing corrections. He also advanced some odds about this work's reception: "The odds are that it will receive very mixed reviews, including several very favorable and several highly unfavorable comments. It is certainly not going to make any "best seller' list. However, it should make a worthwile (sic) contribution to the development of political science." (Shubik 1961, p. 4)

As stated before, Shubik's name was not attached to the referee report. Besides, there is no sign of correspondence between Shubik and Riker before and even after the refereeing process. Moreover, given the vagueness of the comments and

the fact that they were attached to the manuscript version of Riker's book, it is not easy to detect if they were effectively accepted by the author, except for the inclusion of "threat" as a side payment. In the published version of his work, "the threat of reprisal" is still maintained among the various kinds of side payments in politics. (Riker 1962b, pp. 109–10)

Notwithstanding, this report is interesting for many reasons. From the historian's point of view, it represents a convincing appraisal of Riker's work, made by one of the leading game theorists of his time. Furthermore, Shubik was perhaps the better choice to evaluate Riker's work. As he repeatedly stated, even in his later years, his leading talent rested more on being an economic model builder than a mathematical economist. Then, maybe, some other author could have focused his attention on the mathematical flaws of Riker's work rather than, as Shubik did, on its general structure. For instance, another reviewer could have pointed out that coalition theory was being tentatively employed, with an extremely high degree of mathematical sophistication, to develop existence and "stability" results in the General Economic Equilibrium "research program." [8] But probably, such criticism would have totally missed the point. Riker aimed not to offer a sophisticated mathematical theory of political coalitions or to elucidate their mathematical features. Instead, he wanted to provide new insights into political processes by modeling rational behavior following the methodological considerations explained in the first chapter of his work. Shubik, although distant from Riker's concerns (at least, from a disciplinary point of view), understood this perfectly, as his referee demonstrates. An issue which, instead, apparently was not caught by Morgenstern.

5.2 THE ASSUMPTIONS OF RIKER'S MODEL

5.2.1 *The "main hope for a genuine science of politics"*

RIKER DEVOTED the first chapter of TPC, significantly titled "The prospect of a Science of Politics," to present both the methodological assumptions of his work and the main features of his model. These pages can be seen as a complement to the author's previous philosophical papers, encompassing even a general discussion about formal modeling and the idea of rationality.

According to the author, this chapter "is the most important part of the book." (Riker and K. Shepsle 1979, p. 15) Such a statement sounds quite surprising, given the theoretical ambitions of Riker's formal analysis. Then, it can be interpreted as an implicit admission of the author's analysis's mathematical weakness. But also as a remark on what in his analysis is essential: the role of rational choice. As Riker stated, "the notion that people make calculations about what is good for themselves, and try to act based on those calculations." (Riker and K. Shepsle 1979, p. 16) Downs, Buchanan, and Tullock, have advanced a similar argument concerning rationality in politics. However, Riker's analysis of rational behavior also entailed a complete discussion of GT in politics, going over the "self-interest axiom" (Downs) or the "economic nexus" (outlined by Buchanan and Tullock).

He began with a somewhat classical reference to Physics as a role model for every theoretical and applied science. Physics' main feature, in Riker's eyes, lies in its consisting of a "body of related and verified generalizations which describe

8 Moreover, even Shubik himself was involved in these debates. See Cogliano 2019

occurrences accurately enough to be used for prediction." (Riker 1962b, p. 3) To utter predictions, one can deduce this generalization by a set of axioms and verify (despite some difficulties) by experiments and observations.

In Riker's view, different from natural science, social sciences face three enormous difficulties instead of the natural sciences. First and foremost, the normative considerations that embed any attempt to elaborate a positive analysis of human affairs. The main consequence of this normative stance is the impossibility of any prediction. The second and third difficulties entail those elements yet referred to in his previous philosophical papers, the size of events, and the notion of causal determinism. Then:

> "human action is itself enormously more complex than the motion of things [...] To make matters worse [...] our verbal patterns usually present social reality to us in great big slices. Thus the primitive physicists, even prior to the development of an elaborate special vocabulary, were still presented with rather small events to study [...] Primitive social scientists (that is, we of this century, who are just beginning to develop a special vocabulary) are, on the other hand, presented with vast events such as wars and depressions, love affairs and character formation, elections and systems of Jurisprudence, etc. These classes of events are doubtless of great human interest, but they do not admit of that precise definition which is so necessary in science". (Riker 1962b, p. 5)

To create a science of politics, being able to understand the scientific method is an essential aspect.[9]Following the modern developments of natural sciences and some social sciences, the primary tool to reach the scope of making Political Science scientific encompasses the "creation of a theoretical construct that is a somewhat simplified version of what the real world to be described is believed to be like." (ibidem) In other words, the creation of models, using a set of axioms (more or less justifiable intuitively) from which one scholar can deduce not obvious general sentences. These deduced propositions, when verified, become both an addition to the model and a description of its nature. Besides, they also allow to eliminate, or at least try to, the normative elements and simplify the units of study.

> "The main advantage of a model is that it is a convenient way of generating hypotheses and something of a brake on inconsistency. Not that a model is any substitute for creative imagination [...] but the model can guide him in imagining hypotheses and deciding whether or not they are useful. Beyond this main purpose, however, models are helpful in overcoming the special obstacles that stand so firmly in the way of a science of politics" (Riker 1962b, pp. 7–8)

Economics, alongside Psychology, represented Riker's model for scientific social sciences. This discipline has elaborated models of individual behavior, like consumer theory, so an analogous model of political behavior can be developed. And this is the "main hope for a genuine science of politics." (p. 9)

Following the economic approach, political scientists must ground the scientific analysis of politics on individual action. However, the first step to building such a theory necessitates a proper definition of politics. To Riker, an appropriate

9 As Riker wrote: "Those who are interested in creating a science of politics must, therefore, first become students of the scientific method in the hope they can use it in their own concerns." (p. 7)

definition was that envisaged by his former colleague at the Harvard Graduate School, David Easton[10]: politics as "the authoritative allocation of values." (Easton 1953; Riker 1962b, p. 10) Methodological divergencies notwithstanding, according to Riker, this definition embodied, through the notion of "allocation," one fundamental feature: politics as an action and not simply a static study of forms of government, law, or history. Moreover, this definition does not reject all the older traditions in Political Science but subsumes them.

Most interesting, Riker also advanced the parallelism with the well-known definition of economics proposed by the famous British economist Lionel Robbins in 1932 (although he did not refer to Robbins in the text), i.e., economics as "the allocation of scarce resources." (Robbins 1932) This parallelism comes through the idea of both Economics and Political Science concerning "allocation." However, "allocation," to Riker, refers not to a simple physical process but instead to the "social process of deciding how a physical process shall be carried out." (Riker 1962b, p. 11) Political Science, in this sense, pertains to the group of disciplines that study decision-making.[11]

Still, not all decision processes are political or of concern to political scientists. Therefore, he classified the "authoritative allocative decisions of values" as follows:

1. the decisions made by individuals
2. the decisions made by group, which are divided in:
    - the decisions made by a conscious process
    - the decisions made in a "quasi-mechanical way"

Political Science is all about conscious group decisions. In contrast, the first case is the limit case of a single decision-maker, a dictator. The "quasi-mechanical" group decisions are instead those proper of markets and, subsequently, the subject of economic analysis.

The presence of a group of decision-makers also means that, if the total size of the group is greater than 2, this generates the process of forming coalitions: "[T]he process of reaching a decision in a group is a process of forming a subgroup, which, by the rules accepted by all members, can decide for the whole. This subgroup is a coalition." (Riker 1962b, p. 12) For the analysis of coalitions, "a model is at hand," namely von Neumann and Morgenstern's theory of n-person games, "which is essentially a theory of coalitions" (ibidem).

Its assumptions are Rational Choice and Zero-Sum, which are also the assumptions of Riker's model of political coalitions. Therefore, he devoted the last part of the chapter to their explanation. "Zero-sum" refers to pure conflict situations. These situations, as stated by Buchanan and Tullock in their *The Calculus of Consent* are rare in political affairs, as well as less attractive because one of the essential aspects of political situations is "that people consent to remain in them, even when they are on the losing side in particular decisions." (Riker 1962b,

10 Although the latter remained distant from the formal approach endorsed by Riker. See the first chapter

11 This point resembles what Duncan Black said about the unity of politics and economics. See (Black 1950)

p. 30)[12] But the zero-sum condition is a realistic assumption in many political issues, such as elections and voting, where there is a winner and a loser. Indeed, this condition also applies to many situations, whereas non-zero-sum situations can be perceived as pure conflict by the individuals involved. However, unlike rationality, the zero-sum condition is not an essential aspect of any formal analysis of politics. Therefore, it is rationality that Riker explored extensively in his pages.

5.2.2 *Rationality and the study of politics*

THE ECONOMISTS DEALT WITH the issue of individual and collective rationality, and their proper definitions, since the modern development of their discipline. The idea that became prominent after the Second World War was the "consistency view" of rational action. Following game theorist and philosopher Ken Binmore, this represents the framing of those features which characterized individual action, according to classical and modern philosophy, into a mathematical and logical fashion:

> "[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 2015, p. 4)

This view was strengthened and favored by adopting the notion of "revealed preferences" in the late 1930s, which assume that to infer the pattern of choice of different individuals, a researcher must rest upon only their effective decisions and nothing more.[13] Finally, such a "consistency view" found its most significant development in the axiomatization of the utility theory, following von Neumann & Morgenstern's work.

Luce and Raiffa, Riker's primary source concerning GT (other than TGEB), devoted many pages to presenting utility theory, its axiomatic foundations, and its critical points. In the pages of TPC, where he focused on these issues, Riker contended, in particular, their definition of rational behavior. The two decision theorists viewed "the postulate of rationality," or the "law of behavior," based on that, as an entirely tautological concept since it does not describe behavior but only defines preferences. Thus, "the problem is not to attempt to verify the postulate but rather to devise suitable empirical techniques to determine individual preferences." (Luce and Raiffa 1957, p. 50) It means that the theoretical puzzle is not to prove that men want to maximize money, power, or something else. Instead, it is sufficient to represent the structure of the preference properly and, therefore, examine them empirically. As the two authors stated: "Of course, if one attempts to identify utility with some objective measure of the outcome, such as money, then people are not generally rational in the sense of satisfying [the postulate of rational behavior]. But this is irrelevant; it merely implies

12 Buchanan and Tullock's work (see James M Buchanan and Tullock 1962) was published the same year of Riker's work, and to it, the latter devoted a review published on the *Midwest Journal of Political Science*. See Riker 1962a

13 For a general overview, see: Giocoli 2003b; Moscati 2018

that the preference patterns of people are not simply related to expected money returns." (Luce and Raiffa 1957, pp. 50–1)

Riker summarized 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*" (Riker 1962b, pp. 18–9. Italics in the text)

However, in his eyes, such a tautological form also represented the main disadvantage of this approach. Indeed, if all choices are rational, it simply asserts the existence of alternatives and agents. Accordingly, if rationality is to help model political behavior, one must adopt the "cruder and already somewhat discredited" idea of a maximizing economic or political man. As a consequence, the problem of rational action and empirical validation becomes: "how can the rationality condition be stated in such a way that it is more than a tautology but not subject to the criticisms implied in those experiments which show that the scale of individual utility is not the same as a scale of money." (Riker 1962b, p. 20)

Letting apart the maximization of power, an idea that seemingly was not supported by empirical research and rested upon the ambiguity of the notion of power, Riker advanced a definition of rationality based on the notion of winning: "What the rational political man wants, I believe, is to win, a much more specific and specifiable motive than the desire for power." (p. 22). More specifically, this notion lies upon the following definition of rationality:

> *"Given social situations within certain kinds of decision-making institutions (of which parlor games, the market, elections, and warfare are notable examples) and in which exist two alternative courses of action with differing outcomes in money or power or success, some participants will choose the alternative leading to the larger payoff. Such choice is rational behavior and it will be accepted as definitive while the behavior of participants who do not so choose will not necessarily be so accepted."* (Riker 1962b, p. 23 Italics in the text)

Riker stated that not all behavior needs to be rational, but rational behavior is crucial for managing any economic or political institution. He labeled this the "summation" argument, which incorporates the idea that institutions should be treated as whole units. That is, like the markets are controlled by those economic agents who adopt a maximizing behavior, the same holds for the working of political institutions.[14] According to Riker, this "summation argument" is especially apparent in fiduciary relations, which represent the broadest way of governing affairs and acting in political and social situations. A fiduciary relation involves an agent and a principal, and the agent must act on behalf of a principal. Therefore, "as long as the fiduciary morality exists, there seems to be some justification for using models containing the rationality condition, at least until we can discover whether or not they are useful for economic and political science." (Riker 1962b, p. 28)

One could explain Riker's rejection of a preferences-based tautological argument by viewing it as an attempt to make sense of the idea of political rationality for the community of political scientists. Therefore, he preferred a more substantial definition over a purely mathematical approach because the earlier's

14 Note that Downs employed a similar procedure when he handled political parties as single units in his model of elections. (Downs 1957)

meaning could be easily grasped by an audience not comfortable with mathematical sophistication. The problem with this approach is that it seems not to fit well in the discussion about modeling Riker set forth. Indeed, the refusal of the preference-based approach pushed him toward a significantly weaker argument to defend the cogency of rational choice: even if not all agents are rational, the most important agents are (the "summation argument"). In doing so, the author seems to discard what rationality assumption in economics is, viz., one way to constrain the beliefs and desires people are allowed to have in order to make their actions explainable. However, he did not offer a valid alternative to explain individuals' actions *and* properly model them. Indeed, restraining the set of individuals assumed to be rational does not preclude the fact that modeling rational behavior requires strong assumptions concerning people's beliefs and preferences and their formal structure.[15]

Let us advance a parallel with von Neumann's theory characterizing rational behavior with prudence. Indeed, in his 1928 paper, and also in TGEB, rational behavior is represented by the "security payoff" given by the minimax strategy (Neumann 1928): a player does not need to know what his opponent's plan will be in reality, but if he plays the minimax strategy, he will maximize his expected payoff. This idea, as seen, solves the paradox of perfect knowledge that authors like Morgenstern and Hayek discussed, albeit in different forms. (Morgenstern 1976a; Hayek 1937) Like von Neumann, Riker focused only on one feature of human behavior: in his case, the preference for winning outcomes. Avoiding a too narrow idea of rationality (like, for instance, that "individuals' scales of utility [...] are isomorphic with the scale of some objective measure such as money or even power"), he expected that "rationality as winning" could establish the foundation of a theory of political coalitions based on von Neumann & Morgenstern's formal analysis.

If one interprets Riker's argument that way, it could be possible to understand why he chose it. However, as suggested above, it is not consistent with his thoughts regarding modeling. Indeed, there is no contradiction between a formal argument and an idea of rationality that encapsulates concrete human behavior features. Von Neumann's solution was criticized for equalizing rationality and prudence. However, it was not this aspect that undermined its acceptance, but the mathematical difficulty in proving a similar result for those situations (Non-cooperative and Cooperative) different from 2PZSG. A problem that the newborn idea of Nash Equilibrium overcame.

Similarly, Riker's non-mathematical argument defining political rationality did not contradict a more formal discussion. Indeed, a customary procedure followed in formal Political Science concerning spatial preferences and the voting models entails the idea that politicians maximize votes or plurality to pursue the rational aim of winning. However, the latter idea is framed in the notion of Euclidean Distance and, therefore, in a strong axiomatic and formal argument. To develop this idea, political scientists needed a more robust mathematical background than that Riker had at the beginning of the 1960s. (Otto A. Davis and M. Hinich 1966)

15 Besides, Riker implicitly adopted a preference-based idea of rationality when using the V solution of von Neumann and Morgenstern. Indeed in the notion of "imputation" on which the two authors built upon their V-solution, employed by Riker, the fundamental properties encapsulated the notion of individual and collective rationality in preference terms. (Luce and Raiffa 1957, pp. 192–3)

To conclude, Riker's treatment of rationality perhaps is just a device in line with an instrumentalist approach. Indeed, such an approach will come to the forefront again in his discussion of the "size principle." This approach followed Milton Friedman's famous argument regarding the realism of assumptions and the scope of the models, especially their predictive power. (M. Friedman 1954) Since economic agents (or firms, in Friedman's famous example) behave *as if* they were maximizers, therefore the rationality hypothesis is an adequate proxy for describing how they work. The same could be said for political institutions that "[...] select and reward with success behavior which is apparently motivated by the intention to maximize power." (Riker 1962b, p. 21) The closeness between Riker and these arguments can also be detected in his emphasis on verification and prediction over the advanced theoretical arguments elaborated within the community of highly sophisticated mathematical economists.[16]

Riker did not explicitly referred to Friedman in his work on political coalitions. However, an apparently straightforward instrumentalist approach to formal political science will persist even after developing more robust mathematical tools in the works of authors like Ordeshook, Otto Davis, and Melvin Hinich. (Otto A. Davis 1968; Otto A Davis, Melvin J Hinich, and Peter C Ordeshook 1970; Riker and Peter C. Ordeshook 1973).[17]There it was perhaps the main line of continuity between Riker's "quasi-formal" attitude and that completely formal of the scholars that followed him, in the same commitment toward "the prospect of a science of politics."

5.3 THE THEORETICAL MODEL

5.3.1 *The Size Principle*

THE FUNDAMENTAL PRINCIPLE of the theory of coalitions is about their size. Most specifically, thanks to GT, Riker restated it as follows:

> *"In n-person zero-sum game, where side-payments are permitted, where players are rational, and where they have perfect information, only minimum winning coalitions occur."*(Riker 1962b, p. 32, italics in the text)[18]

From this statement, a descriptive statement (or "sociological law") can be provided, namely the "Size Principle," which can be empirically verified: *In social situations similar to n-person, zero-sum games with side-payments, participants create coalitions just as large as they believe will ensure winning and no larger* (Riker 1962b, pp. 32–3. Italics in the text)

Riker offers shreds of evidence about this latter statement, mainly by referring to American political history, in the third chapter. Before, he devoted several pages to discussing the features of von Neumann & Morgenstern's GT he was employing.[19]

16 However, even if Friedman's classical argument was widely debated among economists and social scientists, there is no proof, in his work on political coalitions, that Riker knew it at those time. In any case, it is unlikely Riker did not know Friedman's work since Downs also refers to it.

17 On this point, more will be said later

18 From now on, in place of "minimum winning coalitions," it will be used the acronym MWC

19 The discussion in the main text is complementary to a more detailed analysis outlined in the first of the two appendices to his work. (Riker 1962b, pp. 247–78)

Riker's positivistic attitude about social sciences featured the possibility of knowledge cumulation. Accordingly, he viewed his theory about political coalitions as a partial improvement of the rational approach carried forward by Downs concerning the relationship between rational voters and rational parties. The parallel between the two arguments, advanced by Riker, focused on the analogy between parties and coalitions. Downs assumed that the parties' aim is the maximization of their votes. It resembles the behavior of coalitions that aim to maximize their membership. However, Riker focused on a coalition maximizing its membership only up to its minimal winning number.[20]Furthermore, his analysis does not entail a discussion on voters' and politicians' spatial preferences and the probabilistic distribution of the earlier.[21]

The idea of the "minimal winning size" was originally used by Shapley and Shubik in their GT paper analysis of power. However, Riker employed this notion differently from them. He did not compute each player's value to play a given game (in a nutshell, Shapley's original idea) or the power that each member of an assembly entailed. Instead, the purpose of this notion was to constrain the coalition structure of a game, explore its equilibrium and stability, and offer a way to test these results.

Riker began by exploring the details of the model, starting with a sketch of the theory of games based on Luce and Raiffa's famous textbook and J.C.C. McKinsey's, other than the verbal discussions contained in von Neumann and Morgenstern's first chapter. (Luce and Raiffa 1957; McKinsey 1952) In particular, besides a brief analysis of the rules of the games (the number of players, moves, set of alternatives, state of information, amount of collaboration allowed, and the structure of payoffs), he focused on the number of players. Then, he employed the theory of n-person games, where agreements are possible, and utility is transferable among players.

However, as Riker rightly noted, no unique solution exists for such games. In his view, this was due to the fact that too much emphasis has been put by game theorists on the properties of coalitions, like their existence, reasonableness, or fairness, overlooking instead the possibility of delimiting coalition structures directly.[22]Since among the different solutions that came after von Neumann & Morgenstern's, none appeared to him significantly preferable to the latter, neither in terms of empirical testing nor generality, Riker based his analysis directly on the original solution concepts conceived in TGEB.

The essential idea is that when the number of players is greater than 2 (naming N the set of all players), each coalition that forms in a game, from the coalition made up of a single player to the coalition of the whole set of players, has a value which is defined by its "Characteristic Function," $v(S)$ (where S is a set of players, $S \subseteq N$).[23]This value is the solution of a 2PZSG between this coalition

20 See below

21 This represents the pivotal contribution in Downs' analysis and will be at the core of the subsequent developments of formal political theories in the late 1960s, as well as the introduction of non-cooperative game theory from the 1970s onward.

22 To briefly sum up this point, note that von Neumann and Morgenstern's solution is allegedly never empty (ever exists) but is not unique; the core can be unique (but often non-existent); finally, the Shapley-value is a "fair solution," but players' strategic actions are often obliterated

23 By coalition, in formal terms, it is intended the number of all the possible subsets of n, also comprising the empty set $\emptyset$ and the set of all players. This number equals 2^n. Henceforth, the Characteristic Function will be referred to as CF.

and its opposite, that is, $v(S) = -v(S)$.[24] Each coalition splits its value between its members in the following way: none receives less than he would receive by forming a coalition alone, and only the value of the coalition can be split, no more and no less. The vector of the values for each coalition member is called "imputation." An imputation dominates another if all its components are greater than the other. Therefore, the solution of a n-person game is the set V of all imputations that do not dominate each other (inner stability) and dominate all the imputations outside the set (external stability).

Riker, however, introduced some restrictions on CF. Indeed, he focused only on those coalitions that were winning. A winning coalition is larger than some size, arbitrarily ruled (in this case, m, where $m \geq 1/2$). If there is a winning coalition, its complement is a losing coalition. Otherwise, if there is no winning coalition, all the coalitions are "blocking."

Von Neumann & Morgenstern made the CF workable for analysis by normalizing this value. Therefore they showed the "range" of possible values a coalition could assume, defined as $-p\gamma \leq \bar{v}(S) \leq (n-p)\gamma$, where n is the total number of players, and p is the number of members of each coalition. (Neumann and Morgenstern 1944, pp. 248–53).[25]

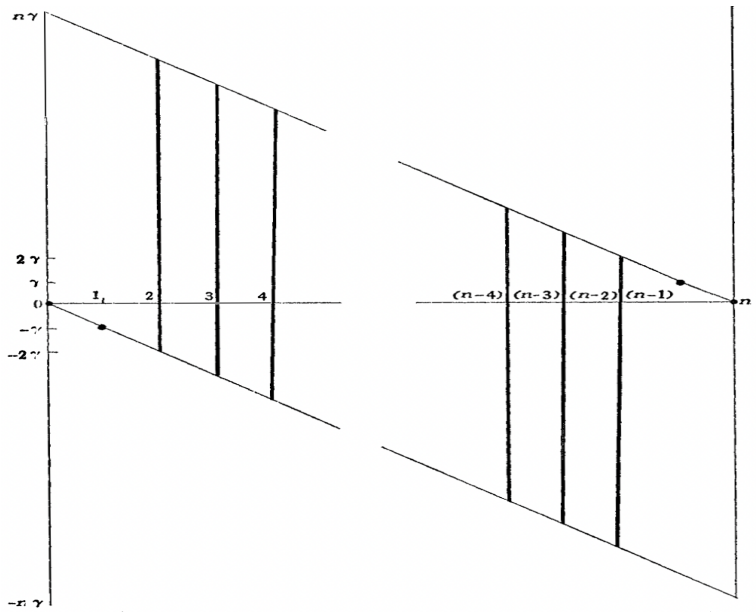

Figure 2: The range of possible values, $v(S)$ for each coalitions in the game

The graphical rendering of the possible values for each coalition in the game is shown in Figure 2.[26] The maximum each coalition can lose is $-n\gamma$, the maximum can gain is $(n-p)\gamma$, which, on a cartesian graph where, on the y-axis we found the value each coalition can obtain (comprised between $-n\gamma$ and $n\gamma$ and on the x-axis, the size of each coalition (from 0 to n), are represented by the lines connecting

24 The features of von Neumann a$ Morgenstern's n-person games are discussed in much detail in a previous chapter of this dissertation.

25 From this, it is apparent that each one-player coalition's value is $-\gamma$, and therefore the $n-1$-members coalition has value γ. The $n-1$-members coalition is the complement of the one-player coalition, and by the zero-sum property, $v(S) = -v(S)$.

26 Riker reports this figure in the first appendix of his work. This is the same graph in von Neumann and Morgenstern's TGEB. Riker 1962b, p. 253; Neumann and Morgenstern 1944, p. 252

$(0, n\gamma), (n, 0)$ and $(0, 0), (n, -n\gamma)$ As apparent, this range comprises infinite values. Accordingly, the only way to narrow this range is to focus only on those CFs representing the value of winning coalitions. Then Riker discriminated between winning, losing, and blocking coalitions by perusing the "rather imprecise" notion of majority. (Riker 1962b, p. 256)[27] Then $S_p \in W$ if and only if $m \leq p$ (where m represents the majority, p the size of a coalition, and W the set of winning coalitions). If n is even, then the value of m is greater than $n/2$. If n is odd, the value of m is comprised between $n/2$ and $(n+1)/2$.[28]

Riker restated von Neumann & Morgenstern's original model, in a more straightforward and less mathematically sophisticated way, to describe in a statical way the coalitional structure of a political situation modeled as a n-person game. However, he also interpreted such a structure in terms of equilibrium and disequilibrium. Each $v(S)$ can assume a range of values, which is a function of its size, and the size of the opponent "blocking" or "losing coalition." Riker's definition of equilibrium and disequilibrium directly reflects the strategy and the rational decisions of each agent in the coalitions:

> If there are some values of $v(S)$ so unnecessarily disadvantageous for S as a whole that rational players reject S in favor of an immediately available alternative T, then these values of $v(S)$ will be said to be in *disequilibrium* and S will be said to be *unrealizable*. Conversely, those values of $v(S)$ which are not disadvantageous in comparison with an immediately available alternative, will be said to be in *equilibrium* and S will be said to be *realizable*. (Riker 1962b, p. 262, italics in the text)

Riker focused his attention only on the winning coalitions, i.e., on the positive side, the "winning region" of Figure 2 (above the abscissa, and for $n > \frac{n}{2}$), represented in Figure 3. (Riker 1962b, p. 41) This is the space of the characteristic functions of the winning coalitions. In particular, he identified three different types of winning coalitions:

1. Those for which the value of CF, in the "winning region," is a function with a negative slope
2. Those with a positive slope in part
3. Those with zero slope

Intuitively, the first states that the value of the coalition decreases with the growth of its size. The second case relates to a coalition that gains by adding new members, at least until a certain point. Finally, there are coalitions whose values are indifferent to their size. So then, the puzzle is the following: if some coalition once reached a majority, reputes its size disadvantageously, and prefers to leave out some members, then this is not an equilibrium.

27 Intuitively, no coalition is winning unless it has over $\frac{1}{2}$ the total membership or votes in the decision-making system.

28 The author discusses a further restriction to take into account the realistic case where different members of the coalition have different importance and therefore different weights. This aspect is important because the majority needed to form a winning coalition can differ in these cases. If the weight of one player is greater than the sum of all the weights of other players, a majority is formed only by that player. Therefore, Riker reworked the definition of m, simply putting it between 1/2 and 1 and imposing restrictions on the weights each player can have. However, when analyzing the specificities of coalition formations and their equilibria, the assumption of equal weight among the members of each coalition will be maintained.

In general, Riker showed that:

1. For functions with a negative slope throughout the whole range for winning coalitions, only minimum size coalition is realizable;

2. For functions with a positive slope in part of the range of winning coalitions, i.e., with a peak, the points on the negative side represent disequilibrium values;

3. Finally, for functions with zero slopes, the uniquely realizable winning coalition size is m.

However, the argument was by no means an axiomatic and exact game-theoretical result. Indeed the results above are obtained by employing a difficult and sometimes unclear verbal discussion about the behavior of each coalition, the range of values, and the decision to add or not a new member to their size.[29]

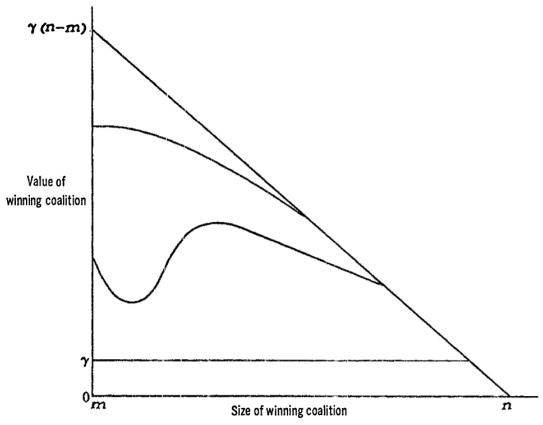

Figure 3: Different types of Characteristic Function

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

Among the abstract assumptions of the models, perhaps that of perfect information was immediately perceived as the most problematic.[30]A more realistic

29 A situation which Riker labeled as a "subgame," even if they are not adequately defined. Indeed, when referring to "subgame," in Game Theory, it is intended as a precise notion, namely the piece of a game that remains to be played beginning at any point at which the complete history of the game thus far is common knowledge among the players. (Gibbons 1992)As I will show in the next chapter, the main limitation of Riker's analysis is that, albeit using von Neumann & Morgenstern's theory of coalitions, he was still unable to tie his analysis explicitly to their formal solution. Therefore, his attempted proof is not really general. (K. A. Shepsle 1974). Riker attempted to provide a new and simplest proof of the size principle in 1966 (Riker 1966)

30 About the other two: rationality, as defined above, is clearly a more realistic assumption, although, as I tried to show, Riker's definition is not consistent with what he said about modeling, with the development of Rational Choice Models in Economics and the subsequent developments of

assumption could entail the idea that minimum coalitions can be subjectively estimated. Therefore the principle becomes that only coalitions estimated to be minimum winning occur. However, it is apparent that this further restriction is tautological and, therefore, cannot be accepted precisely like rationality defined in terms of preferences. This point makes verification quite impossible, at least in a positivistic sense. Indeed, if an MWC is not formed, this can be attributed to misplaced knowledge or uncertainty. Conversely, even if MWC are present, these can originate from "irrational motives" and, consequently, not evidence of the principle. Laboratory experiments could provide a getaway from this alleged paradoxical situation, simulating games to collect evidence about the SP and the coalition formation in general.[31] Then, Riker spent some time testing the results obtained by using GT. (Riker 1962b, pp. 49–53) However, his results were partial, lacking a specific methodology and adequate rules to set forth such experiments. Therefore, he concluded that: "if the behavior in small groups is not likely to produce much information about the behavior under the zero-sum condition, evidence about the usefulness of a model containing it must be found in the behavior of persons in large groups. Here, it is probably impossible to obtain experimental evidence, and one must rely on observation." (Riker 1962b, p. 54) In these situations, although it is often difficult to distinguish between those outcomes which are the product of rational actions and those which are not the "Size Principle," can be verified by using the following argument:

> "Presumably [...] the leaders are subjectively convinced that they have more in the coalition that they need to win. Their conviction is, of course, a certainty if their winning coalition is a coalition of the whole or a grand coalition. When this occurs, one would expect, if the size principle is a valid description of behavior, that they would make strenuous efforts to reduce their oversized coalition in the direction of a minimum winning one. To the extent that they do so behave, the size principle is verified, and confidence is increased in the validity of the model. Conversely, to the extent that they do not so behave, the principle is proved false, and confidence in the model is shown to be unjustified." (Ibidem)

To overcome the difficulties of empirical testing the SP, Riker collected evidence through historical reconstruction of some American and European history episodes. In particular, he focused on those situations when coalitions of the whole (whose value is zero) reduced their size. One instance is the disappearance of the Federalist Party, and the emergence of the Jacksonian Party (or Democratic Party), as a rational response to the coalition of the whole represented by the Democratic-Republican Party (1816-1828). (Riker, 1962, pp. 54 et ss.)[32]The Federalist party almost disappeared from American politics after the War of 1812,

similar models in Political Science. The assumption of side payments (or "transferable utility") is essential to von Neumann and Morgenstern's solution. However, it is also justifiable as a sort of compensation principle, i.e., if you join me, I can share something with you

31 Only in recent times the experimental testability of some assumptions regarding the rational behavior of economic agents, or players in GT, have become an important subfield of Economics. Especially after the awarding of the 2003 Nobel Prize in Economics to Vernon Smith, who pioneered this methodology. (V. L. Smith 1992) However, yet in the 1950s, some attempts to develop such tests were carried forward. See also the remarks made in the previous chapter.

32 The other two instances derived from American history are the emergence of the Republican Party as a consequence of the annihilation 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 various coalitions, at state and local levels. (cit., pp. 59-65)

leaving the so-called Democratic-Republican Party as almost a grand coalition. The coalition dissolved into a "disorganized melangè of blocking coalitions," which he modeled by assuming that a whole coalition does gain nothing. As he reconstructed this episode: "when Jackson took office in 1829, he had command of a relatively valueless coalition to which nearly everybody claimed to belong," he attempted to reshape his coalition into a minimal winning one by expelling some of his members, adopting a reckless attitude toward the member of his party that did not agree with him. "Viewed in this light, some of the events of Jackson's administration that heretofore have been viewed as trivial or embarrassing are seen actually to be the crucial events of his revival of the two-party system." (Riker 1962b, p. 58)

This argument, as well as the justification Riker provided, entailed an "instrumentalist" position, as the following passage seems to suggest further:

> "I do not suggest, of course, that these nineteenth-century statesmen appreciated this principle as a law of rational behavior. What I do insist, however, is that it describes their behavior, even though they probably perceived their problems thus: 'With our overwhelming majority, there are so many and so conflicting interests in the party that none can be satisfied. As long as two conflicting interests remain in the party, neither can be satisfied [which, I add, is why a grand coalition is valueless]. For the sake of action for the interest we approve, we shall therefore decide to satisfy one interest, and if others are offended, they may leave the coalition."[...] (Riker 1962b, pp. 65–6)

In the fourth chapter, Riker returned to the restrictions of the model, focusing mainly on the role of the information. The main point now is that coalition-makers aim to form coalitions larger than the minimum winning size due to incomplete and imperfect information. To further explore this point, the author developed a simple numerical example: a ZSG with 101 players aspiring to form a winning coalition at minimum size, namely 51 players. The total number of players is divided as follows:

1. Two sets, S and T, of 48 players each
2. Five sets A, B, C, D, E of 1 player each

To reach a majority, each of the two sets S and T have to convince at least 3 of the singleton sets to join them.[33] Other assumptions are that each member of S and T is loyal (then they cannot desert the coalition), and the information about the moves of the five singleton sets is imperfect.[34]. Such a situation involves bargaining because to convince players to join the coalitions, they must be offered something.[35] This bargain can be modeled by assuming that each 48 players coalition has some "operating funds," whose amount is $F_S > F_T$ (that is, coalition S has more funds than T). The point is to describe the prospective behavior of coalitions S and T. Each of these two coalitions has two strategies:

1. To make an offer only to three players

33 Then, the solution must be found within the set of all 5-tuples which pay a positive amount to at least three players.

34 It is not known the move of each set

35 A further simplifying assumption is that the uncommitted players receive their promised sum only if the coalition they join becomes a winning coalition effectively

2. To make an offer to four or five players

Given the assumption of imperfect information, the first strategy is not preferable to the second. Indeed, neither coalition can assure that the others will accept an offer unconditionally. Besides, given the difference between operating funds, S must decide how to allocate the addition in the amount it has to increase its expectation of winning. "Hence the problem is, for the coalition which has or believes it has the larger fund, how to exploit its advantage best." (Riker 1962b, p. 83)[36]

From this example, Riker deduced that the effect of imperfect information drove "not-yet-winning coalitions to seek more additions than they need to win" (Riker 1962b, p. 88). Therefore, a hypothesis could be advanced:

> *The greater the degree of imperfection or incompleteness of information, the larger will be the coalitions that coalition-makers seek to form, and the more frequently will winning coalitions actually formed be greater than the minimum size. Conversely, the nearer information approaches perfection and completeness, the smaller will be the coalitions that coalition-makers aim at and the more frequently will winning coalitions actually formed be close to minimum size."* (Riker 1962b, pp. 88–9. Italics in the text)

This "information effect" could explain some "critical elections" in U.S. Electoral history. The American political scientist V.O. Key developed the notion of "critical election" to define situations where the voters' involvement is high, and new electoral groupings are established. (pp. 90 et ss.) However, according to Rikerthis notion can be interpreted as a period where the amount of information in the system declines and grows the uncertainty regarding the winning coalition's size.

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. Nevertheless, this would contradict the assumptions about voters' utility maximization. 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 1962b, p. 98) This paradox for sure represented a blow to any attempt to use rationality to define voters' and parties' behavior in the same model. Instead, Riker sensed that his model offered a getaway to this situation. Specifically, suppose parties seek to maximize the share of votes only up to the size needed to become a minimum winning coalition. In that case, it is no longer convenient for them to becloud their positions in any situation, but only about those issues that concern voters about whom they have imperfect information.

36 Then, for coalition S, this means to prefer the second strategy above. However, this analysis is again outlined in verbal terms, and the model is not mathematically defined. Therefore, Riker's argument is consistent with the assumption of "imperfect information," as stated above. Still, it is unclear why this effect is ruled out by simply making an offer to four players and not, for instance, making a better offer only to three players. At the same time, even in the case of the second strategy, neither coalition can assure that the others will accept an offer unconditionally.

5.3.2 *Strategy and coalition building*

UNTIL NOW RIKER'S ANALYSIS has been eminently static. Thus, the size principle represented an ideal standard to which any rational attempt to build a coalition should conform. In this sense, although resting on von Neumann & Morgenstern's solution for n-persons games, it was not a solution for such games but a "sociological principle." Riker exploited some features of the structure of these games to understand the rational considerations behind coalition politics, like a parliamentary assembly, an alliance in a war, or, more in general, alliances in International politics.

In the second part of TPC, Riker investigated how political leaders set forth coalition-building to reach a stable institutional arrangement, if any. The aim was to fully exploit what he later recalled as the main feature of GT, namely the choice of strategies. (Riker 1992) However, this part is also the weakest of the entire work. Here Riker's analytical limits become apparent, and the intuitions laid behind the discussion about the Size Principle, as well as the information effect, are no more sufficient to sustain his argument entirely. Indeed, his analysis entails considerations about dynamic games and their sequential procedures. These were highly advanced topics, explored comprehensively only after the 1970s in the works of scholars like David Kreps, Robert Wilson, Ariel Rubinstein, and Ken Binmore, among others. Furthermore, these works and concepts determined the definitive boom of Non-cooperative GT in Economics.

The theory of games entails strategic behavior, but in von Neumann & Morgenstern's treatment of n-person games, this feature is shadowed by the general idea of the solution. That is, the value of each CF is the minimax solution determined by a 2PZSG between each coalition and its opposite. The strategic considerations behind each player's decision to join a coalition instead of another are embedded in the notion of imputation. So then, their analysis is eminently static.[37] To overcome this problematic issue, von Neumann & Morgenstern introduced the idea that the set of imputations "correspond to the 'standard of behavior' connected with a social organization" (Neumann and Morgenstern 1944, p. 41). A solution is, therefore, simply an accepted standard of behavior.[38]

Another idea, initially developed only for 2-person Cooperative Games, was proposed by John Nash and involved the non-cooperative foundations of cooperative games. (Nash 2002c) Nash explored how the purely algorithmic solution

37 As the two authors stated explicitly: "We repeat most emphatically that our theory is thoroughly static. A dynamic theory would unquestionably be complete and therefore preferable. But there is ample evidence from other branches of science that it is futile to try to build one as long as the static side is not thoroughly understood." (Neumann and Morgenstern 1944, p. 44)

38 Furthermore, the two authors returned briefly on this issue at the very end of their chapter on the general theory of zero-sum n-person games. There, they stated that the "negotiations, expectations and fears which precede the formation of a coalition and which determine its conditions" can be regarded as a type of quasi-dynamic theory, which also entails the fact that "conducts approved by an established standard of behavior does not conflict with each other, but can be used to discredit the non-approved varieties." This idea was consistent with how von Neumann and Morgenstern conceived game theory about pure theory and the general worldview lying behind it. Regarding von Neumann, the latter point has been further explored in Robert Leonard's work. R. Leonard 2010. Instead, it must be specified that Morgenstern's criticisms of the state of Economics in the 1930s focused on the latter's incapability to address the issue of dynamics properly and, therefore, adopt a too simplified view regarding equilibrium and perfect knowledge. However, in TGEB, Morgenstern accepted the purely static nature of such a theory because it represented, in his words, the first step for further elaborations

of a game, namely a negotiation between two players, could be analyzed as the outcome of a non-cooperative game regarding the determination of what is to be bargained. This situation entailed a dynamic game, namely a multi-stage game. As Luce and Raiffa, that represented Riker's primary source about game theory, summed up Nash's idea: "Roughly, his idea boils down to this: Each player adopts a mixed strategy as a "threat"; the pair of "threats" establishes a payoff, which, in turn, acts as the status quo point for future bargaining; and the bargaining problem is resolved in the manner discussed in section 6.5. [That is, in Nash's 1950 paper.] Therefore, the problem is reduced to selecting the threat strategies so as to influence the status quo - which controls the ultimate payoff - in the most favorable manner."(Luce and Raiffa 1957, p. 140)

Upon Nash's idea, after Ariel Rubinstein's work (Rubinstein 1982), it became customary to explore bargaining in a Non-cooperative and sequential setting, using the ideas of NE in extensive games, namely the perfect subgame NE. [39]

This idea of bargaining fits into Riker's analysis framework. Indeed, in a nutshell, Riker discussed how coalitions form, maintain their structure, or, conversely, add new members across a multi-stage game, whose last stage represents the outcome of the process. For example, consider voting, where a weighted majority is required and different coalitions, or use 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. Finally, if the coalition structure is such that a minimum winning coalition occurs in the final stage, this represents an equilibrium outcome.

When Riker worked on these themes, this kind of GT was still underdeveloped, as the first papers which developed it, like John Harsanyi's or Selten's, date to the mid-1960s.[40] Riker likely knew Nash's Bargaining result, even if he never referred to it in TPC since Luce and Raiffa, as seen, devoted a chapter to bargain and explored Nash's solution. (Luce and Raiffa 1957, pp. 124–34, 140–4) However, they were pretty dismissive of Nash's result, sensing that it would have hardly any relevance to players because it was an entirely artificial mathematical device. It is possible that Luce and Raiffa's statement and the too abstract nature of Nash's work were behind Riker's lack of interest in it. In any case, in dealing with the strategy in coalition-building, he seemed aware of the existence of these

39 This was a stronger idea of NE, developed by German economist Reinhardt Selten, applying to games in extensive form, i.e., multi-stage games. (Selten 1965; Selten 1975) This idea, together with Bayesian Nash Equilibrium, represented the most decisive development of Non-Cooperative GT and paved the way for the game-theoretical revolution in the 1980s. In a nutshell, a Non-cooperative game in extensive form, which can be represented as a tree, can be divided into different sub-games regarding the kind of information disposable to each player. Namely, if the information is perfect, then each node of a game tree, let apart from the terminal nodes, can represent the initial node of a sub-game. A perfect sub-game Nash equilibrium is the strategy profile which is a NE in every sub-game. (Gibbons 1992)

40 As seen, Game Theory was mainly developed at RAND and Princeton Department of Mathematics. Harsanyi escaped from Communist Hungary, obtained a Ph.D. in Economics at Stanford, and in 1964 joined the faculty of UCLA Berkeley Business School, where he spent the vast part of his academic career. Selten completed his studies in Germany but had many visiting positions at Berkeley, working with Harsanyi, from 1967 onward. Furthermore, he spent research time at Princeton in 1961, where he met Morgenstern and especially Robert Aumann and Michael Maschler. Harsanyi's and Selten's pivotal works on GT in extensive form date back to the second half of the 1960s. See also: Giocoli 2009b; for biographical information about Harsanyi and Selten: https://www.nobelprize.org/prizes/economic-sciences/1994/summary/

problems, even if the way he addressed them did not generate a meaningful theory.

Riker's model can be outlined 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 in such a way that losers would prefer to resign from the body rather than acquiesce. In this model, coalition building begins when a leader who 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 action changes the body's internal structure. Thus, in the first stage of such a game, there are n-single member coalitions. In the second stage, there are $n-1$-single-member proto-coalitions, one 2-members proto-coalition, and so on, up to the last stage, where either a winning coalition or different blocking coalitions exist.

Since any attempts to build a coalition generate opposition, the effect of the leader's first step toward building a proto-coalition is that others follow him and try to build their coalitions. The growth of proto-coalitions depends on the leaders' ability to attract followers by offering side payments. (Riker 1962b, 122 et ss.)

These side payments can vary, but Riker listed some examples. Of course, there are payments in promises on particular policies, or subsequent decisions, up to the threat of reprisal. Besides, these side payments also have costs, which the coalition leader himself pays, which must be considered. (Riker 1962b, pp. 109–20) Most importantly, Riker assumed that side payments were scarce and finite, subject to considerations regarding their economic value.

As seen, 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. Therefore, Riker introduced a notion "in some respect stronger, and in some weaker" than von Neumann and Morgenstern's set-valued solution.[41] He introduced the notion of a "uniquely preferable winning coalition," which involves the specification of a determined winning coalition. Namely, a coalition with 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 an amount equal to the best it can do in joining alternative non-minimal winning coalitions.[42]

41 Indeed, the latter did not specify if some coalition in the V-set was winning. See above

42 Riker also listed other four types of proto-coalitions: a "uniquely favored proto-coalition," which is proto-coalition s.t. any winning coalition containing it is more valuable than those not containing it, and if more coalitions contain it, at least one of them is a winning coalition; a "uniquely essential proto-coalition" appears in all winning coalitions. A "unique coalition" is a winning coalition, s.t. only one combination of proto-coalitions in the stage before that actually played can produce a

Any proto-coalition has some advantages (and disadvantages) in different game stages. Therefore, an equilibrium solution is that when a "uniquely preferred winning coalition" occurs, none of the other proto-coalitions can join it or form a new winning coalition. Since this entails the Size Principle, then an equilibrium corresponds to the presence of an MWC. However, the most critical 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 1962b, 147 et ss.)

The effects of this lacking of equilibrium for political analysis could be severe. As Riker wrote: "equilibrium in society is a kind of stability despite the change. And to say that this model lacks equilibrium is to say that the social processes it purports to describe are so unstable- that the political society itself is in fact unstable." (Riker 1962b, pp. 147–8) Therefore, the last three chapters of TPC contained a verbal discussion about the components of this disequilibrium and its consequences.

However, Riker intertwined these considerations with another attempt to test his theory against historical facts. The event he tried to explain now was a famous and controversial page of presidential history, the presidential elections of 1825, "the so-called corrupt bargain of 1825," when Andrew Jackson won a majority in the electoral college but lost the vote at the House in favor of John Quincy Adams.

Suppose this event is analyzed in terms of the dynamics of coalition building. In that case, one can assume that the four presidential candidates were leaders of four different "proto-coalition": Jackson, Adams, William H. Crawford, and Henry Clay (who entered the election, but since there was a maximum of three candidates for a vote in the House, he was forced to transfer his votes.)

The values of the proto-coalitions were the following: $w(P) = 11$ (Jackson), $w(Q) = 7$ (Adams), $w(R) = 3$ (Crawford) and finally $w(S) = 3$ (Clay)[43]. Since Clay, Adams, and Crawford were hostile to Jackson, the latter's proto-coalition was "strategically weak," whereas they Q, R, S formed a "uniquely preferred winning coalition." Jackson's coalition shrinks, whereas Adams' now became the preferred one. The new values were: Adams with 9 states, Jackson with 7, Crawford and Clay with 4 each. To obtain a majority, Adams could ally with Crawford or Clay, but not with both, to maintain an MWC. Eventually, an alliance between Clay and Adams occurred and was the dominant strategy from the latter's point of view. Indeed, if Clay allied with Jackson (despite their ideological differences), Crawford would have allied with Adams, forming an MWC. If Clay joined Crawford, now they had the same values as Jackson. Together, Jackson and Clay-Crawford would have won the election, but the total value would have been split three ways over two. Given these strategic possibilities, Clay joined

winning coalition. Finally, a "strategically weak proto-coalition" is one that cannot become part of a winning coalition. (Riker, cit. p. 127 et ss.) For instance, assuming three proto-coalitions, at a $(r-1)$th stage of a game, P, Q, and R, such that $P > Q > R$, $(Q \cup R)$ is the MWC, and $(P \cup R)$ and $(P \cup Q)$ are other winning coalitions. Given the Size Principle, the values of all the possible winning coalitions are $v(Q \cup R) > v(P \cup R) > v(P \cup Q)$ 'Proto-coalition' R can be defined as a "uniquely favored proto-coalition" (at a given stage of the game) because R is part of both the most valued coalitions (the MWC and that immediately next to it). In other words, the winning coalitions containing R are worth more than those which does not.

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

Adams, a majority was reached, and Adams was elected as the sixth president of the United States. Clay became his Secretary of State.

Once more, the historical argument serves Riker to display the explaining power of his model and provide the basis for a new understanding of historical and contemporary political facts. Again, furthermore, Riker stressed the fact that his example did not show that Adams, Clay, or Jackson were employing game theory or rational choice, but that their behavior can be explained *as if* they did.[44]

To conclude, it is apparent, from the description I offered above, that Riker's analysis had two main features: it was a dynamic process, i.e., it happened across time, and it was a bargaining process, where the bargain occurred between a leader and his followers. However, he neither tentatively tried to develop it as a non-cooperative multi-stage game. What he necessitated to do so was simply far beyond his capabilities at his time. Too much was still to be created, and what was yet existent, like the extended form of games, or the notion of "information set," both yet contained in von Neumann and Morgenstern's TGEB, was not sufficient. However, Riker proved a considerable intuition of what was required to build upon von Neumann and Morgenstern's static approach, where all the strategic choices that preceded the formation of coalitions were subsumed in the general idea of a solution. As he pointed out:

> "Unfortunately, it has, I believe, been generally assumed by game theorists that the theory did not offer many bases for the discussion of strategy in n-person zero-sum games. And, indeed, the inferences on strategy to be drawn from von Neumann and Morgenstern's exhaustive analysis of the essential three-person game are relatively few and unimpressive: that the winning coalition is unpredictable on the theoretical grounds, that the equilibrium payoff is an equal division between partners in a winning coalition, and that departures from the equilibrium are invitations to disaster."(Riker 1962b, p. 133)

It represented a possible setback in the case this theory needed empirical testing and extending this approach to positively characterize strategic behavior. Straightforwardly, one could assume that the creation of the Non-cooperative game theory, where agents cannot communicate with each other and binding agreements are not possible, solved the problem in quite a natural way. Then, whereas strategic considerations lie at the core of Non-cooperative games, Cooperative games refer instead to such aspects as fairness, enforcement, or other normative issues. (Serrano 2005 2005) However, a most theoretical and successful path was instead followed by Nash and reprised from the 1980s onward. As said above, this was the Non-cooperative foundation of Cooperative games, which was later recalled as the "Nash Program." (Nash 2002c; Binmore and Dasgupta 1987) In a certain sense, even Riker tried to make a similar attempt, exploring the strategic considerations behind coalition-building. He foresaw the possible development of GT but was unable to pursue it.

44 As Riker wrote: "In the case of all three of these crucial actions [...] there are local institutional or personalistic reasons available to explain the adoption or a rational strategy. Yet, in each action, the rational strategy *was* adopted. And this fact leads me to believe that it was not so much custom or payer that determined conduct as it was the intuitive perception of the abstractly 'best' strategy as here calculated from the model. It is not, of course, that the participants made calculations such as these but rather that in the concrete problems they perceived the concrete advantages of minimal winning coalitions and acted accordingly." (Riker 1962b, p. 157. italics in the text)

Conclusion: the reviews of Riker's work

TO CONCLUDE, I want to explore the impact of Riker's work, focusing on the immediate influence among the community of scholars the work was aimed at. These were the political scientists, but since the arguments presented by Riker encompassed formal analysis, also other social scientists and perhaps even game theorists. However, the second and third groups did not pay attention to it. Instead, political scientists focused only on some features without discussing in depth the work's formal aspects and what the author said about methodology.

Riker's work was published in 1962. Contrary to the author's expectations, this received little attention, even if all the reviews praised Riker's effort, as well as the originality of his analysis. Besides, there were no real game theorists among the reviewers, and no one provided comments about the inaccuracies and excessive simplifications of his employment of GT.

Perhaps the most enthusiastic among the reviewers was Alfred J. Hotz, who published it in the *Midwest Journal of Political Science*. (Hotz 1963) To him: "[...] the Riker's effort presents a most stimulating and provocative- study, one worthy of the most serious consideration by all of those within the political science discipline who are eagerly working toward a theory of politics." (Hotz 1963, p. 297) However, he did not explore in detail Riker's analysis and limited his discussion to a simple summary of Riker's argument.

Donald Matthews, and Morton Kaplan, instead, were more critical. Matthews, one of the leading scholars of the U.S. Senate, wrote, in *The Journal of Politics*, Riker's work "[...]scarcely can be dismissed as self-evident or trivial [...] Even the most sympathetic reader, however, is likely to put down the book undecided as to to the utility of such highly abstract and formal models". (Matthews 1963, p. 579) In particular, he asked if political scientists have enough knowledge about politics to construct models. Clearly, Riker favored formal model building, and the reviewer did not reject this position entirely on epistemological grounds. However, he also noted that the current state of knowledge at the disposal of political scientists was perhaps too limited, and new empirical theories were needed before working out applicable formal models.

Morton Kaplan, as seen, briefly discussed some game-theoretical implications in international politics during the 1950s. (M. A. Kaplan 1957) He worked mainly with Non-cooperative bi-matricial games, devoid of any formal sophistication. Furthermore, he used GT in a "functionalist way," without explicitly assuming a position in favor of rational behavior and formal modeling. However, Kaplan's work was well regarded by Morgenstern, and the two kept in touch during the 1950s. Kaplan cannot be considered a game theorist, certainly no more than Riker. Instead, he was an expert in international relations. In his review on *The Annals of the American Academy of Political and Social Science*, he contended critically two aspects of Riker's work: its presumed generality and its resting on rational choice. Then, he suggested that other models of coalition-building were possible, and many different values can be accounted for political decision-making. (M. A. Kaplan 1963) Despite his previous use of GT, he maintained his commentary on very general lines.

Richard Fagen's review was most supportive, which appeared on the *American Political Science Review*. (Fagen 1963) Again, he was neither a game theory nor voting models specialist. However, he had written a review on the most recent ideas regarding the study of power, where he also discussed Shapley and

Shubik's 1954 paper. Then, he had some knowledge of game theoretical models. (Fagen 1961) Fagen pointed out that Riker's TPC belongs to the nascent literature in political science, which deals with mathematical reasoning for the study of politics. However, he rightly noted that "[...] this is in no sense a book about mathematics; rather, it is a book about politics written for political scientists." (Fagen 1963, p. 446) Fagen highlighted three different purposes of the book. Firstly the attempt to build a theory of coalitions; secondly, "[...] to nudge –or shove–his fellow professionals toward the theoretical sophistication which (he feels) characterizes the neighboring disciplines of economics and psychology." (ibidem) Finally, a "policy purpose," mainly in international politics. However, this last purpose can be problematic since Riker's argument can be better applied due to its constraints (the ZS condition) to legislature problems. To him:

> "[....] Riker has made an important beginning-one in which he has raised and created enough problems so that many should feel moved or nettled to correct, criticize, extend, or develop his ideas. One of the beauties of his particular effort is that in order to find intellectual stimulation in The Theory the reader need not be entirely conversant with the game theoretical notions which underpin the argument. There are non-mathematical discussions of rationality, leadership, the balance of power, and many other concepts which cut sharply across the substantive compartments of political science. [...] one mark of the success of this book will be the number of controversies and derivative efforts that it engenders." (Fagen 1963, p. 447)

Finally, the most comprehensive review appeared in *The Journal of Conflict Resolution*, by James A. Robinson, an international politics scholar (Robinson 1963). He defined Riker's work as "[a] original and unusually well-written work. (Robinson 1963, p. 763). He outlined a brief but detailed summary and advanced two main comments. One regarding the mathematical model, perhaps being aware that Riker's argument was often imprecise (though the author admitted that he lacked the competence to evaluate this aspect properly). The second was instead about empirical confirmation. More specifically, Robinson did not share Riker's skepticism toward experimental methods in social science and suggested, as a matter of example, some laboratory experiments designed to address international relations topics carried forward by political scientist Harold Guetzow and his research team. However, one must note that Riker's attitude toward these kinds of experiments was less hostile than the reviewer presumed, as the former had been working on some experimental testing of GT in the 1960s.

As Riker lamented in Shepsle's interview, not many people had a sense of his work's theoretical features. (Riker and K. Shepsle 1979, p. 24) Political scientists indeed focused only on some aspects of his model, especially the last three verbal chapters. Furthermore, as apparent by many reviews, Riker's idea came to be framed and discussed mainly in international politics. Riker's last chapter, where he tried to detect some considerations about the developing rivalry between U.S.A. and U.S.S.R., caught the readers' attention more than the formal characteristics of the model. Besides, Riker's adoption of von Neumann & Morgenstern's solution went mainly unnoticed by game theorists.

So then, where does rest the importance of Riker's work? On the "size principle," some debates mushroomed, pointing to a more definitely formal assessment of ideas yet embodied in Riker's TPC. Still, his ideas regarding coalition building were less successful than his "size principle." Still, the issue of how coalition politics is carried forward in a legislature unsurprisingly became a highly debated

topic in formal political science. The studies on this topic, from the 1980s onward, grew distant from Riker's original ideas and closer instead to bargaining models in Economics.

The importance of Riker's work on political coalitions rests upon its being the first attempt to elaborate a model of political behavior explicitly based on a game-theoretical approach, even if this was far from reaching the levels of contemporary formal analyses in economics. Although Riker's work did not offer anything new to the development of game theory, it represented, in the light of what formal political theory became from the 1960s onwards, the first step of one of the most successful translations of theoretical ideas from one domain to another. This highly formal literature fulfilled Riker's vision and was often strictly related to his scholarship at the University of Rochester. This will be the main topic of the next chapter.

6

THE DEVELOPMENT OF POSITIVE POLITICAL THEORY

THIS FINAL CHAPTER is divided into two parts. First, I will focus on Riker's activity in establishing the political science department at the University of Rochester, eventually transforming it into the "stronghold' of "Positive Political Theory" and Rational Choice Political Science. Next, I will show some developments in formal Political Science built upon Riker's work on coalitions.

The University of Rochester's role as an institution and an intellectual community is pivotal in the story of how formal political theory spread into American political science, eventually becoming a definite subfield of the discipline. As suggested by Sonja Amadae, one of the specific features of "Positive Political Theory," different from Public Choice, was that, whereas the latter was strongly institutionally based (at the University of Virginia, and later, at George Mason University), the former is also strongly associated with the scholarship of a single individual.(Amadae 2003) Thus, the development of social sciences at Rochester and the intellectual activity of Riker are deeply intertwined, and to better appraise the latter, Riker's role as an "entrepreneur of ideas" cannot be downplayed. In her historical reconstruction, Amadae also devoted some attention to the co-eval transformation of the Department of Economics, toward a strong theoretical commitment, under the chairmanship, both academic and charismatics, of the economist Lionel W. McKenzie. Therefore, she stated, Riker "had a strong vantage point from which he could build a curriculum that could exert a major influence on the entire discipline of American political science because he had strong institutional support, which, not coincidentally, was similar to the advanced McKenzie's vision on economics. The University of Rochester, whose trustees firmly committed to advancing scientific methods within the social sciences, provides a compelling example of how rational choice theory was furthered as a complex of knowledge spanning several independent but interrelated fields of study." (Amadae 2003, p. 170)

There are different ways to address this topic. One consists in adopting the widespread notion of "intellectual community," which entails the idea of the value of the scientific enterprise as a social activity. A long list of taxonomies involving the features of these communities of scholars is now present in the "History of Science/Sociology of Science" literature[1]. In a very general sense, this started from the notion of "Research School" developed originally by J.B.Morrell (Morrell 1972), which encompasses the pivotal role both of one (or more) charismatic leaders and adequate financial support. However, the debates go on, and the range comprised in the notion of "charismatic leadership" remains very broad, spanning from 'someone who can access resources, or perhaps attract the attention of important 'gatekeepers' in the profession' to "someone who can draw colleagues and students to his research agenda." (Forget and Goodwin 2011, p. 20) Sometimes, the same person fills both, which could be the case with Riker.

1 A discussion is in Forget and Goodwin 2011. See also Steven G. Medema 2011

However, I will remain committed to a more historical discussion on these pages.[2]. Besides secondary sources, my narrative will rest on some material in McKenzie's papers, housed in Rubinstein Library at Duke University, and Riker's papers in Rochester.

In the 1960s and 1970s, Riker's activities at Rochester were paralleled by his broad attempt to establish a network of scholars who shared his same view. Indeed, the decade was also marked by the theoretical works of Buchanan & Tullock, the creation of "Public Choice," as well as by other formal developments, like, say, the spatial theory of voting, thanks to people like Otto Davis, Melvin Hinich, and also Rochester's Ph.D. like, for instance, Peter C. Ordeshook and Kenneth Shepsle. The final paragraph of this first part is devoted to reconstructing the spreading of Positive Political Theory and Rational Choice Political Science in the 1960s and 1970s.

The second part is devoted to presenting the content of some cutting-edge theoretical development of formal analysis in Political Science. It is beyond the scope of this research to review all the main contributions offered in this field, both from Riker and the scholars who joined him. So, I will focus on the comments and debates on the size principle and coalition theory. As seen, Riker provided valuable insights in his analysis of political coalitions. Still, his work was somewhat flawed both mathematically and game-theoretically. By presenting Robert Butterworth's critiques of the size principle, Shepsle's reply to Butterworth, and finally, McKelvey's allegedly trivial (and unpublished) mathematical proof of the principle, I will try to show the importance and the flaws of the size principle and coalitional games.

6.1 INSTITUTIONALIZING THE "POSITIVE POLITICAL THEORY": RIKER AND THE UNIVERSITY OF ROCHESTER

THE UNIVERSITY of Rochester was established in 1850 in Rochester (NY). During the Second Industrial Revolution, this city became an important industrial place, where important companies were founded, most notably Kodak and Xerox. Furthermore, the city also had a tradition of intellectual activities. Here, for instance, lived the great activist and abolitionist Frederick Douglass, and therefore, the city was one of the forums of the antislavery debates before the Civil War. After that, Rochester was also the hometown of the social reformer and women's suffrage activist Susan B. Anthony, which provided further intellectual stimuli. Despite this, in the mid-1950s, the University of Rochester was a small private liberal art college in a geographical position certainly not favorable. Indeed, located in the north-western part of New York State, on the shores of Ontario Lake, and close to Niagara Falls and the Canadian border, it was far from the intellectually stimulating environment of the Ivy League colleges, the University of Chicago or California. Its academic accomplishments were mainly related to the presence of the country-wide renowned Eastman School of Music, which started in 1921 due to the endowment of George Eastman, the founder of Kodak.

2 A historical discussion of the establishment of the Political Science Department, although very general, is contained in Amadae and Bueno de Mesquita, 1999 (partially reproduced in Amadae 2003). For the case of Economics, this is provided, again very generally, by Düppe and E Roy Weintraub 2014a; McKenzie 2012)

Another philanthropist who grandly contributed to the academic rise of the University was Joseph Wilson, the owner of Xerox. In the 1950s, Rochester obtained a huge endowment thanks to him, which listed it among the richest Research Universities in the US. (Amadae and Mesquita 1999) With that money, the University administrators started their program to enhance the academic status of their institution, beginning with recruiting senior and younger professors and establishing new advanced studies programs.

This section aims first to reconstruct the development of social sciences at the University of Rochester between the late 1950s and the early 1960s. One of the consequences of the ambitious and well-financed development plans pursued by the University administrators was the substantial strengthening of social sciences studies. The outcome was the establishment of nation-leading and theory-driven advanced programs in Economics and Political Science. Especially in the case of Political Science, Rochester became the institutional stronghold of "Positive Political Theory."

Among the first scholars hired and tasked to implement research activities was the relatively young but yet renowned mathematical economist Lionel W. McKenzie. Despite some significant results, McKenzie's impact on his discipline was perhaps less revolutionary than Riker's on Political Science. However, the presence of an advanced and theory-driven department of Economics for sure had a substantial impact on Riker's ability to pursue his theoretical and disciplinary agenda.

The next pages will explore McKenzie's transformation of the Economics Department, Riker's activity as chairman of the Department of Political Science, and finally will provide some highlights on Riker's activities toward the building of a network of scholars, with a particular focus on two aspects: the relationship with the Public Choice group; and the spreading of mathematical political theory.

6.1.1 *The activities in the Economics Department*

LIONEL W. MCKENZIE (1919-2010) was surely among the most important mathematical economists of his time. In the 1950s, his accomplishments were extraordinary, also given his peripheral displacement in the community of mathematical economics. A Georgia native and a Duke graduate (in a period when Duke was still only a relatively prestigious regional university in a segregated state), he later moved to Princeton, where he obtained his Ph.D. (although, for personal reasons, he was able to finally receive it only in 1956, after the publication of his first important theoretical papers). Before, he also was a Rhodes Scholar at Oxford and an instructor at MIT. (McKenzie 1999) McKenzie started to work on mathematical economics mainly by self-training, and in 1950, he spent an entire year at Cowles Commission at Chicago University, where he met the main characters of the incipient formal revolution in economics, like Gerard Debreu, Tjalling Koopmans, and others. (E. Roy Weintraub 2002)[3]Furthermore, at Chicago, he received formal education in Mathematics, especially Algebra, Topology, and Measure Theory, thus expanding his knowledge well beyond the calculus-based training that still represented, if any, the only requirement for Economics students and graduates. (McKenzie 1999, p. 4)

3 See also the first chapter of this thesis

Later, he returned to Duke, where he was appointed professor of Economics. His theoretical activity in that period was mainly devoted to the issue of General Economic Equilibrium. In particular, he provided the first mathematically rigorous (and economically interesting) proof of the existence of the General Economic Equilibrium, predating by some months the most famous Arrow-Debreu's theorem. (McKenzie 1954; Kenneth J. Arrow and Debreu 1954; Düppe and E Roy Weintraub 2014a)[4]

This shows that McKenzie occupied a somewhat central role in the rising community of Mathematical Economists. Many colleagues recognized the brilliance of his mathematical contributions. Furthermore, he had strong personal relationships with some renowned economists, like Paul Samuelson. By comparison, whereas Riker was an isolated scholar, both in the community of political scientists (to which he belonged) and in that of game theorists (to which he tried to belong), McKenzie's scientific stature was well esteemed among his peers.[5]

But as it is evident both from his letters and his recollections, in reality, McKenzie was partially dissatisfied with his scholarly activity, and this dissatisfaction was the main force that pushed him toward accepting the offer that Rochester Administrators made in 1956. The sources of such dissatisfaction were twofold, starting with personal circumstances, mainly due to being, at Duke, quite far from the prestigious and engaging departments where the rapid advancements in Mathematical Economics were carried on. McKenzie apparently had his theoretical agenda, which was barely accepted at Duke.[6] Indeed Duke faculty was not particularly inclined toward new developments of Mathematical Economics and, like the vast part of economics departments at the time, remained committed to more traditional analysis, based on verbal logic and descriptive generalizations rather than Econometrics and Mathematics. McKenzie instead favored the rigorous foundation of Economic Theory in a mathematical fashion, to which he contributed grandly. At the same time, he, differently from what could be defined as the "main strand" of axiomatic theory in Economics (that mainly pursued by Debreu at Berkeley, to which pertain some of the most important but also theoretically abstract General Equilibrium results in the 1960s and 1970s) devoted attention ever on the economic relevance of his analysis. Consequently, he was not simply a mathematical economist, or, even less, a mathematician doing Economics research, but in his own words, was an "economic theorist." Mathematics and rigorous analysis were decisive in establishing a logically consistent economic theory, but at the same time, this was not reducible exclusively to a sequence of mathematical theorems and results. (Düppe and E Roy Weintraub 2014a, p. 190).

The call from Rochester came in 1956 amid growing dissatisfaction with the Duke environment. As said before, the University of Rochester was looking for

4 Other than General Economic Equilibrium, McKenzie's contributions to Economic Theory encompassed the foundations of Demand Theory (McKenzie 1957) and the issue of Optimal Economic Growth (the so-called Turnpike Theorems). A comprehensive outline of McKenzie's theoretical contributions is contained in Mitra and Nishimura 2009, and McKenzie 2009.

5 For instance, Samuelson wrote in a personal letter that he was among the few mathematical economists to have a deep understanding of the economics of mathematical economics. (cit. in Düppe and E Roy Weintraub 2014a, p. 195).

6 To these must also be added some more personal issues, like having married a Jewish woman in a state, North Carolina, which, although relatively moderated by comparison with the 'Deep South' (where McKenzie was born), was anyway not alien from a certain degree of antisemitism (other than, obviously, racial segregation for African-Americans).

promising senior scholars with original ideas to establish an advanced graduate program in social sciences, and McKenzie surely matched this request. By looking into McKenzie's correspondence, insights into how the recruitment process occurred can be found. He was offered not only a full professorship but also the chairmanship of the Department of Economics, recently established as a partition from the Business school. This, as well as an exceedingly high salary for his position, captured him, although he remained uncertain for a few months, mainly due to Rochester's lacking of a prestigious academic pedigree, compared to other major universities. Looking for advice, he asked different people, like Samuelson, who counseled him to keep the negotiations with Rochester Staff alive (Paul Samuelson to Lionel W. McKenzie, September 5th, 1956, McKenzie n.d., Box 6). In a letter to his friend and colleague at Duke, Frank de Vyver, McKenzie described the University of Rochester as follows:

> "The place looks pretty good. They already have a large graduate school for the science, and the Eastman school of music and a great medical center. Also, they have one of the largest endowments of the country, and only about 1,600 undergraduates, which they expect to build up to 2,500. They really do de-emphasize athletics, and they play places like Oberlin. They have a scholarly president with a Ph.D. in history from London, who appears to be an effective leader as well. The departments of economics and business administration are merged at the moment, but they are being separated this year. I would probably go there as chairman of the Department, though I am not required to do this. The teaching load is 9 hours for about everybody with less if you are deeply engaged in research. They told me I could write my own ticket on that. In fact they painted a quite rosy picture. I would have at least two appointments available immediately." (Lionel W. McKenzie to Frank de Vyver, October 16th, 1956, LWMP, Box 6)[7]

In the same letter, he enucleated some pros and cons of his eventual move. Among the former, a considerable increase in his wage, by far superior to any sum he could expect at Duke. The negative comprised the northern climate and the administrative responsibility, other than the difficulties of starting a viable graduate program from scratch in a place without tradition or experience. In the following letter, one month later, and after having accepted Rochester's offer, he wrote:

> "[...] The opportunity to play a central role in developing a department is attractive to me. [...] The city seems extremely nice. The university is well equipped and owns a very large endowment, alleged to be the 7th in the country. Their tuition is 850 dollars. They have about as many graduate students as Duke and only 1,500 undergraduate. There will be a couple of places to fill in the Department right away and several people are retiring soon. They have two recently acquired Princetonians, Warren Hunsberger and France (in labor). France seems very promising. He has done some writing with the industrial section in Princeton. Anyway it will be an adventure." (Lionel W. McKenzie to Frank de Vyver, November 23, 1956, LWMP, Box 6)

As stated above, McKenzie was hired to develop a first-level graduate program in Economics. A first move was the split of the Department of economics from

7 McKenzie is referring to the historian Cornelis de Kiewet (1902-1986), previously president of Cornell University

that of business, a decision that predated his arrival but became effective only after his appointment.[8]But the most important was the implementation of the proper hiring policy. At his arrival, the Department had three full professors, one associate professor, one assistant professor, and one lecturer, none of them really on the edge of scientific advancements in economic theory.[9] The fields of research followed the traditional curriculum in undergraduate economics education in the U.S., namely Money and Banking, Public Finance, International Economics, Area Economics, Labor Economics, Government, and finally, Economic History. (Albert W. Noyes to Lionel W. McKenzie, November 20th, 1956, LWMP, Box 6) After accepting the job offer, McKenzie started immediately looking for young scholars to be hired. Even before he accepted the job, he discussed his standards in a letter to Noyes, the Dean of the College of Arts and Sciences. McKenzie wrote that he "[...] would like to have two appointments available in the beginning, preferably one at the associate level and, if the right man is available, one at the full professor level. The full professor would be very valuable if a top-flight man will come. Otherwise, it might be better to make two appointments at the associate level, of promising people who have already demonstrated their ability through publication, or perhaps one at the associate and one at the assistant level, depending on who is available." (Lionel W. McKenzie to Albert W. Noyes, October 29th, 1956, McKenzie n.d., Box 6)

Between the end of 1956 and the beginning of 1957, he sent different letters to people with whom he was well acquainted, like Paul Samuelson, Robert Dorfman, Kenneth Arrow, William Baumol, Milton Friedman, Jacob Viner, Franco Modigliani, Albert Hirschmann, asking for the possibility of hiring graduate students in their Department, as well as for personal information about the present members of Rochester Department of Economics. For instance, he attempted to recruit Gary Becker from Princeton, but without success, as the latter chose, as noted, to remain at the University of Chicago. (Lionel W. McKenzie to William Baumol, November 28th, 1956; Lionel W. McKenzie to Milton Friedman, November 28th, 1956, LWMP, Box 6) The first appointment of the new faculty was Ronald Jones, who had obtained a Ph.D. at MIT with Robert Solow and would become one of the most renowned world experts in Trade Theory (Düppe and E Roy Weintraub 2014a). He was immediately followed by other scholars: Richard Rosett, Edward Zabel, Nanda Choudry, Michio Hatanaka, S.C. Tsiang, Rudolph Penner, Alexander Eckstein, Sherwin Rosen, and finally Robert Fogel. The latter, who later moved to Chicago, was awarded the Nobel Prize in Economics in 1994 for his contributions to Quantitative Economic History. After a year, the graduate program also started, with a strong commitment to the recruitment of students from foreign countries (especially Japan, Europe, and other regions of Asia). Among the first students enrolled in the program, the most notable was Akira Takayama, later a renowned mathematical economist.

Insights on the scientific activity of the Department in the early 1960s can be found in some reports written for the "Committee on Social Science" of the University of Rochester.[10]These reports also provide a general overview of the state

8 This matched McKenzie's ideas concerning education in Economics. Indeed, at Duke, he proposed exactly such a separation between business and economics classes. (Düppe and E Roy Weintraub 2014a)

9 These were respectively: William E. Dunkman, Donald W. Gilbert, Warren Hunsberger, Robert R. France, Jack Taylor, and Alice Wynd.

10 Henceforth CSS

of social sciences research at Rochester and the plans discussed to advance them further. To pursue the enhancement of social science education, the Committee was established in the fall of 1961, with members from all the social sciences departments in the College of Arts and Science, i.e., Anthropology, Political Science, Psychology, Sociology, History, and Economics. As it is apparent from the drafts of the first meetings, between October and November, the main issues to be addressed comprised, other than interdisciplinarity, also how to better fit the development of social sciences at Rochester to the 'national trends' country-wide. Moreover, the activities of each Department, as well as the aims and scope of their research, are described.

The reports of the CSS give a sketch of how the activities in the Department of Economics were organized under the chairmanship of McKenzie. Scientific research was divided into empirical and theoretical analyses. To the first group belonged, for instance, the work of Fogel, devoted to offering a new exploration of the development of American Economic History, focusing especially on the role of railroads. Methodologically, Fogel framed his analysis "within the categories of growth economics," applying advanced mathematical and statistical techniques to collect and analyze data. In the same report, emphasis was also placed on the expansion of the funds and the facilities needed for such empirical research, especially computers and libraries. ("Empirical Research currently under way in the Department of Economics," LWMP, Box 32) Interestingly, although Economic History was only one of the empirical researches, a particular mention was made to the "continued development of relations between the economics and history departments." ("Empirical Research currently under way in the Department of Economics" 1960) A similar plea for interdisciplinarity was also advanced for political science, with particular regard to taxation and fiscal policy. Research in theoretical Economics, McKenzie's field of interest, spanned from the mathematical analysis of competitive markets, with particular attention to the hot topics of GEE's properties (existence, uniqueness, and stability of equilibrium), to international trade and macro-economics. Eventually, among the aims to be pursued, we also find the development of mathematical analysis and statistical analysis in the social sciences, focusing on choice behavior, psychology, and social psychology. ("Research in Theoretical Economics" 1960, LWMP, Box 32)

Both reports are descriptive and very general but are precious in presenting a view of what kind of commitment toward social science and interdisciplinary analysis was pursued at Rochester in the early 1960s. Although they do not enter much detail, they represent the image of a very challenging and exciting scientific environment. An aspect confirmed by the personal recollections, interviews, and oral histories, of some of the people who spent part of their academic careers at Rochester. As Düppe and Weintraub stated: "By the early 1960s, McKenzie had built up the Rochester research community with its own identity, distinct from that of Berkeley, and proud to be labeled theoretical as opposed to mathematical. [...] Rochester welcomed mathematics and science 'majors' into its graduate program as over the 1960s it became known as an exciting place for them to study economics and political science." (Düppe and E Roy Weintraub 2014a, p. 193)

6.1.2 *Setting the "Positive Political Theory"*

In the late 1950s, the Department of Economics was in the middle of significant transformations, and that of Political Science was on the eve of them. One must note that the CSS mentioned above was formed some months before Riker officially assumed his position at Rochester University. Despite this, the discussions about Political Science were certainly influenced by his imminent arrival. In fact, the drafts of the meetings make it apparent that Riker was hired mainly for his ideas about the proper development of the discipline. Furthermore, although he was not yet officially part of the faculty, many, though very general, references were made to his planned activities in the Department.

So then, in the first draft, reporting the situation in the Political Science department, it is written: "Political Science, though not distinguished for its research, provides good teaching at the undergraduate level. A number of recent additions to the Department have increased the research potential. A new chairman, Professor Riker of Lawrence College, assumes his responsibilities in September 1962. There are plans to increase the budget and to build gradually to a ten-man department." (Social Sciences (UR) 1961, October 2nd, 1961)Then, even if the Department was represented in the Committee by Richard Fenno, a scholar committed to traditional analysis, although not hostile toward a more formal one, it seems that some of Riker's ideas were embodied in the discussions. Emphasis was put on the new perspectives Riker would have adopted, namely the "growth toward a doctoral program" starting with the "acquisition of men with broad theoretic interest and with skill in the application of scientific analysis to the subject matter of politics." (Appendix to the minutes of "Committee on the Social Sciences," October 2nd, 1961, LWMP, Box 32). Again, in the third meeting, when Fogel and McKenzie discussed empirical research and applied mathematics, Fenno remarked on Riker's active interest in mathematical methods in the study of political behavior. (Social Sciences (UR) n.d., November 6, 1961, McKenzie n.d., Box 32).

As stated above, each Department was also asked to write a report for the Committee. Unfortunately, the report on Political Science is unsigned. Therefore it is impossible to ascertain if it was Riker's product or instead Fenno's. However, taking a glance at it, Riker's theoretical agenda can be recognized once again. For what concerns teaching activity, the Department adopted the traditional, and somewhat standard curriculum, i.e., American Politics, Comparative Political Systems, International Politics, and Political Theory. But it is also anticipated that "beginning next year [...] we will be giving a different, more theoretically and broadly oriented introductory course". ("Report on Political Science for Committee on Social Science" 1960 Report on Political Science for Committee on Social Science, in McKenzie n.d., Box 32). Such a re-orientation of teaching activities was seen as a pre-requisite for developing a graduate program, but also for re-orienting the activities of the Department, which, in the last years of the 1950s, "was really going downhill in terms of its undergraduate activities," since, as Riker recalled to Kenneth Shepsle, in 1957 "no people decided to major in political science. So that there were no seniors in political science in 1959, which was the nadir of the department." (Riker and K. Shepsle 1979, p. 62)

Among the research projects in which the Department was to be involved, we found the analysis of decision-making in small groups, that of federalism, the analysis of congressional committees, and also more theoretically oriented topics

like "methods and hypotheses which may be used to develop a general theory of politics" or "the relationship between leisure behavior and political behavior." But the intrinsic interdisciplinarity of these projects was also recognized, for instance, between Decision-making Theory, Mathematics, and Economics. ("Report on Political Science for Committee on Social Science" 1960)

As to hiring policies, the aim was explicitly stated as that of seeking "men of broad theoretic interests and with skill in the application of the important analytical techniques of social science to the subject matter of politics." (ibidem) This necessarily entailed an interdisciplinary approach with a broader research horizon, made possible only through a strong collaboration with other disciplines. As the discussions before the CSS clarified, "[...] it is difficult to be precise about the nature of all our needs. Certainly, all existing strength in the social sciences plus Mathematics and Philosophy will be a source of strength to Political Science." However, the emphasis is not exclusively on formal analysis and mathematical methods. For instance, also social psychology was considered part of this renovation process.

The report concluded with a general statement on the future of the discipline: "the future of Political Science as a social science depends upon its ability to link systematics theory about human behavior and accurate descriptions of distinctly political activity. Moves which promote this 'marriage' - within or without the Department - will enable Political Science at Rochester to operate at the frontiers of the discipline. Our goal is to do this and to do so with distinction." (ibidem)

Riker arrived at Rochester in 1962. He was hired by Noyes in 1961, with conditions very similar to that of McKenzie, and started his activities in the fall of 1962. Like McKenzie before him, Riker was looking for new career opportunities and was partially dissatisfied with his Academic environment. Besides, McKenzie certainly played a role in convincing Riker to join Rochester. Indeed, as he wrote: "I recall my discussion with Bill Riker when he came to the University to consider whether to accept a position there [...] Of course I welcomed him heartily to Rochester and described my Department's goals to him. The presence of an economics department whose success was already well known played an important role in making these appointments possible." (McKenzie 2012, p. 232)

Riker assumed his official appointment as full professor of Political Science and chairman of the Department in the fall of 1962. As soon as he arrived, he started his work toward its radical renovation, following the lines discussed above. At Rochester, he found the right environment to advance his own agenda, supported even by those scholars, like Richard Fenno, who remained committed to traditional methods. Riker recalled that other than personal reasons, what captured him were the efforts made by the Rochester administration to pass "from a small liberal arts college to a university and systematically developing departments" and to build an advanced graduate education program. (Riker and K. Shepsle 1979, p. 62)

No one opposed his program to enhance the fortunes of the Department by advancing his view of Political Science, which obviously needed the teaching of Decision and Game Theory courses, as well as methodology ones. Kenneth Shepsle, who enrolled in 1967 in the newly appointed Graduate Program, stated that "when [Riker] came to Rochester, he came as the chair of the Department. Quite innocently, he sent around a memo to his colleagues proposing a Ph.D. program where he emphasized game theory and decision theory, and econometrics. He expected to have a broad discussion with these colleagues about it. His

colleagues immediately said yes. And so he began to build the faculty that could teach that kind of political science." (K. A. Shepsle 2021) What Riker was trying to accomplish can be summed up in the brief memo about Political Science Curriculum that he drafted as a Fellow of Stanford's "Center for Advanced Studies in Behavioral Sciences." (1961)[11]. The same vision, as seen, is contained in all his works in the late 1950s, up to *The Theory of Political Coalitions*.

To better understand Riker's view and how this affected the teaching of political science at UR, some insights on the course and the state of the Department are needed. Rather than the drafts of the CSS, a more comprehensive overview of the Department's activities can be found in the University of Rochester Undergraduate Bulletins. These bulletins present, other than a general description of UR history and activities, also a detailed list of Departments, Faculty members, and courses. For what concerns the academic year 1961-2, the Political Science curriculum mirrored a traditional approach, divided into "International Relations," "American Politics and Institutions," and "Political Theory." The latter also included the introductory course in "Scope and Methodology in Political Science."("University of Rochester Undergraduate Bulletin 1961-2" 1961, pp. 147–9). Riker arrived the following year, and among the first people he hired, we found Arthur Goldberg and especially Gerald H. Kramer as an instructor, who added to the original six members of the faculty (beside Riker himself).[12] Kramer, an MIT Ph.D. had a strong mathematical background, and his role would be essential in the following years to advance the training and the research in mathematical political science within the Department. Following Riker's arrival, the undergraduate courses in scope and methodology doubled, and for the academic year 1963-4, a course in "Positive Political Theory" was introduced in the "Political Theory" section. (1963-4 UR Bulletin) Amadae and Bueno de Mesquita described the transformations of Rochester Political Science as follows: "whereas other programs emphasized the literature, Riker focused on developing tools for rigorous research into the theoretical properties and empirical law of politics." (Amadae and Mesquita 1999, pp. 279–80). In this, the notions of Rational Choice in political decision-making certainly occupied a central place.

Still, like in the case of the Department of economics, the pre-requisite for establishing a successful graduate program were faculty and student recruitment. As Riker later recalled it:

> "I devoted the first year to finding teachers to expand [...] and also outlining and planning a Ph.D. program. [...] I was extremely fortunate to hire two people who were entirely sympathetic with the kinds of things I wanted to accomplish in that Department. One was Jerry Kramer and the other was Art Goldberg. [...] Kramer was the first person that I could find who was both a political scientist and was interested in teaching about statistics. [...] And it was pure luck to be able to find him. And I think that his experience here was very good for him because he came here thinking of his role in the world as being a statistician. And, in the five years or so he spent at Rochester, I think he changed his vision of himself to being a political scientist and being interested in political theory. And I always felt

11 In that memo, Riker explicitly stressed the attention on positive methods of verifying hypotheses and positive theories of politics, whereas 'positive' assumed the meaning of non-normative theories. (Amadae and Mesquita 1999) See also the discussion in the previous chapters

12 These were Richard Fenno, Theodor Bluhm, Glen Wiltsey, William Diez, Dale Neuman and Peter Regenstreif (Riker and K. Shepsle 1979, p. 63)

> that that was one good thing that this Department did for the world was to make Jerry into a political theorist." (Riker and K. Shepsle 1979, p. 65)

Kramer, according to Shepsle, was "probably the leading mathematical political scientist at the time" (K. A. Shepsle 2021) and had a pivotal role in supervising the dissertations of analytically inclined graduate students, like Peter Ordeshook, Shepsle himself and especially Richard McKelvey. After his Rochester experience, Kramer would join the faculty of Yale and spend much time at Cowles Foundation there.

Starting from 1967-8, Richard Niemi joined the faculty. He had a Ph.D. at Michigan State but was also previously an undergraduate at Lawrence, where in 1961, he had attended perhaps one of the first courses made by Riker about "Positive Political Theory," in the same period when Riker was also a Fellow at Stanford. (Niemi 2021) Despite lacking formal training in mathematical modeling, like Kramer or some of the graduate students who began to arrive once the Graduate Program started, Niemi had a robust training in Statistics and fit well in the theoretical agenda Riker was advancing. So then, in the academic year 1967-8, the undergraduate courses covering themes like Game Theory, Decision Theory, and analytical methodology became five. ("University of Rochester Undergraduate Bulletin 1967-8" 1967)

Nevertheless, the most important novelty of the second half of the 1960s was the definitive establishment of the Graduate Program, namely the task Riker was initially called for. The first student enrolled in 1962, but the program officially started in 1963. Ordeshook, who entered with the class of 1964, recalls that, although he chose Rochester over Columbia quite by chance, Riker's treatment of formal political theory deeply struck him. (Peter C. Ordeshook 2021) The program covered practically all the main topics up to that time, spanning from Arrow's Impossibility Theorem to Spatial Models of Voting, but also Buchanan and Tullock's *The Calculus of Consent* and the newly published Olson's *The Logic of Collective Action* (1965). And finally, Riker's work on political coalitions and game theory, using Luce and Raiffa's textbook (Luce and Raiffa 1957). At the same time, as Ordeshook wrote to the author, *"Riker was interested in such things only to the extent that they said something about real politics. Riker's philosophy, then, was quite simple: The emerging field of formal political theory should not spin off into arcane mathematics or a plethora of axioms but instead address classical political questions."* (Peter C. Ordeshook 2021. Italics in the text). Kenneth Shepsle, who arrived in Rochester in 1967, remembers that Riker was deeply involved in graduate education. He taught two seminars every graduate student took, one introductory about Game Theory and one called "Positive Political Theory." (K. A. Shepsle 2021). A more advanced course in Game Theory was likely taught by Gerald Kramer, whose mathematical capabilities were far beyond anyone at the time among Rochester Faculty. However, students, according to Ordeshook and Niemi, were also required to take also traditional courses, like that of Fenno and Bluhm. Anyway, the Rochester Graduate program was identified in the late 1960s for its strong emphasis on mathematical political science and formal modeling. The consequence was that the undergraduate background of the student who applied for it largely differed from that of other political science programs. In Shepsle's words: "When I came to Rochester, it was still a collection of oddballs in a way who ended up in graduate school at Rochester, people who had done work in physics, or in engineering, or in mathematics, who gravitated normally

as undergraduates into political science and then on to Rochester." (Riker and K. Shepsle 1979, p. 70). Shepsle had a Major in Mathematics, while, as seen, Ordeshook was an engineer by training. The class of 1968 featured Richard D. McKelvey, who had a similar background (T. Palfrey 2005), and the trend continued in the 1970s.

Mathematical reasoning occupied a central role in "Positive Political Theory." Indeed, as Riker intended it, this was not simply an economic approach, in the sense of the many parallels advanced, in the course of the history of ideas, between economic and political action.[13] At its very core instead lies the importance of the formal modeling of political phenomena, where concepts like preferences, choice, and equilibrium assumed meaning exactly in relation to the internal consistency of the model. Therefore, mathematical training occupies a pivotal role in the education of political scientists at Rochester. Note, however, that Riker was not really a mathematician. As said before, he was mainly self-trained, but his capabilities were far below those needed to pursue his ambitious theoretical agenda. As Professor Bruce Bueno de Mesquita remarked, "[Riker] had not many mathematical skills, but a great mathematical intuition. He had a good understanding of how mathematics can be used by people with much more training than he had. Of "Political Science," he put emphasis on the word "Science." But he certainly was not a mathematician." (Bueno de Mesquita 2021). As to mathematical education for graduate students, different alternatives existed in the beginning. On the one hand, people like Kramer taught them; on the other, students were encouraged to take econometrics and statistical courses in the Department of Economics or Math. (Peter C. Ordeshook 2021; K. A. Shepsle 2021). For instance, after he joined the Economics Department, James W. Friedman, one of the greatest contributors to GT in the 1970s, helped people in Political Science to get proper game-theoretical training, as Ordeshook recalled. (Peter C. Ordeshook 2021)[14] Furthermore, the graduate program attracted people with good math backgrounds, so very much training was often not required, at least at the beginning. As Bueno de Mesquita put it: "In the early years' many students came in with good mathematics background, Dick McKelvey came with a lot of math, but he was clear that was not going to be perpetually true, so Bill established a course often taught in the summer, a math course for political science. The essentials of calculus, matrix algebra, and linear algebra. The graduate students who were interested in modeling, of course almost everybody had to come with math, or to take a math course." (Bueno de Mesquita 2021) It is remarkable that no formal training in Economics seems to be required, let alone Mathematical Economics. It was just a matter of mastering the basic techniques, i.e., Mathematics and Statistics, not of falling prey to any so-called "imperialism" of Economic science.

6.1.3 *The Spreading of Formal Political Theory in the 1960s and early 1970s*

In 1962, THE SAME YEAR of Riker's appointment, the University of Rochester hired the renowned statistician and economist W. Allen Wallis. Wallis had previously been the Dean of the University of Chicago Graduate Business School and came to Rochester to assume the chairmanship of the University, eventually

13 For a brief discussion of this intellectual history see: Barry 1970

14 See below

becoming also the chancellor, between 1970 and 1976. Under Wallis' mandate, the University completed the transition from a small regional university to a U.S. leading research institution. Besides, Wallis was not only a gifted administrator. Instead, he was part of an extended network of people. At the University of Chicago, he had established strong links with people like the future Nobelists Milton Friedman and George Stigler. He was also a member of the Mont Pèlerin Society, the association of liberal and free-market scholars established after WWII.

Teaching at Rochester was pivotal in establishing Positive Political Theory during the 1960s. During the same period, Riker also was trying to build a network of scholars who shared a similar theoretical agenda. In Virginia, James Buchanan and Gordon Tullock adopted Price Theory and Cost Analysis to address issues spanning from public finance to voting theory, and their activities paved the way for the establishment of Public Choice. Throughout the decade, a series of meetings, academic conferences, and training programs, patronized both by Academic Institutions and such organizations as the National Science Foundation and the SSRC, were organized. [15] Indeed, in the 1960s, after the publication of the "founding text" of Public Choice, *The Calculus of Consent* (James M Buchanan and Tullock 1962), Buchanan & Tullock started a series of conferences around "non-market decision making." Equally, at the beginning of the 1960s, the Department of Government at the Southern Methodist University in Dallas hosted a series of conferences on "Mathematical Application in Political Science," beginning in the summer of 1964 and lasting until the beginning of the 1970s. There, the emphasis was given to theoretical contributions, like voting models, but also to statistical analyses. Eventually, these conferences moved to the Virginia Polytechnic Institute in 1966 since their organizer, Joseph L. Bernd, had moved there. Still, the Virginia Polytechnic was the hometown of Public Choice, thanks to the presence of Buchanan.[16] Besides, third and fourth conferences on "non-market decision-making" were held in New York and Chicago, organized by Riker, in 1966 and 1967, under the patronage of SSRC.

The following pages aim to throw some light on these activities and present how some networks were established.

In the summer of 1963, when still an Associate Professor of Political Science at the Southern Methodist University, Bernd invited Riker to join the planned conference on Mathematical Applications in Political Science, to be hosted the following year. In his invitation letter, Bernd described the conference as an opportunity for post-doctoral researchers to expand their training in formal techniques in political science and consult on research projects. Therefore, Riker was asked to present a paper about GT and legislative policy. (Bernd to Riker, July 18th, 1963, WHRP, Box 11, Folder 2) At that conference, Riker met for sure Downs, who was among the lecturers. However, only Riker's and Downs' lectures addressed theoretical aspects. Riker's was about cyclical majorities, reprising some themes from his 1961 paper. Downs, instead, gave a general appraisal of rationality in politics and political modeling. ("Program of the Conference on Mathematical Applications in Political Science," Riker n.d., Box 11, Folder 2) Riker's lecture then was also published (in a revised version) in the first conference proceedings[17]. In the published version, Riker reassessed Arrow's

15 See the first chapter

16 On the "Virginia School," from which the "Public Choice" would flourish, see Levy and Peart 2020.

17 While Downs's did not

paradox (see the previous chapters, especially the third) with a focus on two episodes where cyclical majorities had occurred: one was the case of the "Powell amendment" in 1956; the second, already discussed in previous work (Riker 1961) was the failure to pass the 17th amendment to U.S. constitution in 1911 (the direct elections of the U.S. Senate).[18]

The other lectures at the conference addressed statistical topics to analyze political behavior, both in the fields of domestic politics and international relations, and were given by renowned statistical scholars like Harold Guetzow, Donald Stokes, and Sidney Ulmer. In general, thirty-five scholars attended the first conference between July 19th and 29th, 1964 (the average age was 39: Bernd to Riker, March 16th, 1964, WHRP, Box 11, Folder 2). With few exceptions, all the applicants lacked advanced mathematical and statistical skills. Also because, as remarked by the organizer, it was given prominence to those scholars lacking advanced training in these specific techniques for the pretty obvious reason that this group was by far more conspicuous in the community of political scientists.

Riker was invited to join for the second conference, too. In the remaining part of the 1960s, these scholarly meetings and subsequent proceedings catalyzed the exchange of ideas and the development of new, increasingly formal political theories. Besides, they favored the "meeting of minds" that made the propagation of formal political theory possible. (Bernd, Claunch, and Herndon 1964-73; Steven G. Medema 2009) Furthermore, from the second conference onward (1965), the statistical analysis paved the way also to an expanding number of theoretical and even methodological contributions. In this, the influence of Riker can be easily detected, as the exchange he had with Bernd could confirm. Indeed Riker exposed to him his concerns regarding the status of political science, deemed as "far behind Economics in quantification," the discipline "hav[ing] had no Alfred Marshall." (Bernd to Riker, July 31st, 1964, WHRP, Box 11, Folder 2) Eventually, the second conference (July 18th-August 7th, 1965) encompassed a decision-theoretic analysis applied to political campaigning (specifically, to investigate if a door-to-door canvass of voters maximized the size of each candidate expected plurality. Kramer 1966); a reassessment of the size-principle by Riker; and finally a landmark paper by Otto Davis and Melvin Hinich, from Carnegie Mellon, where they explored the issue of policy formation in an advanced theoretical framework. (Otto A. Davis and M. Hinich 1966)

Due to the latter's importance, let us spend a few words on this result. Davis & Hinich's work entailed the first mathematical exposition of the spatial preferences model. As seen, the idea of spatial preferences in politics dates back, albeit with differences, to the works of Duncan Black, Anthony Downs, and, even before, Harold Hotelling.[19] However, their works had not explored the problem in depth from a mathematical point of view. Black limited his analysis to single-peaked preference functions, while Downs instead had just overlooked this point,

18 The Powell Amendment case became an example on which Riker often returned. In a nutshell: the issue at stake was a bill for school construction proposed to the House of Representatives in 1956. The bill failed when it was proposed to limit the aid only to desegregated schools (the "Powell amendment"). According to Riker's interpretation, this was an example of a cyclical majority. A majority of representatives were in favor of the bill (both Republicans and Democrats). Those who were against it (mostly Republicans) favored the introduction of the amendment in order to split the opposite group, inducing the southern Democrats to oppose the bill. As a result, the amended bill failed to pass. Riker 1969; Riker 1982.

19 See the third chapter of this dissertation

focusing on the probability distribution of voters' preferences over a single-issue space.

Both Davis and Hinich had not been trained as political scientists. Davis had a Ph.D. in economics from the University of Virginia, where he studied under James Buchanan. Hinich had obtained a Ph.D. in statistics from Carnegie Mellon (he also had a BS and MS in mathematics). (Melvin J. Hinich and Munger 2006; Jones et al. 2011) Hinich had spent many years working as an applied mathematician for research institutions such as Bell before the two met in Carnegie-Mellon. At Carnegie, Davis was one of the promoters of the School of Public Policy and Management, established in 1968.[20] Hinich also worked at Virginia Polytechnic and finally at the University of Texas in Austin.

The most crucial accomplishment in the 1966 paper consisted in formalizing the spatial theory of voting and elections. Thus, they defined in a more precise way Downs' theory, showing why it was a dominant strategy for each candidate to adopt the median voter's preference for the case of a single policy issue. Besides, they extended this result also to the case of n-issues, showing that in that case, if the density of preferred points is normal, the dominant strategy encompassed choosing a vector of policies that coincided with the vector of the means of the voter's preferred positions. Another result in this paper encompassed the quite intuitive fact that given two parties, if the variance of the density of preferred points for the first was much smaller than the second, then the first party could win a plurality, even though it was the smaller party. (Otto A. Davis and M. Hinich 1966, pp. 189–95)

Other than the formal results above, the importance of their analysis rested upon the formalization of Rational Political Choice and the basic generalization of the idea of "Political Space." The issue to be investigated, in the authors' words, was the following:

> "Given the precisely defined [...] and unchangeable preferences of the voters in the population, candidates for public office compete for votes by announcing before the election their exact position on each of the relevant issues. Each voter compares the position taken by the various candidates and casts his vote for that particular candidate whose position is "nearest" [...] his own most preferred position. It is assumed that once elected, a (former) candidate will adopt those policies which he announced during the campaign. Thus the questions to be answered are whether, and under what conditions, dominant strategies exist for the candidates." (Otto A. Davis and M. Hinich 1966, p. 176)

Assuming that, first, policies can be measured by certain indices and, second, that such indices can be normalized for all the voters, then Davis & Hinich represent each voter i's preferred position by a vector, $\vec{x}_i = [x_1, x_2, \ldots, x_n]'$. Similarly, a vector can also represent each candidate j's preferred policy. Therefore, each voter can build a loss function measuring the distance between his preferred point and a candidate's preferred position:

$$L_i(\theta) = (\vec{x}_i - \vec{\theta})' A (\vec{x}_i - \vec{\theta}) \tag{14}$$

This is a quadratic utility function (quadratic form), where A is a symmetric positive definite matrix. Therefore, the idea of Rational Political choice can be

20 See below

addressed in a precise way by noting that each voter's loss function "has an obvious relationship to the economist's notion of a utility function" because it incorporates the idea of declining marginal utility, one of the fundamental notions underlying utility analysis. Each voter then chooses that candidate, which minimizes his loss function. (Otto A. Davis and M. Hinich 1966, p. 178) Though rather straightforward, these assumptions provide an important starting point to characterize formally individual political choices. Given two candidates, 1 and 2, each i voter will cast his vote for the first candidate if $(\vec{x}_i - \vec{\theta}_1)'A(\vec{x}_i - \vec{\theta}_1) < (\vec{x}_i - \vec{\theta}_2)'A(\vec{x}_i - \vec{\theta}_2)$. The authors then showed that, where $n = 1$, the dominant strategy for each candidate was to adopt the median voter preference: $\theta_j = \theta*$. [21]

The idea of individual loss function was borrowed from statistics. However, it was easy to generalize it in a mathematical model, where for any n-alternatives, a choice function exists that maps preference into a Euclidean space R^n. Therefore, the utility function corresponds to the euclidean distance between a voter's preferred alternative and another one. This is the fundamental mathematical assumption on which all the formal developments of Spatial Voting Theory from the 1970s onwards are based, including two of the most important results in highly formal political science: the extension of the median voter theorem to the case of multidimensional issues (Otto A. Davis, DeGroot, and Melvin J. Hinich 1972); and the proof of the persistence of intransitivities in multidimensional voting models (McKelvey 1975. See also Austen-Smith 2006)

The fact that a fundamental contribution like Davis & Hinich's 1966 paper was presented at one of those conferences on Mathematical Political Science bears witness to the fact that the efforts by Riker and his affiliates were starting to produce fruit. Going back to the conferences themselves, it is important to underline that, other than strictly theoretical contributions, the foundations of the models were also a matter of discussion. For instance, in the fourth conference, organized at the University of Virginia, where Bernd had moved, Otto Davis explored some methodological issues that complemented those Riker dealt with in his early works. (Otto A. Davis 1968)

After Bernd moved to Virginia University, the latter became the place that hosted the subsequent six conferences on mathematical political science, lasting until 1973. Eventually, both Davis and Hinich joined the "Public Choice" society (established initially as the "Committee on non-market decision making" and later as "Public Choice Society" in 1968) and became its president.[22] Before them, Riker himself had the same tenure: his appointment in 1966, exactly after the mandates of Buchanan (1964) & Tullock (1965), shows how close was his Rochester group to the activities of the Virginia group.

21 This is nothing but the "median voter" theorem, now demonstrated in a precise way. Given $n = 1$, each voter's loss function is simply the distance between his preferred position and each candidate's preferred position: $|x_i - \theta_1| > |x_i - \theta_2|$ (or the contrary). Consider θ^*, that is the median preference, which satisfies the following conditions:

$$P(x \leq \theta^*) \leq \frac{1}{2} \quad and P(x \geq \theta^*) \geq \frac{1}{2} \tag{15}$$

Suppose the first candidate chooses the median position, and the second chooses $\theta_2 > \theta^*$. Then, since the former obtains for sure the preference of at least $\frac{1}{2}$ of the total of the voters, each strategy is dominant. Clearly, to try to win the election, the second candidate also needs to select a median platform. (pp. 181-2)

22 Davis between 1970 and 1972. Hinich between 1992 and 1994.

While there is no doubt that the impact of Public Choice on postwar social sciences was by far more significant than that of Positive Political Theory (especially after James M. Buchanan was awarded the Nobel Prize in Economics in 1986), their development partially overlapped, and the two sub-fields paralleled and grew up together.[23]

Four years after the first meetings of the Committee on Non-market Decision-Making," together with the change of name to "Public Choice Society," the eponym journal was created (1968). The Journal evolved from the "Papers on Non-Market Decision-making," a review that lasted between 1966 and 1967 and originated from a series of conferences organized in 1963 and 1964 by Buchanan, with Riker as a guest. Riker positively appraised the work of Buchanan & Tullock and favorably reviewed *The Calculus of Consent* in 1962. (Riker 1962a) Besides, in their work, Buchanan & Tullock also analyzed simple majority voting through game theory in characteristic function form, albeit in an elementary model. In doing this, it is apparent that they benefited from Riker's comments. (James M Buchanan and Tullock 1962, p. 150)

The main difference between the two approaches rests in Buchanan and Tullock's normative stance (even if their analysis also entailed a positive analysis) and its link with the analysis of Public Finance from a theoretical perspective. Moreover, finally, Buchanan and Tullock adopted Chicago- Style Price Theory over game theory and mathematical modeling (even if in their 1962 work, some game-theoretical notions regarding n-person games were briefly explored).[24]

In 1966, Mancur Olson, who, the year before, had authored his pivotal analysis of collective choices (Olson 1965), submitted to the SSRC the request to create an interdisciplinary committee to organize a series of conferences on non-market decision-making.(Steven G. Medema 2000) This idea paralleled a similar initiative advanced by Riker in August 1966. (Bryce Wood to Riker, September 20th, 1966, LWMP, Box 29) Unfortunately, both requests were rejected, but the SSRC favored sponsoring a new conference, and especially a summer training program in mathematical political science.

Riker acted as the chairman of the fourth Conference on Non-Market Decision-making, held in Chicago on 8-9th December 1967. During the two days conference, an econometric model of fluctuations in U.S. voting behavior, developed by Gerald Kramer, and a discussion concerning the frequency of voting paradoxes, were discussed.[25] Finally, sociologist James Coleman presented a paper concerning the marginal utility of voting. (Coleman 1968)The list of invited participants gives an idea of the kind of network he had in the late 1960s. Among the others, there are some familiar names, like Davis, Downs, Olson, Kramer, and Ordeshook; cutting-edge political and social scientists, like James Coleman, Vincent Ostrom, Hayward Alker, and Philip Converse; finally, economists and philosophers like Oliver Williamson (future Nobelist in 2009), John Rawls and game theorist (and future Nobelist in the 1994) John Harsanyi. (McKenzie n.d., Box 29)

23 For a history of the genesis and development of Public Choice, see Steven G. Medema 2009; a highly critical (and criticized) perspective is offered by: MacLean 2017. See also Boettke 2019; Levy 2017. On the "Virginia School of Political Economy," which in the 1950s predated the Public Choice, and on Buchanan's political, economic, and philosophical ideas, see Levy and Peart 2020.

24 I will return on this difference in the conclusive next chapter of this dissertation

25 The latter, in particular, is a critical issue in assessing the positive value of formal voting analysis.

A summer course in Mathematical Political Analysis was held in 1968, with the joint patronage of the SSRC and the Inter-University Consortium for Political Research (ICPR), at the Univesity of Michigan, Ann Arbor. The course was divided into two parts. The first, directed by Riker, stressed the main issues of newborn "Positive Political Theory" as Spatial Voting Theory and GT. The second, directed by Kramer, investigated advanced econometric techniques for political model building. ("Report to MSSB on Summer Course in Mathematical Political Analysis," Riker 1968) This course is interesting for two main reasons: first, as Riker wrote in the report submitted to the "Mathematical Social Sciences Board" within SSRC[26]: "almost all the participants asserted that the course covered materials not available in the curriculum of their home schools [...]"; second, all the teaching staff (Riker, Kramer and Riker's graduate student William Zavoina) were part of the Rochester political science community, so that this course could be interpreted as "Positive Political Theory" setting foot in a place, the University of Michigan, that was the stronghold of the quantitative (but not theoretical) analysis of Political Science that accompanied the behavioral revolution.

The complex of these initiatives gives the picture of a vibrant intellectual community centered around Rochester, but with layers also at Virginia and, especially, Carnegie Mellon, where different Rochester Ph.D. were recruited (notably Ordeshook and McKelvey). According to Ordeshook, Carnegie Mellon was as crucial to developing formal political science in Riker's mold in the 1970s as Rochester itself. It was the academic institution where Herbert Simon spent most of his career and was working when he was awarded the Nobel Prize in Economics in 1978. However, Simon had virtually no influence on political science there. A decisive role was played instead, other than by Davis and Hinich, by economist Allen Meltzer, the Dean of the Business School, himself interested in the causes of government growth, and by William Cooper, who succeeded Davis as the Dean of the School of Urban and Public Affairs and recruited McKelvey once he finished his Ph.D. with Riker, in 1974.(T. Palfrey 2005; Peter C. Ordeshook 2021)

At Carnegie, the group made up of Ordeshook, McKelvey, Davis, Hinich, and Howard Rosenthal (a political scientist and another MIT Ph.D.), James Lange (sociologist), Thomas Schwartz (social choice theorist) offered some of the most important contributions to the second generation of works in "Positive Political Theory," eventually completing its transformation from the pioneer, and not adequately formal, early works of Riker, to a fully-fledge axiomatic and mathematical research field.[27] In 1978, McKelvey moved to Caltech, establishing the last and perhaps most advanced (from the mathematical point of view) layer of Positive Political Theory in the 1980s and 1990s. (T. Palfrey 2005) By then, one could safely claim that Riker had eventually concluded his battle to establish American Political Science on formal grounds.

26 See the first chapter. To the board belonged, among the others: Noam Chomsky, James Coleman, Robert Dahl, Lionel McKenzie, Frederick Mosteller, and Parick Suppes

27 Among these works, for instance, the first attempt to map congressional preferences in a spatial setting by Rosenthal and Keith Poole.(Poole and Rosenthal 1985) Further, all McKelvey's main papers on the pervasiveness of intransitivities in Euclidean preferences space were an outcome of his years at CMU between 1974 and 1978.

6.2 SOME DEVELOPMENTS OF POSITIVE POLITICAL THEORY

THIS FINAL SECTION aims to show some developments of positive political theory. In a review about the "future of a science of politics" (Riker 1977), Riker outlined three ideas that, in his view, were passable of being analytically treated in a fashion following closely economic theory. These are: the somewhat intuitive idea that simple majority elections in single-member districts favor the two-party systems; the Median Voter Theorem, and more generally, the spatial analysis of voting; finally, the "size principle" and coalitional politics.[28] In the last section, I briefly showed what the development of spatial analysis owed to Davis & Hinich's (but also Ordeshook and McKelvey's) precise characterization of utility functions for political actors and Euclidean preferences space. For what concerns the first point, simple majority elections, also known as "Duverger's law"[29], it was examined in a highly formal fashion from the 1980s onward, using non-cooperative game theory (see especially Cox 1997). The case of the size principle and coalitional politics is slightly different. As seen, it was the most ambitious theoretical contribution made by Riker. At the same time, it was criticized on both theoretical and empirical grounds, leading to some attempts by Riker's associates like Shepsle and McKelvey to refashion it more precisely, still using Cooperative GT but not necessarily the von Neumann & Morgenstern's solution. Coalitional politics still occupies a pivotal role in the formal analysis of politics. Therefore, by briefly addressing even the content of one of the cornerstone results in this field, David P. Baron and John Ferejohn's 1989 result on bargaining in legislatures (Baron and Ferejohn 1989), I will try to detect some lines of development that moved away from Riker's original result.

6.2.1 *Elaborating upon the Size-Principle*

THE SIZE PRINCIPLE was the main accomplishment of Riker's analysis of political coalitions. As seen, it states that in n-person zero-sum games, where side-payments are permitted, only minimum winning coalitions occur. This principle means, first, that the strength of a coalition is not a monotonic function of its size; and second, that, in such situations where there are coalitions larger than the minimum winning size, it is rational to expel some members, to attain the maximum payoff. It was allegedly easy to assess MWC empirically by looking at it in real-world politics or political history. This fulfills Riker's aspiration of a "positive theory" of political behavior and not an exclusively mathematical analysis. The importance of this principle in Riker's theoretical work was even strengthened by the apparent weakness of the second part of his analysis, that is, the dynamical model of coalition-building. However, the result was criticized on theoretical and empirical grounds.

Focusing on the "theoretical side," most critiques centered around Riker's treatment of the issue of "equilibrium" that he read in terms of the "realizability" of an MWC, the role of side payments, and finally, the role of transferable utility.[30] Of these critiques, the first two broadened the perspective of Riker's analysis, determining a shift in the 'tool-box' of game-theoretical political coalition analysis

28 Henceforth often SP
29 After the French political scientist Maurice Duverger
30 Henceforth TU

from the "stable set" solution idea to the "Bargaining Set." Game Theorists Robert Aumann and Michael Maschler first introduced this idea to integrate the existing cooperative solutions (the "stable set" and "the core") with some behavioral considerations. (Aumann and Maschler 1964) The intuitive idea behind this notion is that the solution of a game is provided by a couple representing a stable payoff vector and a coalition structure. To be "stable," the members of each coalition must be able to defend their payoff against the possible objections of their partners.

Political scientist Robert Butterworth attacked the SP by stating that a more precise idea entailed focusing on the attempts not to be expelled made by some members of a non-minimum winning coalition. (Butterworth 1971) Although flawed from a game-theoretical perspective, this intuition pushed Kenneth Shepsle to investigate the plausibility of the SP with a more mathematically sounding analysis. (K. A. Shepsle 1974). Therefore he introduced a slightly simplified version of the "Bargaining Set." However, his analysis, too, was incomplete. Finally, McKelvey and Richard D. Smith were able to provide a consistent mathematical demonstration of the principle by using the "bargaining set." (McKelvey and R. D. Smith 1975)

The main issue with their analysis, though, was that it had lost any "positive feature," being, therefore, a purely mathematical result.[31] To overcome this difficulty, McKelvey addressed the problem by eliminating the hypothesis of transferable utility, elaborating, and experimentally testing, alongside Ordeshook and Mark D. Winer, a new solution concept for n-person games, the "Competitive Solution."(McKelvey, Peter C. Ordeshook, and Winer 1978)

In a paper published in the *American Political Science Review* Butterworth disputed Riker's SP showing that if side-payments are permitted, then: "*no incentive can be found to cause rational players to reject the initial 'unrealizable' coalition, in favor of an immediately available alternative.*" (Butterworth 1971, p. 741, italics in the text) Significantly, this was a different way to look at the function of side payments. In Riker's analysis, a leader uses side payments to add members to a coalition. Butterworth instead considered the possibility of some kind of exchange or "bribery" among the players. Suppose a coalition member's loss of being expelled is higher than what it may cost him to remain in the coalition, although with a lesser, or even negative, payoff. In that case, this member could "bribe" his companions for not being expelled. This point is apparent with a simple numerical example.[32]

Take a five-player zero-sum game.[33] There are three possible partitions of winning coalitions: ([1,2,3,4,5]); ([1,2,3,4];[5]); ([1,2,3];[4,5]).[34]According to Riker's SP, the expected winning coalition must be that of minimum winning size, namely a coalition made up of three members. Assuming the following characteristic functions for each partition:

31 This aspect, together with the apparent simplicity of the mathematical result, could also explain why this proof was never published but remained in a mimeographed form as a CMU working paper. Nevertheless, McKelvey, in other words, referenced it.

32 Note that Shepsle and McKelvey & Smith adopted the same numerical example in building upon Butterworth's idea.

33 Butterworth's analysis is not axiomatic. Other than zero-sum, this game needs to be symmetric. McKelvey also analyzed the simple games. A simple game is a game where only two possible outcomes are possible: winning and losing. Formally, $v(B) = 0$ or $1 \quad \forall B \in P(N)$ ($P(N)$ is the set of all subsets of N)

34 Note that I am referring to partitions, not the total number of winning coalitions.

1. $v(1,2,3,4,5) = 0$
2. $v(1,2,3,4) = 20$ & $v(5) = -20$
3. $v(1,2,3) = 30$ & $v(4,5) = -30$

To these correspond the following imputations:

1. $v(1,2,3,4,5) = (0,0,0,0,0)$
2. $v(1,2,3,4) = (5,5,5,5)$ & $v(5) = (-20)$
3. $v(1,2,3) = (10,10,10)$ & $v(4,5) = (-15,-15)$

If side payments are allowed, the following question arises: Is it possible to find an incentive for three players in a four-players coalition to expel the fourth player and reach a minimum winning size? From the values above, a three-players coalition could obtain a total value of 30. Namely, each player could gain an additional 5. However, for the fourth player, the total change is a drop of 20, passing from receiving 5 as a member of the winning coalition to -15 as a member of a losing coalition. Therefore, he could prefer to avoid this total loss, for instance, by offering 6 to each player in the coalition for letting him stay. The four-player coalition is still valued 20. The imputation for the coalition is now the following: $(11,11,11,-13)$. It clearly dominates the imputation $(10,10,10,-15)$ that corresponds to what each player would receive in the winning coalition $v(1,2,3)$ and what the fourth player would lose, being part of the losing coalition $v(4,5)$. Therefore, the winning coalition's size will be 4 and not 3, contrary to the size principle.

As Butterworth summed up this result: "the payoffs as given by the rules to the three-person winning coalition dominate those given to the four-person winning coalition, as Riker says; *but* the payoffs of the four-person winning coalition with bribing in operation dominate those of the three-person coalition without bribing." (Butterworth 1971, p. 742) He also showed that this case holds for at least two of the three situations outlined by Riker, namely when the value of the winning coalition decreases with the growth of its size (the example above; in Riker's language, where the winning coalition's characteristic functions are negatively sloped); when the slope is zero. Notice that Butterworth did not reject Riker's analysis *in toto*, but generalized it by proposing a new principle called the "maximum number of positive gainers principle." (MPG) This principle states nothing about the size of winning coalitions. Instead, it asserts that there is a range within which the number of positive gainers must fall (the positive gainers must be members of minimum winning coalitions). In other words, even if there is a positive incentive, say b, for a coalition to expel members by reaching a minimum winning size, it is not necessary to expel members to gain b. Butterworth claimed that each example explained by the "Size Principle" is equally explained by MPG, which, in addition, also shows why some winning coalitions pay negative payoff to their members.

Riker's reply, on the same issue of *The American Political Science Review*, focused on an apparent inconsistency in Butterworth's reasoning. Namely, assessing that a four-player coalition's payoff could be $(11,11,11,-13)$ is equivalent to saying that a 3-players coalition worth 33 exists, violating the rules of the game, according to which a three-person coalition is worth 30. (Riker 1971) Nevertheless,

Butterworth had raised several important theoretical points that the formal treatment should encompass in a more mathematically sounding analysis of the principle. This was the aim of Shepsle's paper, published in 1974, again on the APSR.

Shepsle noted that both Riker's original analysis and Butterworth's criticism had two main problems. First, the inherent instability of zero-sum essential games; second, how the two authors had employed game theory. As he wrote:

> "To speak of equilibrium in a game $G(n, v)$ is to have some *solution* in mind, the elements of which possess certain properties of stability. Having specified a solution concept, and having assured ourselves that the game $G(n, v)$ possesses a solution, it is then (and probably only then) appropriate to ask whether anything general may be said about the size of winning coalitions. *Neither Riker nor Butterworth tie their analysis explicitly to a formal solution to $G(n, v)$ and this, I believe, has been a source of some confusion.*"(K. A. Shepsle 1974, p. 508, 'solution' is italicized by the author. Otherwise, my italics)

As seen, neither Riker's nor Butterworth's arguments were grounded on any formal solution concept. In particular, Riker's adoption of the stable set, namely von Neumann & Morgenstern's original idea of a solution for n-person games, was only justified by his dissatisfaction with many solution concepts developed after that (starting from the "core" and the "Shapley Value"). (Riker 1962b, pp. 38–9) However, this choice undermined the general mathematical consistency of his result. Accordingly, Shepsle's inquiry encompassed the existence of an actual coalition size that alone sustained an imputation in the stable set. Assuming Butterworth's numerical example[35], the stable set of this game is:

$$V = \{ (10, 10, 10, x, -30 - x) \mid -20 \leq x \leq -10 \} \tag{16}$$

This set is infinite since the losses are determinate only up to an interval. Still, there is no explicit mention of the coalition structure that generates some set elements as an outcome of the game. Any coalition whose size is greater or equal to the MWC can sustain an imputation in V.[36] Since three, four, or five-members coalitions can sustain the imputations in the "stable set," then the size of the winning coalition cannot be determined only by this. A result that clearly proves the flaw in Riker's analysis.

To overcome this issue, Shepsle first introduced two behavioral constraints: first, he defined the *acceptable coalition structure* for each outcome in V;[37] second, each player is constrained only by the characteristic function. In doing this, Shepsle accepted one of Butterworth's points, namely the possibility of bribery activity by members of coalitions. He conveyed that, given a subset V^* of V and a set:

$$V - V^* = \{ (10, 10, 10, x, -30 - x) \mid -20 < x < -10 \} \tag{17}$$

35 See above

36 To see this point. Take $|C| = 3$ (which is an MWC). Then $10 + 10 + 10 \leq 30 = v(1, 2, 3)$. Take instead $|C| = 4$. Then $10 + 10 + 10 + x \geq 20 = v(1, 2, 3, 4)$ is not part of V. Therefore, $|C| = 4$ is sustained by V.

37 Formally, it means that each coalition that form must receive at least their values as specified by the characteristic function: $\sum_{i \in K_j} x_i > v(K_j)$, where $K = (K_1, K_2, \ldots, K_m)$ are partitions of the set of players.

Any coalition C with $|C| = 4$, does not satisfy the first constraint. Therefore, if the outcome of the game is in $V - V^*$, only coalition structures which include an MWC are acceptable. But this is not the case for $V^* = \{ (10, 10, 10, -10, -20) \}$. (K. A. Shepsle 1974, p. 513) Therefore: "the size principle will be strongly sustained in this game if there is some reason to exclude the $y \in V^*$ as likely outcomes. A careful specification of constraint (2) will permit us to conclude that the outcome of the game is among the elements of a subset of $V - V^*$. Hence, the only acceptable coalition structures are those that contain an MWC." (ibidem) In other words, the SP is heavily dependent on some "sociological" considerations about the structure of the game.

To strengthen his argument, Shepsle adopted an idea closely related to another Cooperative GT solution concept, namely the "Bargaining Set." Unlike the stable set, this idea specifies the coalition structure that sustains a given payoff vector. That is, given a game $G(n, v)$, an outcome is a *payoff configuration* $(\vec{x}; \vec{K})$ consisting of a vector of payoffs and a coalition structure:

1. If $x_i \geq v(i)$ for all x in $(\vec{x}; K)$, then it is an *individually rational payoff configuration* (IRPC)

2. If $\sum_{i \in K_j} x_i \geq v(K_j)$, for all K_j in $(\vec{x}; K)$, then it is an *acceptable payoff configuration* (APC)

Any elements in the stable set with a coalition structure K_1 containing only the three positive gainers is both an IRPC and an APC.[38]

Since, according to Shepsle, the best point Butterworth made in his critique of Riker's Size Principle entailed the activity of the member of coalitions (and the second behavioral constraint Shepsle introduced pertained to this aspect), the "bargaining set" was apt to operationalize it. Put formally, consider an IRPC $(x; K)$, and $s, t \in I$. s has an *objection* against t, (in notation $[y^s(t); B]$) if there exists a coalition B, with $s \in B$ and $t \notin B$ and a payoff vector y which satisfies the three following conditions:

$$\sum_{i \in B} y_i = v(B) \tag{18}$$

$$y_s > x_S \tag{19}$$

$$y_i \geq x_i \quad \forall i \in B \tag{20}$$

A player t has a *counterobjection* to $[y^s(t); B]$ if there exists a coalition C, with $t \in C$ and $s \notin C$ and a distribution z such that:

$$\sum_{i \in C} z_i = v(C) \tag{21}$$

$$z_i \geq x_i \quad \forall i \in C \tag{22}$$

38 To see this point:

1. IRPC: $x_1 = 10 > -20$; $x_2 = 10 > -20$; $x_3 = 10 > -20$
2. APC: $10 + 10 + 10 \geq 30$ and $x - 30 - x = -30$

$$z_i \geq y_i \quad \forall i \in B \cup C \tag{23}$$

If to every objection to an IRPC $(x; K)$ there is a counter-objection, $(x; K)$ is said to be *efficacious*. Shepsle finally showed that no vector in V^* is efficacious and that the only IRPCs in V that are both APC and efficacious are those where "*both gains and losses are symmetrically shared by a minimum winning coalition and a maximum losing coalition.*" (K. A. Shepsle 1974, p. 515)

While he limited his analysis to the five-player game and did not generalize it, he so concluded:

> "One message, in any event, is clear. It is unlikely that much can be said with confidence about coalition structure without analytically viable (and perhaps sociologically rich) solution concepts. From Butterworth's initial research and the analysis presented in the first three sections of this paper, it appears that minimum winning coalitions constitute *unstable* equilibrium points in n-person zero-sum games. That is, there appear to be forces in the coalition formation process that drive winning coalitions toward minimal size, but fail to keep them at that point. If, however, the usual assumptions about n-person zero-sum coalition processes are supplemented with assumptions about coalition intentions and capabilities, there are good reasons to expect minimum winning coalitions in all but the most extreme instances."(K. A. Shepsle 1974, p. 515)

In their unpublished 'Comment on The Debate over Riker's "Size Principle,' McKelvey and Smith objected to Shepsle's analysis on two technical points. First, he considered the "Bargaining Set" as a subset of the "stable-set," which is valid only under certain conditions; second, in his example, objectors and counter-objectors did not belong to the same coalitions, in contrast with what the theory prescribes. (McKelvey and R. D. Smith 1975) However, as they wrote: "Despite the above difficulties, we feel that Shepsle is basically on the right track, and his argument that the size principle and the MPG principle are not based on any formal solution concept is correct." In particular, they demonstrated that in a large class of games the size principle follows directly from the assumption of "internal stability" of coalitions, which in bargaining set theories is embodied by the requirement for "coalitionally rational payoff configurations." (pp. 3-4)

Formally, a payoff configuration $(x, B) = (x_1, \ldots, x_n; B_1, \ldots B_m)$, is a pair with $x \in R^n$ and B a partition of N satisfying $\sum_{i \in B_k} x_i = v(B)$ for $k = 1, \ldots, m$. x_i represents the payoff to the i-th player. That is, coalitions that form should get their value. For internal stability of payoff configurations, two other stronger conditions are necessary:

1. Individual rationality if and only if $x_i \geq v(i) \quad \forall i \in N$. This condition requires that every individual gets his value.

2. Coalitional rationality if and only if $\sum_{i \in T} x_i \geq v(T) \quad for \quad T \subseteq B_k \in B$. This condition requires that every subset of an extant coalition gets his value.[39]

McKelvey and Smith demonstrated that the internal stability conditions guaranteed by the "coalitional rationality" give rise to the size principle. The result is apparent from looking at the usual example provided earlier by Butterworth.[40]

39 Note the slight change from Shepsle's. His analysis rested upon individual rationality and the "acceptability" of some payoff configurations. The second condition refers to a partition of I. Instead, McKelvey refers to a subset of a coalition in a partition.

40 And also discussed by Shepsle. See above

In this example, only three-person coalitions within the coalition structure receive their value (i.e., are coalitionally rational payoff configurations). Indeed, three-member subsets of a larger four-person coalition receive less than the value they could attain if they were to form a coalition just of themselves (since in the four-person coalitions, any three members would receive a value of 15 collectively, but a three-person coalition receives a value of 30). Hence, a winning coalition maximizes its value by reducing its size to the minimum winning size. Besides, the same holds for the four-members winning coalitions (namely, those coalitions where the fourth player bribes his place in them).

While the result held for the example above, the two authors generalized it to a large class of games, especially simple and symmetric games.[41] Two other formal conditions were added. First, how the value of a coalition changes when a player is added. Suppose a player is added to a minimum winning coalition, and the marginal value is less than the proportional share each player is receiving. In that case, a game exhibits "decreasing returns to scale."[42] The second condition entails the "inessentiality" of a subset of players in a coalition. A set of players is inessential in a coalition if they can just as well by not joining. Then, they proved two main theorems: in a simple game, where a payoff configuration is coalitionally rational, for some $B_k \in B$, and $B_k \in W$, it is either $B_k \in M$, or some individuals in B_k are inessential for the coalition (namely, a winning coalition is either minimal or some individuals can obtain the same payoff by not joining it). The second theorem refers to symmetric games with decreasing returns to scale (and again, a coalitionally rational payoff configuration) and asserts that in this case, if a coalition is winning, it is also minimal winning.

The results show how fruitful Riker's insights were. Besides, in the late 1960s-early 1970s, other young scholars, like the future Nobelist Robert Wilson, a graduate student at the Stanford Business School, began to analyze social choice theory and voting through cooperative game theory. (See R. Wilson 1971; R. Wilson 1972)[43] However, it was apparent that, once fully axiomatized, the size principle and coalition theory lost much of their simplicity and alleged predictive power. The next section aims to review some attempts to enhance the usefulness of the formal approach for political scientists. This issue is related to the game-theory revolution in Economics from the 1980s onward. Therefore, the section will also explore its impact on Positive Political Theory.

6.2.2 *The game-theory revolution and its impact on "Positive Political Theory."*

COALITION THEORY RESTED at the core of the formal political theory. The attempts to overcome the difficulties mentioned above were twofold: the first rested on Cooperative GT but discarded the critical hypothesis of transferable

41 Symmetric games are games where the value of a characteristic function depends only on the number of players. Finally, note that a simple game cannot be zero-sum (because if $v(B) = 1$, then $v(B^C) = 0$, whereas for the zero-sum condition, it should be -1)

42 Formally: if, for all $T \subseteq S \subseteq N$ and $T \in M$, then $\frac{v(S)}{|S|} > \frac{v(T)}{|T|}$ (M is the set of all minimum winning coalitions). If the condition above holds for all $T \in W$ (the set of all winning coalitions), McKelvey spoke of "uniformly decreasing return to scale."

43 That the issues of coalitions and voting are tied is evident by looking at a majority of voters as a 'winning coalition' and the outcome of the voting process as an imputation. For instance, the simplest, now quasi-textbook example relates the lacking of a Condorcet Winner (i.e., the "Condorcet Paradox") to the emptiness of the core.

utility; the second adopted bargaining theory in extensive games, namely the most recent developments of Non-cooperative GT. To the first group belongs the new solution concept for N-person games without transferable utility, i.e., McKelvey's and Ordeshook's "Competitive Solution." (McKelvey, Peter C. Ordeshook, and Winer 1978; McKelvey and Peter C. Ordeshook 1978) The second is represented by one of the most influential and important models in formal political science, i.e., David P. Baron's and John Ferejohn's analysis of bargaining in legislatures. (Baron and Ferejohn 1989) A passage from Ordeshook deserves to be quoted in full:

> "Briefly, bargaining and cooperation are studied formally using one of two approaches. The first (classical) approach is the one illustrated by the definitions of the V-set, Bargaining Set, and Competitive solution, whereby more or less ad hoc restrictions are used to define a subset of the feasible set of imputations or proposals, possibly with some attempt at a behavioral justification. The idea here, of course, is to abandon the idea of point predictions and, by focusing on the properties of sets of outcomes rather than on the specific elements of those sets taken one at a time, to narrow the range of likely outcomes. The second approach accepts the critique of the classical approach that a set-theoretic formulation fails to address the issue of how agreements are enforced (if cooperative agreements are enforceable, the argument goes, it must be the case that they are part of an equilibrium to some appropriately conceptualized non-cooperative game), and proceeds instead with an explicit model of the bargaining process in extensive form. (Peter C. Ordeshook 2007, p. 183)

In Cooperative games, the assumption of transferable utility is directly subsumed in how a characteristic function is defined, namely a real-valued function that assigns a value represented by a real number to each coalition. This number amounts to the quantity of utility that the coalition members can secure and divide among themselves. Assuming that utility is linear with money and that side payments are allowed, then the value a coalition is worth is linear with the exchanges among the players.[44] McKelvey and others showed that this assumption was untenable for Euclidean preferences. Indeed, in that case, one could easily show that for some groups of voters, the value of their coalition cannot be summarized by a single number. (McKelvey, Peter C. Ordeshook, and Winer 1978, pp. 194–5) This calls for a redefinition of the 'characteristic function' as a set-valued function rather than a real-valued function, that is $v(S) \subseteq R^n$.

However, if the hypothesis of transferable utility is discarded, many cooperative results in coalitional games are not guaranteed to exist anymore. To overcome this issue, McKelvey and Ordeshook developed their new solution concept for n-person games, without TU, the "Competitive Solution." They analytically explored this idea in a paper presented at a conference on Game Theory and Political Science, sponsored by the Mathematical Social Science Board of the SSRC. (McKelvey and Peter C. Ordeshook 1978)[45] Besides, they also published a paper with a third political scientist, Mark Winer, where this concept was also experimentally tested.(McKelvey, Peter C. Ordeshook, and Winer 1978)

The rationale behind the new solution was the idea that "potential coalition must bid their members in a competitive environment via the proposals they offer.

44 Note, however, that TU is a fairly technical hypothesis and does not preclude that players exchange resources among them.)

45 See above

"Since several coalitions are attempting to form simultaneously, an efficient bid rewards the critical members of each coalition. (McKelvey, Peter C. Ordeshook, and Winer 1978, p. 200) A proposal of $C \subseteq N$ is an ordered pair (u, C) such that $u \in v(C)$ and $u \in v(N)$. A proposal (u_1, C_1) is 'viable' if, for any two proposals, (u_1, C_1) and (u_2, C_2) if it is not the case that $u_1 <_i u_2$ for all $i \in C_1 \cap C_2$ (this is the set of pivotal players between C_1 and C_2). Then, a proposal is viable if the pivotal players do not strictly prefer a proposal over another into which they pivot. Define K as a set of proposals, a proposal is viable in K if it is viable against all the proposals in K.

In order to define the "Competitive Solution," two other conditions need to be illustrated. One refers to K. If each coalition in K has exactly one proposal, and if all proposals in K are viable against each other, then K is balanced. Since many distinct balanced sets of proposals may exist, the proponents of the "Competitive solution" focused on those proposals that 'upset' K, namely that are "as attractive as possible" to their critical members. Therefore, a "Competitive Solution" is a set K that is balanced and that is not upset by any proposal (u, C).

Apart from its formal properties, in the mind of its proponents, the competitive solution should raise the predictive power of the theories. Indeed McKelvey wrote: "concepts such as the V-set are principally mathematical abstractions without behavioral rationale. Hence, it is difficult to assess their applicability when particular rules constrain bargaining or negotiation procedures in committees."(McKelvey, Peter C. Ordeshook, and Winer 1978, p. 189) The main advantage of the "Competitive Solution," thus, was the possibility of its empirical testing.

The words above could surprise given McKelvey's stance on deeply mathematical analysis. However, they also closely follow Riker's commitment to positive political analysis, stressing the need for analytically and empirically sound theories. Another aspect that pointed to a strong affinity with Riker's approach was the commitment to cooperative game theory. However, as Riker himself noted, "unfortunately, this enterprise comes just at the time that, in economics, theorists are abandoning cooperative theory for non-cooperative theory." (Riker 1992, p. 219) What he was talking about here was, of course, the game-theoretic revolution that swayed the development of economics from the late 1970s-early 1980s onward. Remarkably, one of the issues that fuelled that revolution was the alleged lacking of strong behavioral considerations in existing cooperative theory.[46][47]

Perhaps nothing can offer the clearest view of how little non-cooperative game theory was of interest for political scientists still in the 1970s than taking a glance at Riker and Ordeshook's textbook *An Introduction to Positive Political Theory.* (Riker and Peter C. Ordeshook 1973)[48] In this book, four chapters out of 12 are devoted to GT, totaling 124 pages. More startling is perhaps how the material is presented. One chapter is devoted to n-person games, one to the power index, one to the size-principle, and finally, one to two-person games. Namely, the taxonomy is not about the main feature of the game, that is, if either communication is free among the players and binding agreements are

46 an issue only partially solved by those solution concepts like the "bargaining set."

47 Another point instead entailed the elaborations upon the idea of "Nash Equilibrium" in the 1960s and 1970s, extending it also to extensive games.

48 A similar discourse may be made for another comprehensive review of game theory and politics. (Brams 1975)

possible (in a nutshell, if the games are Cooperative); or not (Non-cooperative games). For what concerns n-person games, under this label, the authors only discuss exclusively coalitional games without even mentioning the case of non-cooperative games. These and the "Nash Equilibrium" are treated only for 2-players situations, alongside the bargaining problem (a Cooperative solution). In the two authors' taxonomy, 2-person games are divided into zero-sum and non-zero-sum, finite and infinite, Cooperative and Non-cooperative. However, very little space is devoted to the Nash Equilibrium itself (if only because in the kind of games they mainly deal with - namely, zero-sum finite non-cooperative games - the Nash Equilibrium corresponds to von Neumann's minimax). Significantly no space at all is devoted to those cutting-edge attempts by Harsanyi and Selten to extend Nash Equilibrium to games in extensive form and incomplete information.

To overlook Harsanyi's and Selten's contributions greatly diminishes the importance of GT for any kind of theoretical analysis, both in economics and political science, as the following words of David Kreps, written in 1991, make apparent:

> "The recent impact of game-theoretic methods in economics [...] traces in large measure from the ability to think about the dynamic character of competitive interactions by using extensive form games. [...] *The great successes of game theory in economics have arisen in considerable measure because game theory gives us a language for modeling and techniques for analyzing specific dynamic competitive interactions.*"(Kreps 1991, pp. 50–1)

Why did Riker and Ordeshook neglect these developments? The reasons could be numerous. One is simply that, by looking closely at the nature and origins of these contributions, it is not surprising that they easily went off the radar of non-specialist scholars. For example, Selten's was written in German and never translated (even if he expanded his early result in another pivotal paper in 1975, published in the *International Journal of Game Theory*). Harsanyi, although a member of the American economists' community, after a Ph.D. at Stanford, under the supervision of Kenneth Arrow, was a faculty member at the Business School at Berkeley. Therefore, he remained peripheral to the community of economic theorists. Nevertheless, the community of game theorists was so small that both had strong relationships with people like Aumann, Maschler, Shapley, and others. Another explanation calls into play Riker and Ordeshook's primary references for game theory. Apart from the canonical work of Luce and Raiffa, for many aspects outdated in the 1970s, the other main sources were the still introductory and somewhat non-technical works of Anatol Rapoport and Morton Davis, dated respectively 1966 and 1970.

Still, these are far from being exhaustive explanations. First, as showed in the previous section, Harsanyi had some contact with the public choice community, while Riker had some acquaintances with game theorists in the late 1960s. Most importantly, if it is true that Riker's handling of advanced mathematical techniques was somewhat defective, the case of Ordeshook was different. His game theory knowledge was certainly superior to what the joint textbook suggests. Indeed, although Game Theory did not occupy a central place in the economics department at Rochester (like in the vast majority of economics departments in the 1960s and 1970s), still already in the 1960s, young economist and game theorist James Friedman joined its faculty. Although less famous than Harsanyi or Selten, Friedman is rather well known for his critical applications of game theory

to market theory.[49] Friedman also helped Ordeshook to expand his knowledge of game theory far beyond the introductory character of Luce and Raiffa's canonical treatment. (Peter C. Ordeshook 2021) Hence, when Ordeshook approached the project of writing a textbook, his knowledge of game theory was probably second to none among political scientists (except for McKelvey).

So, the most convincing explanation for Riker and Ordeshook not paying attention to Non-cooperative game theory is, in my view, the following. Since it is scarcely the case that a scientific contribution is readily adopted, and Harsanyi's and Selten's did not exhaust the issue of how to treat properly non-cooperative games in extensive form[50], it is not surprising that Riker preferred to remain close to the idea of game theory that captured him at the beginning, i.e., games in coalitional form. Indeed, politics, in Riker's view, is both an enforcement activity and a composition of interests.[51] One of the most appealing features of Non-cooperative GT is that Nash Equilibria are self-enforcing; clearly, this idea closely resembles the working of competitive markets. In the case of politics, however, institutions enforce the choices and actions of political actors. But institutions are the outcome of deliberate binding agreements. Despite its dependence on somewhat fuzzy behavioral justifications, n-person Cooperative theory permits focusing on the properties specific arrangements have. It also provides room for compounding different interests, an idea quickly captured by the notions of imputation and set-valued solutions.

Later, the developments of non-cooperative game theory showed that similar results could be reached even with a 'point-valued' solution as the Nash Equilibrium. Not surprisingly, the most successful results also entailed 'cooperative features,' like in bargaining theory. The development of such models was one of the main accomplishments of 1980s game theory. In one of such models, Baron and Ferejohn proposed their analysis of bargaining in legislatures. To conclude this section, I thus provide a brief outline of their crucial result, explaining how it relates to Riker's ideas and paved the way for an entirely new class of theoretical models.

David P. Baron and John A. Ferejohn published their work in the *American Political Science Review* in 1989. (Baron and Ferejohn 1989) The fundamental issue they addressed was that of endogenous agenda formation in a unicameral, majority rule legislature, to which they provide a non-cooperative game theory model. Social choice literature showed that a voting equilibrium was almost impossible to reach in many multidimensional settings. So that, as in McKelvey's most famous result, it could be virtually possible for an agenda-maker to obtain his preferred winning majority by pitting any alternative against every other, to obtain his preferred winning majority. Rather, in 1979, Shepsle had shown how to reach equilibrium in a multidimensional voting space. In a nutshell, it is possible to split a decision on multiple issues so that it is possible to ensure a Condorcet Winner for each single-issue voting (an idea he defined as "Structure

49 Moreover, he also contributed to the first formal proof of the so-called "Folk Theorem," a fundamental class of theorems that relate the existence of some subgame perfect NE to the length of games.

50 Take, for instance, the work on sequential equilibrium by Kreps and Wilson, 1982

51 In the introduction to their textbook, the authors outlined three central processes in politics: "the selection of society's preferences, the enforcement of the choices that revealed them, and finally, the production of goals or outputs that embody the choices."(Riker and Peter C. Ordeshook 1973, pp. 2–7)

induced equilibrium," see K. A. Shepsle 1979). Shepsle then reshaped a social choice problem into an institutional setting, namely that of a committee system in parliamentary legislatures. However, his equilibrium was still "based on a concept of stability drawn from the field of social choice."(Baron and Ferejohn 1989, p. 1181).

Like Shepsle, Baron and Ferejohn focused on an institution-based analysis: the sequential nature of proposal making, amending, and voting. Differently from him, however, they modeled the situation as a non-cooperative, multisession game. They showed that a Nash equilibrium exists for such a game. Besides, this equilibrium differs on the basis of what institutional structure is adopted, namely if a "closed rule" or an "open rule" is adopted. Under a closed rule, no amendment is possible for any proposals. A motion is voted against the status quo, and only two outcomes are possible: win or lose. Instead, in an "open rule" system, amendments can be offered to the motion on the floor. In this process, it is crucial who is the member who either makes a proposal or brings the legislature to a vote (what Baron and Ferejohn defined as the "recognition rule"). The assumption is that, at the beginning of a legislative session, each member i has a probability p_i of being recognized. Given a set X of feasible proposals, a proposal is a vector $x^i = (x_1^i, \ldots, x_n^i)$, s.t. $\sum_{j=1}^{n} x_j^i$. While in a closed rule system, there is only one recognized member, in an open rule system, another member j may be recognized with probability $\frac{p_j}{\sum_{k \neq i} p_k}$, and thus offer an amendment. Therefore the model is considerably more complicated.

Baron & Ferejohn represent the preferences of each legislature member with the utility function $u^j(x^k, t) = \delta^t x_j^k$ (t is the session in which the legislature adopts the distribution x^k). Each player's pure strategy s_τ^i is the set of functions mapping the domain H_τ (the set of histories up to the time τ, namely what happened in the game until τ) into the set of proposals X if i is recognized; Otherwise, into the set {yes, no}. A mixed strategy is a probability distribution over the strategies s. Besides, each player has a *continuation value* $\delta v_i(t, g)$, the value for i if the legislature moves to subgame g.

Theirs is a model of bargaining, where a proposed distribution is advanced and can be accepted or refuted. As seen previously, John Nash presented the first systematic analysis of bilateral bargaining (see Nash 2002b). Following Nash's intuition, Rubinstein offered an axiomatic model of bilateral bargaining, where each player is asked to split a dollar. (Rubinstein 1982) The first player proposes $[p, 1-p]$. If this proposal is accepted, the game is terminated. If not, the second player makes a proposal himself, and the game continues until an agreement is reached. Each player is impatient: a discounting factor δ reduces the outcome as long as an agreement is not attained. This game is modeled as a non-cooperative one with complete information. Then, each player has a set of strategies, consisting of the set of all functions representing an offer and a response.[52] Since this is a multistage game, the only Nash Equilibrium must be a subgame perfect one: it must be the Nash equilibrium in each subgame. Rubinstein had shown that the only solution to this game is that the first proposal

52 F is the set of all strategies of the player who starts the bargaining: formally, the set of all sequences of functions $f = \{f^t\}_{t=1}^{\infty}$, where $f^1 \in S$. For t odd, $f^t : S^{t-1} \to S$, and for t even, $f^t : S^t \to \{Y, N\}$. Similarly, G is the set of all strategies of the player who has to respond in the first move.That is, $g = \{g^t\}_{t=1}^{\infty}$, s.t. For t even $g^t : S^{t-1} \to S$, and for t odd, $g^t : S^t \to \{Y, N\}$. (note the inverse order)

is readily accepted. This result paved the way for an enormous amount of literature.

Baron & Ferejohn's model belongs to this group. However, their analysis outlined some crucial differences with respect to Rubinstein's. First, Rubinstein's model is bilateral, i.e., there are only two players; Baron & Ferejohn's instead is multi-lateral. The second difference concerns the role played by impatience: the equilibrium distribution approaches equality as long as impatience diminishes, in Rubinstein's model, while Baron & Ferejohn's analysis does not display a similar effect. Finally, the subgame perfect Nash equilibrium of Baron & Ferejohn's model changes as the institutional arrangement changes. Then, in a closed rule system with a finite number of sessions, it presents similarities with Rubinstein's (namely, the first proposal is accepted). However, things are considerably muddled when the system is "open rule;" the results then depend, among other things, on the size of the impatience.

One of the most famous outcomes of Baron & Ferejohn's game-theoretic model is that amendment rules impact the equity of resources and their distribution. In the authors' words:

> "With a closed rule, the equilibrium outcomes are majoritarian, the first proposal is passed, and benefits are distributed to a minimal majority. Since each member acts noncooperatively, the majority is not a coalition in the sense the term is used in cooperative game theory. Compared to a closed rule, the opportunity to make an amendment under an open rule reduces the agenda power of the member recognized first and results in an outcome that more evenly distributes the benefits among the majority. Unless impatience is great, however, the distribution is majoritarian, and the proposal made by the first member recognized is not necessarily accepted. If, however, the number of members is small and there is substantial impatience [...], the equilibrium is "universalistic" in the sense that every member receives benefits."(Baron and Ferejohn 1989, pp. 1199–1200)

It is important to underline that this result does not directly undermine Riker's size principle. Indeed it could still be the case that minimal majorities occur.[53] However, the nature of the 1989 result is entirely different from Riker's. In the fifth chapter, I showed that Riker's analysis of political coalitions was twice flawed. First, the size principle, although intuitive, did not entail a strong game-theoretical solution and behavioral justification. Second, in what he called the "dynamical model," Riker did not set forth adequately the implicit bargaining process (and, again, the behavioral premises were ill-defined). Through the "Bargaining set" and the "Competitive solution," scholars like Ordeshook and McKelvey had cleared up the first issue. But, as Baron and Ferejohn wrote: "Cooperative models of politics abstract from the process by which alternatives arise and assume that coalitions will freely form to defeat alternatives when a majority of members prefers another available alternative." (p.1200) Refashioning the problem as a Non-cooperative bargaining problem allowed the authors to highlight the sequential aspects of legislative activity and voting and to complement what Riker had attempted to do almost thirty years before. An outcome that, like so much else in this dissertation, points to the ambivalent nature of Riker's analysis. On the one side, we had his failure, due perhaps to over-optimism

53 A table in Baron & Ferejohn's paper displays different values of equilibria for different discount factors and different sizes in the legislature.(Baron and Ferejohn 1989, p. 1197)

about the effective strength of the Cooperative game theory; on the other hand, the precious insights his endeavor was capable of providing for the analysis of political problems in a new, more "scientific" outlook.

7

CONCLUSIONS

IN THESE CONCLUSIVE pages, I want to advance some considerations on the current state of Rational Choice Theory and Positive Political Theory within contemporary Political Science debates. The heydays of Rational Choice in Political Science came in the 1980s. Then, the enthusiasm for such a theoretical approach froze out, especially since the beginning of the new Millenium. One reason can be the methodological debates in the 1990s when Rational Choice theorists were called to respond to the apparent lacking of empirical validations for most of their analyses. For some political scientists, the ensuing methodological debate enshrined the definite failure of the conquering attitude of Positive Political Theory in Political Science. (see Green and Shapiro 1994; J. Friedman 1995) The consequence was that the theoretical and formal heights reached between the 1980s and 1990s are ever more often subsumed directly into economic theory and the label of "political economy." Likewise, I want to discuss the proper relationship between Economics and Political Science, starting with Riker's work and its evolution. Finally, since this work concerned the History of Game Theory, I want to briefly expose how the reconstruction of the entry of GT into Political Science complements the more general history of the theory of games.

FORMAL POLITICAL THEORY AND MATHEMATICAL ECONOMICS. RIKER'S "DILEMMA"

BEHAVIORALISM DID NOT supply Political Science either with a unified method or a unique theoretical framework, like, on the contrary, the postwar mathematical approach did for Neoclassical Economics. Nevertheless, Behavioralism represented the discipline's mainstream during the 1950s and 1960s. After that period, nothing similar emerged in political science.

The demise of behavioralism was due to different causes, most notably the dramatic changes that affected American Social Sciences and Society in the late 1960s. The Vietnam war and the students' movement called for a different approach to social studies, and no discipline was excluded.[1]Many young scholars started to dispute Political Science's role in interpreting and fueling these events. For instance, within the heterogeneous groups that opposed the Vietnam War, the "Caucus for a New Political Science" was created in 1967. (Dryzek 2006) Despite some resistance from the "Behavioralists," David Easton, in his 1969 APSA presidential address, advanced a conciliatory attempt to consider these new positions. Easton explicitly talked of a "new revolution in Political Science" and identified seven central tenets of "what could be called a Credo of Relevance" for this "new revolution." (Easton 1969, p. 1052) These entail, for instance, the

1 Take the case of Economics. In 1968 to protest against the decision to hold the meeting of the American Economic Association in Chicago, where in August the police had brutally assaulted mostly pacific demonstrators, a group of renowned economists, most notably Lawrence Klein (the future 1980 Nobelist), arranged, in an unprecedented and never repeated move, an alternative meeting in Philadelphia.

ideas that "substance must precede technique" or that "behavioral science conceals an ideology of empirical conservatism."

In general, the battle cry of the revolutionaries encompassed a politicization of the profession. Thus, it rejected the pretense of value-free social science, one of Behavioralism's main facets. Even because, as Easton noted, Political Science as an enterprise has failed to anticipate the social and racial crises that invested the US in the last part of the 1960s.[2]

Finally, Easton also advanced a plea for "creative speculation." By this formula, he meant, in the steps of "the great political theorists of the past," "new and often radically different conceptions of future possible kinds of political relationships." (Easton 1969, p. 1058) But aside from what seems an appeal to utopia, he called for "boldly speculative theorizing that is prepared to build upon rather than to reject the findings of contemporary behavioral science itself and that is prepared to contemplate the implications of these findings for political life, in the light of alternative, articulate value frameworks." (ibidem)

As shown previously, Riker's attitude toward the Behavioralists exhibited little, if any, hostility. Quite the contrary, his theoretical agenda paralleled and complemented (at least in his view) the reformist agenda advanced by them. Nevertheless, he also pointed out what seemed to him the significant defects of behavioralism, especially its loose theoretical attitude and the missing of a clear picture of individual action. In his words: "the behavioralists [...] were totally atheoretical. They had no picture of human beings."(Riker and K. Shepsle 1979, p. 21)[3]

Furthermore, Riker also interpreted the development of Political Science as a cumulative enterprise, with any better theory supplanting or complementing the old ones. Thus, he did not join the Post-Behavioral revolution simply because the plea for a politicization of the discipline made no sense to him. Up to the point that he apparently rebuffed even the term "Post-Behavioral." Indeed, to define the call for an orientation of political science other than empirical collections of numbers, the simple notion of "science" was sufficient. Instead, to him, "[t]he post-behavioral revolution, as other people use the word, seems to refer to the success [that] Straussians and others of that sort have had in persuading people that science is biased." (Riker and K. Shepsle 1979, p. 127) He did not contend with the practical or reformist motivation for developing a science of politics. Still, he held, even such an enterprise required scientific solid and methodological foundations, as the Behavioralists had recognized, and the Post-Behavioralists were menacing.

Riker never gave up his methodological concerns and commitment to a "genuine science of politics." This view centered around a well-definite expectation: the possibility of producing actual explanations and predictions in social sciences, as Economics seemingly did. However, his view of Political Theory evolved quite differently from the premises above. The change had much to do with the possibility of providing real predictions in political science and the role of "political craftmanship" other than pure and simple "rational action." Besides, it displays how Riker perceived what Positive Political Theory was accomplishing and the relationships between Economics and Political Science. Then, in the following

2 As proof, he reported the negligible number of articles the APSR published in 1958-1968 on hot topics such as racial conflicts, urban crises, poverty, violence, and civil disobedience.

3 Still, to state that behavioral political science displayed an anti-theoretical attitude is a non-entirely accurate description. See the first chapter of this dissertation

pages, I will show, first, how Riker changed his view concerning equilibrium, and second, how he changed his ideas about explanations and predictions in social science. This will point to how Riker interpreted Economics and was influenced by it. But also the differences between how he interpreted it and how this discipline was transforming since the Postwar years.

An important essay to reconstruct Riker's evolving attitude both toward Economics and formal Political Theory was a review he wrote in 1977, significantly titled *The Future of a Science of Politics*. (Riker 1977). In these pages, he again contended the particularistic explanation of political events, emphasizing general analyses instead. Such a view encompassed a "positivistic view of science," whose central element requires a set of scientific laws, namely "well-verified generalizations." Besides, by scientific laws, Riker defined not only those discovered by observation but also theorems derived from axioms. He seemingly attributed the same scientific status and explanatory power to both.[4]

Riker's model is the "Price Theory," that "satisfies, in structure and outcome, [his] notion of what a science is just as well as, perhaps, physics." (p. 22) This theory, he argued, states that prices are determined by equalizing supply and demand in a competitive market. The Law of demand can be derived by empirically validating how the quantity demanded varies when the prices rise or fall. This evaluation can be extended to axiomatic theory, so developing the theory of consumer choice. Despite the supply side of the price theory being less satisfying and intuitive from the empirical point of view than the demand side, economists generalized both demand and supply into a theory of competitive equilibrium. In Riker's words:

> [This theory] contains all the elements in our previous description of a science. It starts with an empirical law, which is presumably universal when properly restricted. This Law is then imbedded [sic] in a theory of choice. In turn, this initial theory is elegantly elaborated to produce a nonobvious and far from trivial inference about market clearing, which is in turn strongly supported by empirical evidence." (Riker 1977, pp. 21–2)

As seen previously, Riker identified three political problems that could be subject to theoretical treatments like economists had done with Price Theory. These were the Spatial Theory of elections, the influence that voting rules have on the outcome of elections, and finally, the theory of political coalitions and the size principle.[5] He also outlined several points that could favor the flowering of his rational choice approach in political science. First, these theories have the "essential structure of science" since they are empirical laws and can be axiomatized to generate theorems about how people interact. Second, these political theories encompass the notion of equilibrium. Namely, they do not rest on interpreting motives but instead focus on the outcome of social interactions resulting from different purposive actions. Third, all the theories refer to small and often repeated events, a point yet discussed in the philosophical papers in the late 1950s. Finally, Rational Choice theory solves the problem of interpreting intentionality and human goals without precluding judgments about the motives of human action and their regularity. On the latter aspect, Riker went further,

4 "Law and axioms thus reinforce each other. The necessity of the inference makes the law seem reasonable, and the empirical validity of the law makes the axioms seem true. Thus, with a theory there is a much stronger reason than mere observation to accept a scientific law." Riker 1977, p. 15)

5 See the second section of the previous chapter

stating that "the assumption of rationality serves just about the same function in social science that the principle of mechanism once served in physical science." (Riker 1977, p. 32)

However, eventually, Riker became highly skeptical of the possibility of obtaining useful predictions in Political Science, even using a game theory or formal models. Therefore, his focus shifted from equilibrium to disequilibrium. (Riker 1980) Riker interpreted equilibrium in terms of a stable arrangement of tastes and, consequently, disequilibrium as the impossibility to reach this stable arrangement. In the textbook he published with Ordeshook in 1973, he also presented a threefold categorization of equilibria ("social equilibria"). First, a "Strong, Unique, Equilibrium," the product of interactions so precise (formally speaking) and of goals so specific that society will *certainly* arrive at it (my italics). If some circumstances displace equilibrium, society will return to it as soon as possible. The standard example is price formation in a competitive market, but in Political Science, according to the authors, one can find this type of equilibrium in Social Choice Theory. Secondly, they set forth a "Weak, unique equilibrium," a social outcome that is the product of (usually) more complicated interactions toward more complicated goals (their example was that of monetary Macroeconomics). To these, they added, finally, "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. (Riker and Peter C. Ordeshook 1973, pp. 150–1) Riker identified the size principle as a type of "weak, unique equilibrium." (p. 177) However, it is apparent that the classification above does not say anything about equilibrium existence, namely the mathematical problem of developing a formal model, but reinstates the emphasis on prediction.

In the 1980 paper, Riker was more explicit than before in linking equilibrium and predictions. Indeed, he defined Economics as a theory that "admits predictions of an equilibrium" and explained its prestige, among the social sciences, to the "actual occurrences of numerous predicted equilibria." (Riker 1980, p. 434) Since he associated equilibrium only with predicting actual results, or outcomes, in Riker's vision, Political Science was particularly scattered by such impossibility theorems like those produced by McKelvey and Norman Schofield in the 1970s. These results demonstrated that virtually any situation involving choices in a euclidean space model generates a "cycle," that is, the breakdown of the global transitivity necessary to reach a consistent social choice.[6] Riker interpreted these results as proof that something similar to economic equilibrium could be impossible to achieve in Political Science. Consequently, the latter discipline necessarily involved the study of persistent disequilibrium situations. Then, famously, Riker stated that:

6 Suppose there are n alternatives. Even if no alternative beats all $n-1$ alternatives, it still could be possible to find a set of k alternatives (themselves in a cycle) that beats all the $n-k$ alternatives. This is a "top cycle." McKelvey showed that the 'top cycle' could include all possible alternatives in an n-dimensional space. Norman Schofield demonstrated a similar result. Assume a point x in a multidimensional policy space. For each agent, there is an indifference curve passing for x. Given these indifference curves, one can find the set of points $P_c(x)$, the set of points that some winning coalition prefers to x. Namely, for the set of all winning coalitions, $P_w(x)$ is such that there are some points y that cannot be included in $P_w(x)$ by some path $yR_iz \dots R_ix$. This means that x is always beaten by some point, no matter how the voting procedure is organized. See McKelvey 1975; Riker 1980

> [Not economics but] "politics is the *dismal* science because we have learned from it that there are no fundamental equilibria to predict. In the absence of such equilibria we cannot know much about the future at all, whether it is likely to be palatable or unpalatable, and in that sense our future is subject to the tricks and accidents of the way in which questions are posed and alternatives are offered and eliminated." (Riker 1980, p. 443)

Note that his earlier analysis of political coalitions already encompassed a similar idea. Indeed, Riker devoted many pages to presenting a verbal discussion on the implications of persistent disequilibrium on his model. However, at the time, Riker seemed still convinced that a better theory could have solved this issue. Later, he had become convinced that a significant step was needed that could not entail a simple mathematical procedure. To overcome such a decisive difficulty, Riker stated that it was necessary to return to the study of institutions, especially on their role in determining political outcomes, by rational players' ability to use all the manipulative rhetorical techniques to select their preferred outcomes. This new research program did not dismiss the idea of rational choice or the use of game-theoretic techniques but simply the idea that political outcomes could be predictable. In Riker's words: "The sum of our new sophistication is [...] that political outcomes truly are unpredictable in the long run. We may have few pretty well-verified generalizations to guide us (for example, the size principle or Duverger's Law), but for the most part, we live in a world that is uncertain because it lacks equilibria."(Riker 1980, p. 445)

Riker's point is important, especially for political theory, since the author used this persistence of disequilibrium to provide a view of liberal democracy, as opposed to a populist one (a Madisonian view vs. a Rousseuvian one). Then the prominent discrimination between the two consists precisely in the fact that the first type of democracy, through social choice, does not encompass anything similar to the "general will," which is a type of "equilibrium" because it represents a stable arrangement of tastes and values. Rules and not the outcomes better define a democracy, according to Riker. Indeed, the latter can always be manipulated by strategic voting and rhetoric (Riker coined the term "herestetics" to define this aspect).[7]

This evolution seems to entail a substantial demise of formal political theory 'economic-style.' Wherever the significance of equilibrium is questioned, the prospect of developing a discipline resting on formal analysis (like utility functions or game theory) and not limiting itself to mimic economic ideas is jeopardized. Therefore, Riker's paper called for a reply by other formal political theorists. Ordeshook, indeed, responded in the same issue of APSR, defending a view definitely closer to Mathematical Economics, especially the significance of

7 Riker devoted the last part of his scholarly career, from the late 1970s to 1993 (when he died), to further developing this issue. Among his last work, perhaps the most important is certainly *Liberalism against Populism* (Riker 1982). In this political theory work, Riker reprised and extended the issues of the 1980 paper. In addition, he also presented a historical narrative to show "how political events can be interpreted as part of the continuing efforts by participants (either leaders or losers) to manipulate outcomes to their advantage."(Riker 1982, p. 213). More specifically, he addressed the issue of how slavery became a national relevance issue in the decades before the American Civil War. In subsequent works, starting from the presidential address to the American Political Science Association, Riker similarly explored other events in American political history (starting from the passing of the American Constitution). See: Riker 1984; Riker 1986; Riker 1996 However, Riker's reconstructions have been contended on historical ground. Comment and review of these criticisms are contained in: McLean 2002; McLean 2009.

the notion of equilibrium. (Peter C. Ordeshook 1980)[8] He dealt only superficially with such issues concerning the reliability of equilibria, why a Nash Equilibrium is played, or whether a coalition displays stability. Instead, he focused on the meaning of the notion of equilibrium in economics and, consequently, in formal political theory too. Ordeshook summed up Riker's positions as follows: "Believing that political processes do not share the straightforward stability found in abstract representations of economic markets, he infers that political scientists are disadvantaged in their 'science' in contrast to economists, whose paradigm Riker has borrowed."(Peter C. Ordeshook 1980, p. 447) However, he was not compelled by the "new" Riker.

First, he claimed that Riker attributed too much significance to the idea of equilibrium as stability and, therefore, that price theory itself referred to market stability. Indeed, as it turned out, "the presumed stability of markets is an abstract fiction that most economists recognize as a *a theoretical* impossibility." (Peter C. Ordeshook 1980, p. 447 Italics in the text). It was something existing in the formal model but not necessarily in reality.

The same holds for the predictive power of the theory: abstract descriptions of markets might predict how a change in the general settings of the model affected prices and consumption but could not anticipate what these changes would be. Then, "[...] market's stability is also a fiction of the mathematical abstractions used to represent it."(Peter C. Ordeshook 1980, p. 448) For instance, the value of the coalition of buyers is zero in a market with one seller and two buyers. The core (i.e., the set of all undominated imputations) comprises only those imputations that attribute value to the seller and nothing to the buyers. Namely, the single seller is a monopolist and will extract all the added value from any exchange. Nonetheless, it could be reasonable to assume that buyers could form a cartel and negotiate as a team, and a new equilibrium would arise. For example, this now becomes a bargaining problem, where the monopolist tries to obtain a distribution as advantageous as possible on the Pareto frontier. However, the core consists now of all the points in the Pareto frontier, and therefore, the existence of equilibria is not a sufficient condition for predicting outcomes. (Peter C. Ordeshook 1980, p. 448)

Ordeshook's central point is thus that equilibria are elements of the formal model and therefore display features like existence, uniqueness, and stability, but *do not necessarily entail predictive power*. This argument does not exhaust the question of the impossibility of social choices but instead points to new ways of analyzing the problem. In the author's own words, "theorizing about them requires developing new concepts and [...] the optimism of the past over the ease with which the economists' paradigm could be transplanted into politics must give way to the realization that political scientists themselves must contribute to the development of that paradigm."(Peter C. Ordeshook 1980, p. 450)

In my view, looking at the differences between Riker's argument and Ordeshook's allows a finite appraisal of how Riker used GT and Economic Theory, as well as of how the initial development of Positive Political Theory differed from postwar Neoclassical Economics and other attempts to extend economic reasoning across domains different from economics.

8 In this reply, Ordeshook's position changed with respect to the above-mentioned textbook, suggesting that perhaps that part was mainly Riker's authorship.

As seen, after Weintraub's pivotal studies, historians of economics have interpreted the development of economics as a mathematical discipline relating it to the parallel development of mathematics as a formalist program. (E. Roy Weintraub 2002)[9] Giocoli summed up the radical transformations in Economics between the 1930s and the 1950s as two distinct visions of Economics. (Giocoli 2003b) The first is the idea of Economics as a "system of forces," which entails the idea that its main subject is the analysis of the processes generated by market and non-market forces, including, but not exclusively, the processes leading the system to an equilibrium. Having followed this idea for more than a century, economists eventually replaced it with the alternative vision of economics as a "system of relations," which encompassed the idea of "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 2009a, 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 which is the outcome of the interplay of economic forces that 'generate' it in an empirically meaningful way, whereas other 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. In such a framework, equilibrium either exists or does not but never "arises."

Riker's formal analysis looks 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 partial equilibrium analysis in economic models. Take, for instance, how, in his 1962 work, Riker defined this notion: "[T]he notion of equilibrium is that of a relationship of forces arranged so that the deviation from some point of balance results in a (possibly automatic) correction back to balance." (Riker 1962b, p. 147) See also the threefold classification above.

This can also explain why he was so concerned with the issue of disequilibrium. Indeed, in a "system of forces" framework, disequilibrium and equilibrium have the same importance. 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. But, on the contrary, in a purely axiomatic model and within the notion of equilibrium widely employed in game theory (that is, Nash Equilibrium), equilibrium is simply the necessary outcome of the model, i.e., the solution of a problem modeled like a mathematical problem. Naturally, then, the main issue in such models is the existence or non-existence of equilibrium itself (and, possibly, its multiplicity).

The point is that in Economics, GT was conceived within the "system of relations" framework. This simple historical fact shows how the entry of game theory into Political Science differed substantially from the way it conquered Economics. It also makes it apparent how the task Riker tried to accomplish, especially in his analysis of political coalitions, was arduous to achieve. Not only because Riker lacked the advanced game-theoretic skills that could have made

9 See the first chapter

his research more mathematically robust, as seen in the fourth and fifth chapters of this dissertation; but also because he considered game theory from a viewpoint that was too different from his actual development in mathematics and, later, in Economics.

To further strengthen this point, recall how Riker addressed the issue of rationality in his work on political coalitions (Riker 1962b, 17 et ss.)[10]. 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, from its substantive content. [11] Instead, he presented an idea of rationality based on the basic principle of a preference for winning over losing. His rejection of a preference-ordering argument can be explained by the attempt to make sense for political scientists of the idea of political rationality. This, if only to make an audience not comfortable with mathematical sophistication, which he knew was capable of quickly grasping a more meaningful intuition of such a key concept. However, Riker's argument was feeble and did not fit well in his discussion about modeling in social science. Indeed, Riker defended his assumption of Rational Choice by adopting what he defined as a "summation argument": even if not all agents are rational, the most important agents are. This totally missed the fact that rationality in economics has another and more significant meaning: it is a way to constrain the beliefs and desires people are allowed to have for their actions to make them explainable in a theoretical sense. (Reiss 2013, p. 31) In this sense, even if certain ideas regarding rational behavior according to economic theory can be interpreted as describing actual people's behavior, this does not preclude that the goal of modeling rational behavior requires strong assumptions concerning beliefs and preferences and their formal structure.

One could explain Riker as resting on an old idea of economic analysis as a "system of forces" that, attractive as it might be, was increasingly being displaced in the very area of economic analysis that Riker had selected as his theoretical reference (i.e., GT) by new ideas concerning axiomatization through a view of economics as a "system of relations." An area, it should be added, that until very recently has always been the most distant from the possibility of empirical validation. This, in a nutshell, was Riker's unsolvable dilemma.[12]

POSITIVE POLITICAL THEORY AND "ECONOMIC IMPERIALISM"

ABOVE I SHOWED how Riker's employment of economic theory differed from what postwar Economics was becoming. Nevertheless, I also argued that the case for "Positive Political Theory" was different since the latter, from the 1970s onward, definitely embraced an approach much closer to Mathematical

10 See the fourth chapter

11 What game theorist and philosopher Ken Binmore defined "the consistency view" of rational action: See Binmore 2015

12 In another methodological review paper, published in 1990, Riker returned on some topics yet addressed. Most notably, now he linked Rational Choice and equilibrium to explanation and no more to prediction. He wrote that "the difference between prediction and explanation is that explanation requires much more convincing support." (Riker 1990, p. 167) Sociological laws can offer predictions, but they cannot explain phenomena unless they are "placed inside a theory of equilibrium." (p. 176) Still, Riker did not contend with the philosophical debates concerning the real meaning of explanation in social sciences or if mathematical models really fit this scope.

Economics. The following paragraphs aim to assess if formal political theory development was an "act of conquest" by Economics or an independent event.

Stretching the employment of economic theory and tools outside the traditional domains of Economics (viz., markets, jobs, and money) has been customarily labeled as "economic imperialism" (or "economics imperialism"). Economist Kenneth Boulding allegedly coined the term "economic imperialism," alluding to "an attempt on the part of economics to take over all the other social sciences." (quoted in Tullock 2004, p. 3) Gordon Tullock made it popular through a brief essay at the beginning of the 1970s. In that essay, he praised the economic approach while making a plea for blurring the disciplinary boundaries among social sciences. (Tullock 2004)

Two decades ago, Sonja Amadae and Bruce Bueno de Mesquita have disputed the "economic imperialism" thesis in the case of Positive Political Theory in three different ways. (Amadae and Mesquita 1999) First, this idea is premised on the assumption that Rational Choice Theory was fully articulated within Economics and then "colonized" other fields, including Political Science. However, as seen, such a historical reconstruction is simply untrue, especially in the case of Game Theory.[13] Second, to assume the existence of "economic imperialism" displaces the credit for innovation from political scientists to economists. Again, this is not a correct factual reconstruction. As seen previously, political scientists have made contributions to formal developments (think, for instance, to the literature concerning voting or such Cooperative GT solutions like the "Competitive Solution")[14]. Finally, Amadae and Mesquita claimed that: "[...] the economics imperialism scenario ignores that both economists and political scientists have had to reconsider their subject areas as market phenomena are increasingly seen to be interlaced with non-market 'externalities,' and 'political economy' is taken to be a single unit of study which entails recognizing the unification of politics within economics." (Amadae and Mesquita 1999, pp. 289–90)

It is important to note that even Ordeshook shared a view similar to the latter. He argued that the development of "positive political theory," or "political economy" was nothing more than the "natural evolution of a paradigm that had previously integrated both disciplines but that economists refined in the first part of this century after shedding many of the encumbrances reality places on theorizing." (Peter C. Ordeshook 1990, p. 10) Accordingly, the re-emerging field of political economy represented the "reintegration into a refined paradigm of those features of reality that economists discarded in order to facilitate theorizing." (ibidem)

The points above focus on the issue of "economic imperialism" from a historical point of view. Thus, these effectively point out that the intellectual activities of Riker and "Positive Political Theory" cannot simply be eschewed as an example of the imperialistic attitude of economics. So, then, historically, things are far more complicated than usually assumed.

"Economic imperialism" has also been a matter of philosophical inquiry. Looking at this issue from a Philosophy of Science perspective is, in my view, necessary because, if it is true that Riker and early political game theorists could not be

13 As they rightly pointed out, "[...] 'Rational choice,' denoting conscious decision making in a strategic environment with rational competitors, as originally articulated by von Neumann & Morgenstern (1944) became the status quo within political science before economists fully grasped its merits for their field." (Amadae and Mesquita 1999, p. 290)

14 See the previous chapter

significantly influenced by economics since Game Theory in the 1950s and 1960s occupied still a marginal place in the latter discipline, it is equally correct that "Positive Political Theory" and "Economics" became very close. Moreover, this closeness is still maintained today. Then "Economic Imperialism" is (at least) twofold. First, it refers to economists using their tools to address non-traditional topics, such as politics, law, sociology, crime, etc. Second, it encompasses an economic-like formal approach to producing entirely new theories. The difference is apparent if you think, for instance, of the spatial analysis of Voting in Political Science and Gary Becker's famous economic analysis of discrimination.

Becker was influenced by Milton Friedman's positive methodology, dominant in the so-called "Chicago School" until the 1970s.[15] He used Price Theory to assume that each individual has a taste for discrimination and that someone could experience some disutility whenever he was close to a member of another ethnic group. Thus, the preference for discrimination could be represented by a "discrimination coefficient" added to the traditional capital and labor costs. (Fleury 2012, 11 et ss. Lazear 2000) In doing this, he related individual discrimination to market discrimination and presented a market solution to the problem. Indeed, if practicing discrimination is a cost for firms and individuals, then market competition could erase it by the survival of the most competitive firms, namely those with lower costs. Note that Becker did not produce a new theory to explain discrimination, but he just employed what seemed to him a potent general tool, Price Theory, in order to provide new insights into the phenomenon. The same can be said about many works in the Public Choice literature or about Mancur Olson's classical analysis of collective choice. (Olson 1965; Peter C. Ordeshook 1990)

Take the Spatial Theory of Voting instead. While it is true that its development would have been impossible without adopting the same approach of mathematical economic theory, developing the theory required more than a passive embrace of analytical tools. If Downs' most famous result was quite intuitive (although it was influenced by the less obvious result of Hotelling), Black's analysis, although far from being exceedingly advanced from the mathematical point of view, required a sharp mathematical characterization of specific political notions, like the assumption of "single-peakedness." Things got even more complicated when these results were extended to n-dimensional spaces, like in the highly mathematical works of McKelvey, Schofield, Ordeshook, Davis, Hinich, and others (some briefly reviewed in the previous chapters). These authors did not limit themselves to using some tools to enhance their understanding of a phenomenon but built from scratch entirely new theories that would have been impossible without adopting the same highly mathematical reasoning commonplace in economics, but that still required the "invention" of mathematical counterparts of political notions about voting.

To fully appraise this point, let us look at the representation of preferences in microeconomics. First, a commodity space, which corresponds to a Euclidean co-

15 On Friedman's methodology see: M. Friedman 1954; but also: Caldwell 1994. See also the fourth chapter of this dissertation. Becker addressed the issue of discrimination in his Ph.D. Dissertation, later published in 1957 with the title *The Economics of Discrimination*. He was one of the most prominent price theorists of the 'Chicago School,' with Milton Friedman and George Stigler, and was awarded the Nobel Prize in Economics in 1992. See: Spencer and Macpherson 2014, pp. 189–209. For a historical and critical appraisal of the "Chicago School," see: Horn, Mirowski, and Stapleford 2011

ordinate system, R^n_{++} is assumed.[16] Then, each point represents an item subjected to trade, and a utility function can condense the idea of individual action. Indifference curves and preference sets represent preferences for these items. Each economic agent faces a maximization problem, which mathematically means finding those quantities and prices of each item that maximize each individual's utility function. Graphically, this can be represented by that indifference curve tangent to the budget set. This so-called "consumer problem" is an optimization problem whose extension to a virtually infinite number of goods and agents represents an economy's "General Equilibrium." The development of economic theory then showed how the conditions of existence of this equilibrium could be general, i.e., what happens when some mathematical feature of the utility function, the budget set, or the goods traded changes.

The most common cause of Consumer Choice assumes that both the budget set and the indifference curves are convex. These properties display two intuitive ideas: a mix of different goods is preferable to the same quantity of only one good, and, at constant prices, the greater bundle of goods is always preferred to the lesser (for instance, a bundle comprising four apples and four pears is always preferred to a bundle containing two apples and two pears). A particular case entails satiable preferences, that is, preferences where the second condition above holds only up to a particular point. This property is also crucial in the formal analysis of politics since each rational voter's utility function is the distance between her preferred outcome and the proposal to be voted.

Microeconomics encompasses each agent determining her consumption or production of goods and services in a decentralized way. Similarly, the Spatial Voting theory is about choosing some point in the feasible space, following some majoritarian rule. This, however, does not entail that the latter was merely an offspring of the former. The following assessment by Ordeshook is worth quoting in full:

> "The use of single-peaked preferences, or the more general conceptualization of convex preferences sets with internal satiations points, contributes importantly to the reintegration of the fields of political science and economics. [...] With respect to [...] the presumed imperialism of economics [...] many economists regarded the notion of spatial preferences with internal satiation points as merely a peculiar special case. [...] However, with the derivation of such preferences from neoclassical assumptions, we now see that *such preferences are not merely a special case but that they follow from what distinguishes political institutions from decentralized markets.* Hence, because what substantively distinguishes economics from politics is reflected in the formal representation of preferences, this distinction becomes part of the paradigm and can be manipulated and recombined by anyone operating within the paradigm." (Peter C. Ordeshook 1990, p. 20. Emphasis added)

The notion of disciplinary imperialism has, of course, been investigated more deeply by philosophers of science. While Ordeshook's discourse may already seem explicatory enough, it may thus be worthwhile to connect the previous twofold distinction to the taxonomy advanced by the Finnish philosopher of science, Uskali Mäki. (Mäki 2009) He distinguishes between "expansionism" and "imperialism." The earlier refers to the expansion of the scope of a theory,

16 R^n_{++} means that only the strictly positive orthant in the euclidean space is assumed as commodity space. This is an intuitive property (the commodities to be traded are assumed to exist, that is, to have positive value), but it is not general.

namely the facts that a theory can explain. "Imperialism" instead is a subset of "expansion." "Economics Imperialism," therefore, is so defined: "[It] is a form of economics expansionism where the new types of explanandum phenomena are located in territories that are occupied by disciplines other than economics." (Mäki 2009, p. 360) In his words, the main difference between the two is "based on *historical and social contingency*: in one case, there were, in the other, there were not, established disciplines addressing the phenomena that are later added to the expanding scope of the expansionist discipline. The very idea of imperialism presupposes that of boundaries: economics imperialism is a matter of crossing disciplinary boundaries. From this perspective, the difference has a pragmatic character: it is defined in terms of the (existence or non-existence of the) practices of the conquered or would-be conquered disciplines and the relations between the practices in the conquering and conquered disciplines" (Mäki 2009, p. 361. Italics in the text)

The main reason behind "imperialism" in sciences lies in providing a unified explanation of phenomena. Therefore, despite accepting "unification" as a "virtuous achievement of scientific theorizing," Mäki proposes three kinds of constraints. For the purpose of these pages, the most interesting is what the author defines as the "ontological constraint," in turn divided into: "derivational unification" and "ontological unification." In a nutshell, the first refers to the ability to explain many phenomena from a parsimonious set of theories, while the second "is a matter of redescribing large classes of apparently independent explanandum phenomena as forms or manifestations of a common system of entities, causes, and mechanisms." (Mäki 2009, p. 364)

Notice that these notions are not easy to operationalize because it is not straightforward to find theories that fit perfectly in one category or another. Besides, even the actual difference between them is slightly fuzzy. However, in my view, one could interpret "derivational unification" as the attempt to devise a kind of 'explain-everything' theory, as it occurs in the most simplified versions of rational choice theory and even price theory. Then, assuming people behave as if they always maximize, the theorist can develop a wide-range explanation of different social phenomena. Of course, this explanation does not necessarily contrast other explanatory theories, but it could display a greater appeal since it refers to clear and circumscribed initial assumptions. For example, this is the case of Becker's theory concerning the taste for discrimination. On the contrary, extending the Theory of Games and related approaches that are now customarily in Microeconomics to other social phenomena, as it has been done in the analysis of Voting or of Rational-Choice models of political institutions, means assuming that a degree of unity among the world phenomena is possible and that the task of theorizing is precisely to make it possible to represent such unity. (see Mäki 2009, 364 et ss.) Mäki associates this position with a realist view of science. According to him, "economics imperialism" is unjustified as an approach only in those cases where there is "derivational unification" but not "ontological unification." Thus, he does not oppose it on matters of principle, provided at least that Economics does not present itself "hegemonically as being in possession of superior theories and methods, thereby excluding rival theories and approaches from consideration." (Mäki 2009, p. 374)[17]

17 Mäki refers to this situation as 'Economics Imperialism*(starred)'

Looking at the difference between Price Theory' and Microeconomics can better clarify the point. It is often implied that these subsume one another because both adopt the hypothesis of rational choice and supply & demand analysis. Still, there are differences, as it is clearly outlined in the most recent version of the somewhat classical "Chicago Price Theory" textbook. (Jaffe et al. 2019) There, it is argued that:

> "In emphasizing markets and competition, price theory is different from microeconomics. Both typically begin with the consumer or household, but price theory stresses how consumers react to prices, many times without reference to utility or even 'rationality;' whereas microeconomics takes care to lay down an axiomatic foundation of the utility function and individual demand functions. Price theory then quickly gets to market equilibrium, treating related subjects such as compensating differences, tax incidence, and price controls.
>
> Microeconomics makes more intensive use of game theory, which traditionally puts somewhat more emphasis on rationality and optimizing agents. Both price and game theory model behavior as an equilibrium, but the latter typically focuses on interactions among small numbers of agents and strives to make separate predictions for each one. The rest of the market is treated as a constant. [...] with its emphasis on competitive market equilibrium, basic price theory is not concerned with bid prices but rather the ultimate transaction price, aggregate quantities produced and sold, and how they are connected with costs of various kinds, as well as how the good is situated in the consumer demand system.(Jaffe et al. 2019, pp. 2–3)

If this reconstruction is correct, then Positive Political Theory, differing as it does from "Price Theory," but being closer to, and in part, subsumed into, Microeconomics, would not be, in Maki's terminology, an unjustified example of "economics imperialism." Such a statement could be corroborated by the fact that by using Game Theory and other formal tools in Voting theory or in studying political coalitions and institutions, theorists such as Ordeshook or McKelvey did not invade a disciplinary field but provided an entirely new class of analyses and results that would have been impossible to reach otherwise. At the same time, however, following what was said in the previous section, Riker's faith in the predictive power of the "economics-like" approach, starting with his linking equilibrium and useful predictions, could be easily interpreted as a form of unjustified "economics imperialism," at least in Mäki's characterization.

A IDEOLOGICAL CONSERVATIVE BIAS?

A FINAL POINT that deserves to be explored is that of the possible existence of a "conservative and free-market bias" in Positive Political Theory. In some works (e.g., Mirowski 2002; Amadae 2003; Erickson et al. 2015), the development of Mathematical Economics, Game Theory, and Rational Choice Theory in the Postwar years is deeply linked with the political issues concerning the Cold War and therefore the defense of a free-market economy against the perils of socialism and Marxism. This argument parallels and sometimes overlaps the so-called "economic imperialism" thesis.

Above, I argued that it is not correct to assume that the entry of Game Theory into Political Science was a form of economic imperialism from the historical point of view. Things are more complicated from the point of view of the philosophy

of science. There, one could still claim that Riker's intellectual enterprise was an unjustified mode of economic imperialism but that the same does not necessarily hold in the case of "positive political theory."

However, this argument does not exhaust the issue of what sustained Riker's commitment to formal analysis. Despite differences, as mentioned earlier, Riker's commitment to formal theory and the Theory of Games followed similar paths to that of Mathematical Economists in the 1950s. Indeed, practical aspirations fueled research in General Economic Equilibrium models, Econometrics, or Linear Programming: such advanced analysis could have been helpful to applied scopes (not necessarily coincidental with a radical program for a free-market society).[18] Similarly, Riker's main aim was to advance the scientific understanding of Politics, to utter true sentences about political issues. He found that game and economic theories were sound and valuable and employed them. His agenda in the 1950s was not political but rather methodological.

In the 1970s, Riker's own political ideology evolved toward the political and philosophical ideas that animated people like Milton Friedman, George Stigler, and Gary Becker. He thus embraced a solid free-market attitude.[19] Besides, as seen, Riker made explicit from the late 1970s onward the view that the inevitability of majority cycles and the emptiness of such concepts like people's will supported the absolute superiority of liberal democracy (social choice) over a populist one. (Riker 1982) Resting, as they did, on the mathematical analysis of Social Choice, these conclusions were subsumed in such works as Arrow's in the early 1950s. However, it is open to debate whether it was the defense of the liberal democracy against the radical one (or the protection of the market system against collectivist planning) which mainly sustained their development.

Perhaps a pivotal role in Riker's moving toward libertarian positions was the presence, at Rochester, although for a brief period, of the renowned legal scholar Henry G. Manne. Trained at the University of Chicago Law School, Manne was a critical figure in developing Law & Economics as an established field of research. Besides, he was a committed libertarian, influenced by his Chicago experience and the reading of the works of the Austrian economist Ludwig von Mises and UCLA economist Armin Alchian. (Gindis 2020; Manne and Stocker 2012)[20] While he was not an economist, Manne took economic theory very seriously because he was persuaded of the impossibility of understanding legal phenomena without analyzing the market forces shaping the actors' incentives. Besides, he did not

18 Take, for instance, the case of the Cowles Commission at Chicago in the 1950s. Its members were mostly European and with left-wing preferences. Such a radical attitude often put the researchers who worked there in contrast with the faculty of the Department of Economics, which hosted the Commission, and whose attitude toward mathematical economics was less enthusiastic than that of Cowles affiliates. Hibbard n.d.

19 In his interview with Shepsle, he stated that "I have gone 180 degrees in what I think the appropriate reformist position is."(Riker and K. Shepsle 1979, p. 145)

20 Aaron Director was the pivotal figure in establishing "Law and Economics" in Chicago, in particular, through the introductory Price Theory course he taught at Chicago Law School in the late 1940s. After that, the transition from having an economist teaching at Law School to establishing an entire field of research was relatively fast. In the late 1940s and the 1950s, Director was more and more involved in his teaching and research activities with law scholars, applying price theory to address traditional legal and public policy issues like antitrust. (Steven G. Medema 2009) Armen Alchian was mainly renowned for his economic theory of property rights. (Alchian 1965; Alchian and W. R. Allen 2018) Ludwig von Mises was a committed classical liberal scholar whose view of economics, which he defined as 'praxeology,' disputed both Socialism and Neo-classical economic theory. (Caplan 1999)

limit his interest to theory but was well active in the economic training of lawyers, judges, and law professors.

Manne arrived at Rochester in 1968, hired by W. Allen Wallis, himself a former faculty of the University of Chicago, very close to Friedman and Stigler, and a member of the Mont Pèlerin Society. He was tasked to set up a Law School and, in the meantime, temporarily joined the Political Science department. Still, his involvement in the department activities was negligible (Bueno de Mesquita 2021). The plan of establishing a Law School eventually faded away, and Manne moved to Miami. However, it may not be a mere coincidence that in the 1970s, Riker started to collaborate even more intensively with the Liberty Fund and joined conferences and activities, often organized by the "Law and Economics Center," founded by Manne at the University of Miami.[21]

In his interview with Shepsle, Riker returned to the issue of an "inherent conservative bias" in Positive Political Theory. (Riker and K. Shepsle 1979, 143 et ss.) He then delineated a somewhat ideal-type explanation of why people get attracted to social science and why formal Political Theory, among all the different areas of Political Science, is more inclined to show a rather conservative position. "Most people, I think, enter Political Science, as indeed all of the social sciences, as undergraduates because they have moral concerns. And they want to make the world a little better in some way or another. And as they get attracted to science itself, why then [sic] they tend to lose interest in the reform enterprise that attracted them in the first place." (Riker and K. Shepsle 1979, p. 144) These words fit perfectly with Riker's epistemological position (as exposed above), namely his progressive vision of science and the theoretical attitude he strived for when he started using GT. As he continued, "I think that the notion that political theorists are conservative is simply an accident that they are not interested in reform because they become interested in theory." (Riker and K. Shepsle 1979, p. 145) He strongly rebuffed the idea of an inherent conservative bias, putting aside his political ideas.

Perhaps the main reason for this allegation too often levied against the formal approach in Political Science is Buchanan's Public Choice with its fairly classical liberal commitment. Previously, I showed how Riker's activities for establishing Positive Political Theory in the 1960s (and later) often overlapped with Public Choice. However, there were some differences between the two approaches. One of the most significant was exactly that Public Choice was more politically committed than positive political theory. As apparent by Buchanan and Tullock's first papers, before their joint 1962 text and as reconstructed masterfully by such historical works as Levy and Peart 2020 they, alongside scholars like Warren Nutter and Rutledge Vining, were motivated by growing concerns regarding the proper role of social sciences and government in the 1950s. Accordingly, in many scholars' eyes, Public Choice came to champion a libertarian attitude.[22]

It is true that the Rational Choice approach to studying political institutions fuels similar aspirations, at least partially. However, as the general case of eco-

21 A detailed list of these activities is contained in Gindis 2020. The "Liberty Fund2 is an American think tank founded in 1960 by businessman Pierre F. Goodrich. Its focus is on spreading conservative and libertarian ideas and educational policies.

22 Note, however, that it is for sure simplistic and imprecise to imply that Public Choice is ideologically biased. More in general, it can be seen as a "set of theories" of governmental failures (James M. Buchanan 2003), but this does not imply that every Public Choice Scholar aims to reduce the size of State activity as small as possible.

nomics shows, this is not enough to validate the view that a discipline resting on a mathematical representation of individual preferences and choices is inherently politically conservative.

CONCLUDING REMARKS: WHAT THIS DISSERTATION HAS TRIED TO ACCOMPLISH

Three years before his death in 1993, Riker took part in the Conference organized by E. Roy Weintraub at Duke, the first specifically devoted to the history of Game Theory. There, Riker presented a paper on the entry of the theory of games in Political Science, which was also a bit autobiographical given his role in such development.(E. Roy Weintraub 1992; Riker 1992) Despite his later turn toward studying institutions and "herestetics," the research program he had established starting in the 1960s had flowered successfully, becoming one of the most original and engaging subfields of political science in the 1980s and 1990s.

When Riker started his theoretical enterprise, American Political Science was entering a period of ferment and disciplinary transformation. In his view, Economics could offer a viable model for establishing a "genuine science of politics." Eventually, however, Political Science did not reach unity from the methodological point of view, like, say, Mathematical Economics did. On the contrary, at the end of the 1980s, Stanford professor Gabriel Almond famously talked about separate tables in Political Science, and likewise did Cornell professor Theodor J. Lowi in 1992. (Gabriel A. Almond 1988; Lowi 1992) Having introduced a double cleavage to conceptualize the actual state of the discipline, one methodological (soft/hard) and one ideological (left/right), Almond gave the following unpleasant picture of the whole field:

> "Now there is uneasy separateness. The public choice people seek an anchorage in reality, a "new institutionalism," to house their powerful deductive apparatus; the political econometricians want to relate to historical and institutional processes; the humanists cringe at the avoidance of political values by "scientism," and suffer from feelings of inadequacy in a world dominated by statistics and technology; and the radical and "critical" political theorists, like the ancient prophets, lay about them with anathemas against the behaviorists and positivists, and the very notion of a political science professionalism that would separate knowledge from action. But their anti-professionalism must leave them in doubt as to whether they are scholars or politicians." (Gabriel A. Almond 1988, pp. 827–8)

Of course, many things have changed again in Political Science in the thirty years since these words were written. Yet they and the following transformation prove that Riker's pretense that his approach could be the "main hope for a genuine science of politics" has not been fulfilled. Even more significantly, it has not been discarded on theoretical grounds but rather on, so to speak, "empirical," at least since the number of scientific papers using game theory in the leading political science journal peaked in the 1990s. After that, the enthusiasm for formal political theory froze out. (Lohmann 2008)

In a widely discussed book published in 1994, two political scientists, Donald P. Green and Ian Shapiro, questioned how Rational Choice Theories conformed to those empirical phenomena they pretended to explain. (Green and Shapiro 1994). Famously, they argued that one of the most troubling issues with rational

choice theories lies in their being "method-driven" rather than "theory-driven." Namely, the most important advancements in this approach were obtained by the progressive refinement of technical (i.e., mathematical) tools more than by the emerging of real-world problems. Their criticism was openly based on the assumption that what is interesting from the mathematical point of view is not necessarily important for providing a useful explanation of actual phenomena.

Nevertheless, even before Green & Shapiro's criticism, the issue of incorporating different degrees of knowledge and errors into game theoretical models to make them amenable to empirical validation was somewhat a "hot topic" among formal theorists. In McKelvey's eyes, the problem was that of matching Nash Equilibria with experimental data obtained through laboratory experiments. The result was a "statistical facelift to traditional non-cooperative game theory," labeled as "Quantal Response Equilibrium." (T. Palfrey 2005, p. 16; McKelvey and T. R. Palfrey 1995)McKelvey and his coauthors were not responding directly to Green & Shapiro. Instead, their concerns originated from long-lasting (and still open) problems in game theory, like the actual playing of Nash Equilibrium strategies in simple experimental settings. The highly technical idea of "Quantal Response Equilibrium" was an attempt to overcome these difficulties. It has since been used in Political Science, Economics, and Econometrics and lies at the boundaries of these disciplines.

After Green & Shapiro's book, other formal theorists called for more empirical relevance, although not necessarily endorsing the full criticism against the use of rational choice theory in political science. In many aspects, these replies followed the blaze trailed by Riker with his Rational-Choice institutionalist research program in the 1980s. However, a few differences existed. For instance, Ordeshook emphasized the "engineering" approach in studying institutions, namely the possibility of using GT and formal analysis not to predict outcomes but instead for institutional design. (Peter C. Ordeshook 1996) This solution aimed to encompass the long-lasting positive vs. normative debate, whereas normativism was simultaneously strengthened by the powerful tools of GT and limited in its scope. The issue was no more what is the best institution or polity but how we could improve the existing ones. This, it should be noticed, was not distant from the accomplishments of Baron & Ferejohn's model when they related "more universalistic" outcomes to "open rule committees." [23] Indeed, it is implicit in their model that an open rule committee favors the bargaining power of each member and, therefore, may reduce internal conflict. In a different fashion, but with a similar aim, Norman Schofield called for a "theory of rationality based on both preference and belief," which he labeled the "Condorcetian Research Program." This with the express goal of overrunning the weaknesses of a "pure" preference-based approach. (Schofield 1996)

To sum up, it could hardly be said that, thirty years after Riker's death and sixty years after his work on political coalitions, Game Theory and, more generally, the formal approach occupy the central place in contemporary Political Science. Nevertheless, "Positive Political Theory" does represent a definite and well-established subfield of the discipline, even if its boundaries with economic theory seem destined to blur further.[24]

23 For a discussion, see the sixth chapter of this dissertation

24 As seen, according to Ordeshook, the re-emerging field of political economy represented the reprise into Economics of those features of reality that economists discarded in order to facilitate theorizing." (Peter C. Ordeshook 1990, p. 10) Besides, Riker wrote: "the main practical benefit

Yet, the goal of my research was not to address the place of formal political science in the current state of the discipline but rather to show and discuss, both from an internalist and an externalist perspective, how political scientists started to use game theory. I gave attention to three main aspects: first, the role of Riker within this process as a formal theorist; second, his role as an "intellectual entrepreneur," or "charismatic leader," of an intellectual community. Finally, my research aimed at reconstructing an often neglected page of the history of game theory, showing the cross-fertilization between economics and politics.

I start with the latter point. Cross-fertilization is a two-sided process. Usually, when the economic analysis is employed across different domains, this is interpreted as an "act of conquest." In this process, social scientists other than economists can display a two-fold attitude: resist the invasion or be reduced to background actors, sometimes mocking economists' theory. I showed instead that the peculiarity of Riker's approach lies in the fact that he took Game Theory exceptionally seriously. He advanced an entire research program, which, in his eyes, coincided with the scientific development of political science as a whole. Especially in the 1970s, Political Game Theory paralleled and sometimes was even more animated than game theoretical research in Economics, at least with respect to specific issues and approaches, like Cooperative games. Eventually, Positive Political Theory joined the game-theoretical revolution, much as it happened in Economics: Cooperative models were marginalized, and Non-cooperative theory came to prominence. However, its path toward this outcome started well before Economics, and Riker's theory of political coalitions represented a cornerstone of this process.

I also showed that Riker lacked the necessary mastering of Game Theory to fulfill his extremely ambitious theoretical aims. This is the issue of Riker as a formal theorist. His use of GT was somewhat defective and imprecise, and I advanced several explanations for this. An important one encompasses biographical circumstances. Riker received an undergraduate major in Economics, but in a period (the late 1930s-early 1940s) when such training was completely different from what is now, namely when minor if not any mathematical or statistical training was required. The same holds for his graduate training in Political Science. Consequently, his early analyses were too often poorly formalized and rarely caught the interest of mathematical game theorists.

Yet, this explanation is far from complete. A more comprehensive answer is needed to complement the first one, an answer grounded on some epistemological assumptions, like the difference between alternative views of formalism in Economics. As I stated, Riker did not entirely recognize Mathematical Economics and Game Theory's innovative character with respect to interwar Economics. Besides, he adopted a "hyper-positivist" view of science, where there was apparently little room for the idea that a model could be just a "caricature" (Gibbard and Varian 1978) or an "idealization" (Reiss 2013) of reality, and where rationality was a "substantial" theory rather than just a "formal" one, as instead, it has become customary in postwar Economics according to its most extreme mathematical version.

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of rational choice theory to political science is that it has opened the door to political economy as a part of political science." (Riker 1990, p. 180) The main source about Political Economy and Economics is for sure the textbook by Persson & Tabellini, Persson and Tabellini 2002).

Apparently, Riker looked for robust predictive power in game theory as the basis for developing a genuinely empirical and truly scientific political science. Furthermore, he looked for such power in that part of game theory, namely von Neumann & Morgenstern's cooperative game theory, which was the least mathematically "able" to advance predictions due to its being grounded on solution concepts that comprised infinitely many elements or even no solutions at all. Unfortunately, as a massive amount of literature shows, it is challenging to say what the predictive power of game theory actually is. Philosophers of science and economists have dealt extensively with such issues as what is a model and what is the explanatory power of a model. Still, no unified view exists. Nonetheless, a somehow shared view among economists and formal social theorists is that no actual prediction can be obtained through game-theoretic models, as well as via formal analysis like rational choice theory, at least not in the trivial sense of the term 'prediction.'

Besides, like other formal approaches, Game Theory exhibits a peculiar feature. Martin Shubik, who worked extensively in the field (as seen), outlined a three-fold partition: "high church" game theory, "low-church" game theory, and finally, "conversational game theory." (Shubik 2002) To the first group belong the highly mathematical works which introduced new concepts or ideas (say, for instance, the results of people like Nash, Shapley, and Aumann). The vast part of economics using Game Theory belongs to the second group. Finally, as the name suggests, conversational game theory encompasses the most straightforward and widespread discussion about game theory (i.e., the adoption of a "game-theoretic" mindset in actual problems without resting on any formal analysis). The curious feature is that, if any at all and broadly, the notion of predictive power fits better with the last group rather than with the previous two. Indeed, finding situations that can be effectively addressed with a game-theoretic mindset is straightforward. However, in these cases, finding alternative explanations for the same situations is equally simple since the perimeter of what is to be explained is extremely vague or poorly defined. On the contrary, building a rigorous model belonging to the other two prongs allows for a more precise characterization of the problem, but at the expense of any possibility of actual predictions.

In my view, the most convincing, though not unique, way, to live with this puzzle (note to the reader: to live with but not to solve) has been provided by Ariel Rubinstein. To him, Game Theory is primarily the study of considerations used in decision-making in interactive situations, with no normative implications and very little empirical significance. (Rubinstein 2007) In Rubinstein's words, "[g]ame theory is viewed as a cousin of logic. Logic does not allow us to screen out true statements from false ones and does not help us distinguish right from wrong. Game theory does not tell us which action is preferable or predict what other people will do. If game theory is nevertheless useful or practical, it is only indirectly so. In any case, the burden of proof is on those who use game theory to make policy recommendations, not on those who doubt the practical value of game theory in the first place." (Rubinstein 2007, p. 634) In a broad sense, the Theory of Games is, more than anything else, a language, like mathematics.

Encompassing this new language within the toolbox of political scientists is the true landmark contribution of William H. Riker, even though he failed on his grander scheme of exploiting Game Theory to turn Political Science into genuinely empirical science. Nevertheless, such a conclusion should not delegitimize the theoretical efforts by Riker (and others like him) to adopt Game Theory in

their respective disciplinary fields. Indeed, the issues that initially puzzled him and later fueled his commitment to formal analysis lie at the heart of any attempt to look for a science of politics.

BIBLIOGRAPHY

Adcock, Robert (2009). "Interpreting Behavioralism". In: *Modern Political Science: Anglo-American exchanges since 1880*. Ed. by Robert Adcock, Mark Bevir, and Shannon C Stimson. Princeton University Press Princeton, NJ, pp. 180–208.

Adcock, Robert and Mark Bevir (2006). "Political Science since World War Two: Americanization and its Limits". In: *The American Political Science Review* 100, pp. 597–604.

Advanced Studies in the Behavioral Sciences, Center for the (1959). "Plans for continuation of the Center". Box 10, WHRP.

Alchian, Armen A. (1965). "Some Economics of Property Rights". In: *Il Politico* 30.4, pp. 816–829.

Alchian, Armen A. and William R. Allen (2018). *Universal Economics*. Liberty Fund.

Almond, Gabriel A (1966). "Political theory and political science". In: *The American Political Science Review* 60.4, pp. 869–879.

– (1988). "Separate Tables: Schools and Sects in Political Science". In: *Political Science and Politics* 21.4, pp. 828–842.

Amadae, Sonja M. (2003). *Rationalizing capitalist democracy: The cold war origins of rational choice liberalism*. University of Chicago Press, Chicago, IL.

Amadae, Sonja M. and Bruce Bueno de Mesquita (1999). "The Rochester School: The origins of positive political theory". In: *Annual Review of Political Science* 2.1, pp. 269–295.

Arrow, Kenneth J (1983). "Cowles in the history of economic thought". In: *Cowles Fiftieth Anniversary Volume*. Ed. by Alvin K Klevorick. Yale University Press, New Haven, CT.

– (1991). "Jacob Marschak (1890-1977) A Biographical Memoir". In: *National Academy of Sciences. Biographical Memoirs*.

– (1951a). "Mathematical Models in the Social Sciences". In: *The Policy Sciences. Recent Developments in Scope and Method*. Ed. by Harold D. Lasswell and Daniel Lerner. Stanford University Press, Stanford, CA, pp. 507–32.

– (1951b). *Social choice and individual values*. 1962nd ed. John Wiley & Sons, New York, NY.

– (2014). "The Origins of the Impossibility Theorem". In: *The Arrow Impossibility Theorem*. Ed. by Eric Maskin and Amartya Sen. Columbia University Press, New York, NY.

Arrow, Kenneth J. and Gerard Debreu (1954). "Existence of an equilibrium for a competitive economy". In: *Econometrica: Journal of the Econometric Society* 22.3, pp. 265–90.

Aumann, Robert J. and Michael Maschler (1964). "The Bargaining Set for Cooperative Games". In: *Advances in Game Theory.* Ed. by Michael Dresher, Lloyd S. Shapley, and Albert W. Tucker. Vol. 52. Princeton University Press, Princeton, NJ.

Austen-Smith, David (2006). "Economic Methods in Positive Political Theory". In: *The Oxford Handbook of Political Economy*. Ed. by Barry R. Weingast and Donald A. Wittman. Oxford University Press, Oxford, pp. 899–914.

Austen-Smith, David and Jeffrey S. Banks (1999). *Positive Political Theory I*. University of Michigan Press, Ann Arbor, MI.
Backhouse, Roger E (2017). *Founder of Modern Economics: Paul A. Samuelson: Volume 1: Becoming Samuelson, 1915-1948*. Oxford University Press, Oxford.
Backhouse, Roger E and Steven G Medema (2009). "Robbins's Essay and the Axiomatization of Economics". In: *Journal of the History of Economic Thought* 31.4, pp. 485–499.
Baer, Michael A, Malcolm E Jewell, and Lee Sigelman (1991). *Political science in America: Oral histories of a discipline*. University Press of Kentucky, Lexington, KY.
Baron, David P. and John A. Ferejohn (1989). "Bargaining in Legislatures". In: *The American Political Science Review* 83.4, pp. 1181–1206.
Barry, Brian (1970). *Sociologists, Economists and Democracy*. University of Chicago Press, Chicago, IL.
Bergson, Abram (1938). "A Reformulation of Certain Aspects of Welfare Economics". In: *The Quarterly Journal of Economics* 52.2, pp. 310–34.
Bernd, Joseph L., John M. Claunch, and James F. Herndon, eds. (1964-73). *Mathematical Applications in Political Science*. Vol. I-VII. Southern Methodist University Press-The University of Virginia Press.
Bernheim, B. Douglas (1984). "Rationalizable Strategic Behavior". In: *Econometrica: Journal of the Econometric Society* 52.4, pp. 1007–28.
Binmore, Ken (2015). "Rationality". In: *Handbook of Game Theory vol. 4*. Ed. by H. Peyton Young and Zamir Shmuel. Elsevier Science Publishers BV, Amsterdam, pp. 2–26.
Binmore, Ken and Partha Dasgupta, eds. (1987). *The Economics of Bargaining*. Basil Blackwell, Oxford.
Bjerkholt, Olav (2015). "How it all began: the first Econometric Society meeting, Lausanne, September 1931". In: *The European Journal of the History of Economic Thought* 22.6, pp. 1149–1178.
Black, Duncan (1948a). "The decisions of a committee using a special majority". In: *Econometrica: Journal of the Econometric Society* 16.3, pp. 245–61.
– (1948b). "The elasticity of committee decisions with an altering size of majority". In: *Econometrica: Journal of the Econometric Society* 16.3, pp. 262–70.
– (1948c). "Un approccio alla teoria delle decisioni di comitato". In: *Giornale degli Economisti* 7.5-6, pp. 262–284.
– (1950). "The unity of political and economic science". In: *The Economic Journal* 60.239, pp. 506–514.
– (1958). *The theory of committees and elections*. Cambridge University Press, Cambridge.
– (1969). "On Arrow's Impossibility Theorem". In: *The Journal of Law & Economics* 12.2, pp. 227–248.
Blaug, Mark (1997). *Economic theory in retrospect*. Cambridge University Press, Cambridge.
– (2003). "The formalist revolution of the 1950s". In: *Journal of the History of Economic Thought* 25.2, pp. 145–156.
Boettke, Peter J. (2019). "The Allure and Tragedy of Ideological Blinders Left, Right and Center: A Review Essay of Nancy MacLean's Democracy in Chains". In: *Research in the History of Economic Thought and Methodology* 37B, pp. 123–147.
Boulding, Kenneth E. (1958). "Theoretical Systems and Political Realities". In: *The Journal of Conflict Resolution* 2.4, pp. 329–334.

Brady, Gordon L. and Gordon Tullock, eds. (1996). *Formal Contributions to the Theory of Public Choice. The Unpublished Works of Duncan Black*. Kluwer Academic Publishers.
Brams, Steven J. (1975). *Game Theory and Politics*. Dover Publications, Mineola, NY.
Buchanan, James M and Gordon Tullock (1962). *The calculus of consent: Logical foundations of constitutional democracy*. University of Michigan Press, Ann Arbor, MI.
Buchanan, James M. (1954a). "Individual Choice in Voting and the Market". In: *Journal of Political Economy* 62, pp. 334–343.
– (1954b). "Social Choice, Democracy, and Free Markets". In: *Journal of Political Economy* 62, pp. 114–23.
Public Choice: The Origins and Development of a Research Program (2003).
Bueno de Mesquita, Bruce (2021). "May 22th 2021". Interview with the author.
Butterworth, Robert L. (1971). "A Research Note on the Size of Winning Coalitions". In: *The American Political Science Review*, pp. 741–748.
Caldwell, Bruce (1994). *Beyond Positivism. Economic Methodology in the Twentieth Century [1981]*. Routledge, New York and London.
Campbell, Angus et al. (1980). *The american voter*. University of Chicago Press, Chicago, IL.
Caplan, Bryan (1999). "The Austrian Search for Realistic Foundations". In: *Southern Economic Journal* 65.4, pp. 823–838.
Christ, Carl F. (1952). "History of the Cowles Commission 1932-1952". In: *Economic Theory and Measurement*. Cowles Commission for Research in Economics Chicago, pp. 3–65.
Coase, Ronald H. (1994). *Essays on Economics and Economists*. University of Chicago Press, Chicago, IL.
Cogliano, Jonathan (2019). "An Account of "The Core" in Economic Theory". In: *Center for the History of Political Economy at Duke University Working Paper Series* 2019-17.
Coleman, James (1968). "The Marginal Utility of a Vote Commitment". In: *Public Choice* 5, pp. 39–58.
Copeland, Arthur H. (1945). "Review to The Theory of Games and Economic Behavior". In: *Bulletin of the American Mathematical Society* 51.7, pp. 498–504.
Corry, Leo (2003). *Modern algebra and the rise of mathematical structures*. Birkhauser, Basel.
Cox, Gary W. (1997). *Making Votes Count. Strategic Coordination in the World's Electoral Systems*. Cambridge University Press, Cambridge and New York.
Dahl, Robert A (1961). "The Behavioral Approach in Political Science: epitaph for a monument to a successful protest". In: *The American Political Science Review* 55.4, pp. 763–772.
– (2005). *Who governs?: Democracy and power in an American city [1962]*. Yale University Press, New Haven, CT.
Dahl, Robert A. and Charles E. Lindblom (1953). *Politics, economics and welfare: planning and politico-economic systems, resolved into basic processes*. Harper & Brothers, New York, NY.
Davis, Otto A, Melvin J Hinich, and Peter C Ordeshook (1970). "An expository development of a mathematical model of the electoral process". In: *The American Political Science Review* 64.2, pp. 426–448.

Davis, Otto A. (1968). "Notes on Strategy and Methodology for a Scientific Political Science". In: *Mathematical Applications in Political Science*. Ed. by Joseph L. Bernd. Vol. 4. University Press of Virginia, Charlottesville, VA.
Davis, Otto A., Morris H. DeGroot, and Melvin J. Hinich (1972). "Social Preference Orderings and Majority Rule". In: *Econometrica: Journal of the Econometric Society* 40.1, pp. 147–57.
Davis, Otto A. and Melvin Hinich (1966). "A Mathematical Model of Policy Formation in a Democratic Society". In: *Mathematical Applications in Political Science II*. Ed. by Joseph L. Bernd. Southern Methodist University Press, Dallas, TX, pp. 175–208.
Debreu, Gerard (1959). *Theory of value: An axiomatic analysis of economic equilibrium*. Yale University Press, New Haven, CT.
– (1983). "Mathematical economics at Cowles". In: *Cowles Fiftieth Anniversary Volume*. Ed. by Alvin K Klevorick. Yale University Press, New Haven, CT.
– (2008). "Mathematical Economics". In: *New Palgrave Dictionary of Economics*. Ed. by Steven Durlauf and Lawrence E Blume. Palgrave MacMillan, London.
Deutsch, Karl W. (1954). "Game Theory and Politics: Some Problems of Application". In: *The Canadian Journal of Economics and Political Science* 20.1, pp. 76–83.
Dimand, Robert W. (2019). "The Cowles Commission and Foundation for Research in Economics". In: *The New Palgrave Online (2019)*.
Downs, Anthony (1957). *An Economic Theory of Democracy*. Harper & Row, New York, NY.
Dryzek, John S (2006). "Revolutions without enemies: Key transformations in Political Science". In: *American Political Science Review* 100.4, pp. 487–492.
Dummett, Michael (2005). "The work and life of Robin Farquharson". In: *Social Choice and Welfare* 25.2, pp. 475–483.
Dummett, Michael and Robin Farquharson (1961). "Stability in Voting". In: *Econometrica: Journal of the Econometric Society* 29, pp. 33–42.
Düppe, Till (2010). "Debreu's apologies for Mathematical Economics after 1983". In: *Erasmus Journal for Philosophy and Economics* 3.1, pp. 1–32.
– (2012). "Gerard Debreu's Secrecy: His Life in Order and Silence". In: *History of Political Economy* 44.3, pp. 413–49.
Düppe, Till and E Roy Weintraub (2014a). *Finding equilibrium*. Princeton University Press, Princeton, NJ.
– (2014b). "Siting the new economic science: The Cowles Commission's activity analysis conference of June 1949". In: *Science in context* 27.3, pp. 453–483.
– eds. (2019). *A Contemporary Historiography of Economics*. Routledge, New York and London.
Easton, David (1951). "The decline of modern political theory". In: *The Journal of Politics* 13.1, pp. 36–58.
– (1953). *The Political System: An inquiry into the state of Political Science*. Alfred A. Knopf, New York, NY.
– (1965). *A Systems Analysis of Political Life*. John Wiley & Sons, New York, NY.
– (1969). "The New Revolution in Political Science". In: *The American Political Science Review* 63.4, pp. 1051–1061.
Easton, David, Luigi Graziano, and John Gunnell (2002). *The Development of Political Science: A comparative survey*. Routledge.

Edgeworth, Francis Y. (2008). "Mathematical methods in political economy [1909]". In: *New Palgrave Dictionary of Economics*. Ed. by Steven Durlauf and Lawrence E Blume. Palgrave MacMillan, London.
"Empirical Research currently under way in the Department of Economics" (1960). Box 32, LWMP.
Erickson, Paul (2015). *The world the game theorists made*. University of Chicago Press, Chicago, IL.
Erickson, Paul et al. (2015). *How Reason Almost Lost its mind: The strange career of Cold War Rationality*. University of Chicago Press, Chicago, IL.
Fagen, Richard R. (1961). "Some Contributions of Mathematical Reasoning to the Study of Politics". In: *The American Political Science Review* 55.4, pp. 888–900.
– (1963). "Review to The Theory of Political Coalitions". In: *The American Political Science Review* 57.2, pp. 446–7.
Farquharson, Robin (1970). *Theory of Voting*. Blackwell, Oxford.
Farr, James (2009). "The Historical Science (s) of Politics: The Principles, Association, and Fate of an American Discipline". In: *Modern Political Science: Anglo-American exchanges since 1880*. Princeton University Press, Princeton, NJ, pp. 66–96.
Filippov, Mikhail (2005). "Riker and Federalism". In: *Constitutional Political Economy* 16, pp. 93–111.
Fisher, Franklin M. (1989). "Games Economists Play: A Noncooperative View". In: *The RAND Journal of Economics* 20.1. Ed. by 20, pp. 113–24.
Fisher, Irving (1892). *Mathematical Investigations in the Theory of Value and Price*. Yale University Press, New Haven, CT.
Fleury, Jean-Baptiste (2012). "Wandering through the Borderlands of the Social Sciences: Gary Becker's Economics of Discrimination". In: *History of Political Economy* 44.1, pp. 1–40.
Fogel, Robert William et al., eds. (2013). *Political arithmetic: Simon Kuznets and the empirical tradition in economics*. University of Chicago Press, Chicago, IL.
Forget, Evelyn L and Craufurd D Goodwin (2011). "Intellectual communities in the history of economics". In: *History of Political Economy* 43.1, pp. 1–23.
Friedman, Jeffrey, ed. (1995). *The Rational Choice Controversy*. Yale University Press, New Haven, CT.
Friedman, Milton (1954). *Essays in Positive Economics*. Chicago University Press, Chicago, IL.
Fudenberg, Drew and David K. Levine (2016). "Whither Game Theory? Towards a theory of Learning in Games". In: *Journal of Economic Perspectives* 30.4, pp. 151–170.
Fudenberg, Drew and Jean Tirole (1991). *Game Theory*. MIT press, Cambridge, MS.
Garceau, Oliver (1951). "Research in the political process". In: *The American Political Science Review* 45.1, pp. 69–85.
Gaspard, Marion and Thomas M. Muller (2021). "Building comparison spaces: Harold Hotelling and mathematics for economics". In: *Journal of Economic Methodology* 28.3, pp. 255–73.
Gibbard, Allan and Hal R. Varian (1978). "Economic Models". In: *The Journal of Philosophy* 75.11, pp. 664–77.
Gibbons, Robert A. (1992). *A Primer in Game Theory*. Prentice-Hall, New York, NY.

Gindis, David (2020). *Law and Economics Under the Palms: Henry Manne at the University of Miami, 1974-1980*. URL: https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3567105.
Giocoli, Nicola (2003a). "Fixing the point: the contribution of early Game Theory to the tool-box of modern Economics". In: *Journal of Economic Methodology* 10.1, pp. 1–39.
– (2003b). *Modeling rational agents: From interwar economics to early modern game theory*. Edward Elgar Publishing, Cheltenham.
– (2009a). "Mathematics as the role model for neoclassical economics". In: MPRA Paper.33806, pp. 145–165.
– (2009b). "Three alternative (?) stories on the late 20th century rise of game theory". In: *Studi e Note di Economia* XIV.2, pp. 187–210.
Goodman, Leo and Harry Markowitz (1952). "Social Welfare Functions Based on Individual Rankings". In: *American Journal of Sociology* 58, pp. 257–62.
Green, Donald P. and Ian Shapiro (1994). *Pathologies of Rational Choice Theory. A Critique of Applications in Political Science*. Yale University Press, New Haven, CT.
Griffith, Ernest S., ed. (1948). *Research in Political Science*. University of North Carolina Press, Chapel Hill, NC.
Gunnell, John G. (1995). "The Declination of the "State"and the Origins of American Pluralism". In: *Political Science in History: Research Programs and Political Traditions*. Ed. by James Farr, John S Dryzek, and Stephen T Leonard. Cambridge university press, Cambridge and New York, pp. 19–40.
Hagemann, Harald (2011). "European émigrés and the 'Americanization'of economics". In: *The European Journal of the History of Economic Thought* 18.5, pp. 643–671.
Hayek, Friedrich A. von (1937). "Economics and knowledge". In: *Economica* 4.13, pp. 33–54.
– (1952). *The Counter-Revolution of Science: studies on the Abuse of Reason*. The Free Press, Glencoe, IL.
Heaney, Michael T and John Mark Hansen (2006). "Building the Chicago school". In: *The American Political Science Review* 100.4, pp. 589–596.
Heard, Alexander (1949). "Research on Political Behavior: Report of a Conference". In: *Items* 3, pp. 41–44.
Herfeld, Catherine (2018). "From theories of human behavior to rules of rational choice: tracing a normative turn at the Cowles Commission, 1943–54." In: *History of Political Economy* 50.1, pp. 1–48.
Herring, Pendleton (1947). "The social sciences in modern society". In: *Items* 1.1, p. 5.
Hibbard, Jane (n.d.). *Kenneth Joseph Arrow*. Stanford Oral History Program.
Hicks, John R. and Roy G.D. Allen (1934). "A reconsideration of the theory of value. Part I". In: *Economica* 1.1, pp. 52–76.
Hinich, Melvin J. and Michael Munger (2006). "In memorian: Otto "Toby" Davis, 1934-2006". In: *Public Choice* 128, pp. 357–9.
Horn, Robert van, Philip Mirowski, and Thomas A. Stapleford, eds. (2011). *Building Chicago Economics*. Cambridge University Press, Cambridge and New York.
Hotelling, Harold (1929). "Stability in Competition". In: *The Economic Journal* 39.153, pp. 41–57.
Hotz, Alfred J (1963). "Review to The Theory of Political Coalitions." In: *Midwest Journal of Political Science* 7.3, pp. 295–7.

Howson, Susan (2011). *Lionel Robbins*. Cambridge University Press, Cambridge and New York, pp. 114–136.
Hurwicz, Leonid (1945). "Review to The Theory of Games and Economic Behavior". In: *The American Economic Review* 35.5, pp. 909–25.
Hutchison, Terence W. (1938). *Significance and basic postulates of economic theory*. MacMillan and Co, London.
Igersheim, Herrade (2019). "The Death of Welfare Economics. History of a Controversy". In: *History of Political Economy* 51.5, pp. 827–865.
Ingrao, Bruna and Giorgio Israel (1987). *La Mano Invisibile. L'equilibrio economico nella storia della scienza*. Laterza, Bari.
Israel, Giorgio and Ana Millan Gasca (2009). *The World as a Mathematical Game: John von Neumann and the Twentieth Century Science*. Birkhauser, Basel.
Jaffe, Sonia et al. (2019). *Chicago Price Theory*. Princeton University Press, Princeton, NJ.
Jones, Bryan et al. (2011). "In memorian: Melvin J. Hinich, 1939-2010". In: *Public Choice* 146, pp. 1–8.
Kalisch, G. et al. (1952). *Some Experimental n-Person Games*. Tech. rep. 948. RAND-Memorandum.
Kaplan, Morton A. (1957). *Systems and Process in International Politics*. John Wiley & Sons, New York, NY.
– (1963). "The Theory of Political Coalitions". In: *The Annals of the American Academy of Political and Social Science* 347, pp. 137–8.
Kenny, Michael (2009). "Birth of a discipline: interpreting British political studies in the 1950s and 1960s". In: *Modern Political Science: Anglo-American exchanges since 1880*. Ed. by Robert Adcock, Mark Bevir, and Shannon C Stimson. Princeton University Press Princeton, NJ, pp. 158–179.
Key, V.O. (1949). *Southern Politics*. Alfred A. Knopf, New York, NY.
Kramer, Gerald H. (1966). "A Decision-Theoretic Analysis of a Problem in Political Campaigning". In: *Mathematical Applications in Political Science II*. Ed. by Joseph L. Bernd. Southern Methodist University Press, Dallas, TX, pp. 137–60.
Kreps, David (1990). *A Course in Microeconomic Theory*. Princeton University Press Princeton, NJ.
– (1991). *Game Theory and Economic Modelling*. Clarendon Press, Oxford.
Kuhn, Harold W. and Sylvia Nasar, eds. (2002). *The Essential John Nash*. Princeton University Press, Princeton, NJ.
Kuhn, Harold William and Albert William Tucker (1953). *Contributions to the Theory of Games*. Princeton University Press, Princeton, NJ.
Lakatos, Imre (1978). *The Methodology of Scientific Research Programmes. Philosophical Papers Vol. 1*. Cambridge University Press, Cambridge and New York.
Lasswell, Harold D. and Abraham Kaplan (1950). *Power and Society: A Framework for Political Inquiry*. Yale University Press, New Haven, CT.
Lasswell, Harold D. and Daniel Lerner, eds. (1951). *The Policy Sciences. Recent Developments in Scope and Method*. Stanford University Press, Stanford.
Lazear, Edward P. (2000). "Economic Imperialism". In: *The Quarterly Journal of Economics* 115.1, pp. 99–146.
Leonard, Robert (2010). *Von Neumann, Morgenstern, and the creation of game theory: From chess to social science, 1900–1960*. Cambridge University Press, Cambridge and New York.
Leonard, Thomas C. (2016). *Illiberal reformers*. Princeton University Press, Princeton, NJ.

Levy, David M. (2017). "A Review of Nancy MacLean's "Democracy in Chains: The History of Radical Right's Stealth Plan for America"". In: *George Mason University Working Paper No. 19-19*.
Levy, David M. and Sandra J. Peart (2020). *Toward an Economics of Natural Equals. A Documentary History of the Early Virgina School*. Cambridge University Press, Cambridge and New York.
Lindblom, Charles E. (1997). "Political Science in the 1940s and 1950s". In: *Daedalus* 126.1, American Academic Culture in Transformation: Fifty Years, Four Disciplines, pp. 225–52.
Lohmann, Susanne (2008). "Rational Choice and Political Science". In: *The New Palgrave Dictionary of Economics*. Ed. by Steven Durlauf and Lawrence E. Blume. Palgrave Macmillan UK.
Lowell, Lawrence A. (1910). "The Physiology of Politics: Presidential Address, Sixth Annual Meeting of the American Political Science Association". In: *American Political Science Review* 4.1, pp. 1–15.
Lowi, Theodor J. (1992). "The State in Political Science: how we become what we study". In: *The American Political Science Review* 86.2, pp. 1–7.
Lucas, William F. (1994). "Von Neumann-Morgenstern Stable Sets". In: *Handbooks of Game Theory with Economic Applications*. Ed. by Robert J. Aumann and Sergiu Hart. Vol. I. Elsevier Science Publishers BV, Amsterdam, pp. 544–587.
Luce, Duncan R. and Howard Raiffa (1957). *Games and Decisions: Introduction and critical survey*. John Wiley and Sons, New York, NY.
Maas, Harro and Andrej Svorencik, eds. (2016). *The Making of Experimental Economics. Witness Seminar on the Emergence of a Field*. Springer-Verlag, New York, NY.
MacLean, Nancy (2017). *Democracy in Chains: The deep history of radical right's stealth plan for America*. Viking, New York, NY.
Mäki, Uskali (2009). "Economic Imperialism. Concept and Constraints". In: *Philosophy of the Social Sciences* 39.3, pp. 351–80.
Manne, Henry and James Stocker (2012). "Interview with Henry Manne". In: *Securities and Exchange Commission Historical Society*.
Marschak, Jacob (1946). "Neumann's and Morgenstern's new approach to Static Economics". In: *Journal of Political Economy* 54.2, pp. 97–115.
Maske, Kellie and Garey Durden (2003). "The Contributions and Impact of Professor William H. Riker". In: *Public Choice* 117.1-2, pp. 191–220.
Mathematical Training of Social Scientists, SSRC Committee on (1955). "Recommended policies for the Mathematical Training of Social Scientists: Statement by a Committee of the Council". In: *Items* 9.2, pp. 13–16.
Matthews, Donald R. (1963). "Review to The Theory of Political Coalitions." In: *The Journal of Politics* 25.3, pp. 578–80.
McKelvey, Richard D. (1975). "Intransitivities in Multidimensional Voting Models and Some Implications for Agenda Control". In: *Journal of Economic Theory* 12.472-482.
McKelvey, Richard D. and Peter C. Ordeshook (1978). "Competitive Coalition Theory". In: *Game Theory and Political Science*. Ed. by Peter C. Ordeshook. New York University Press, New York, NY.
McKelvey, Richard D., Peter C. Ordeshook, and Mark D. Winer (1978). "The Competitive Solution for N-Person Games Without Transferable Utility, with an Application to Committee Games". In: *The American Political Science Review* 72.2, pp. 599–615.

McKelvey, Richard D. and Thomas R. Palfrey (1995). “Quantal Response Equilibria for Normal Form Games”. In: *Games and Economic Behavior* 10, pp. 6–38.
McKelvey, Richard D. and Richard D. Smith (1975). “A Comment on The Debate Over Riker’s "Size Principle"”. Mimeographed, Carnegie Mellon University, Box 19, WHRP.
McKenzie, Lionel W. (1954). “On the existence of general equilibrium for a competitive market”. In: *Econometrica: Journal of the Econometric Society* 27.1, pp. 54–71.
– (1957). “Demand theory without a utility index”. In: *The Review of Economics Studies* 24.3, pp. 185–9.
– (1999). “A Scholar’s progress”. In: *Keio economic studies* 36.1, pp. 1–12.
– (2009). “Equilibrium, Trade and Capital Accumulation”. In: *Equilibrium, Trade, and Growth: Selected Papers by Lionel W. McKenzie*. Ed. by Tapan Mitra and Kazuo Nishimura. MIT press, Cambridge, MS.
– (2012). “The early years of the Rochester Department of Economics”. In: *International Journal of Economics Theory* 8, pp. 227–235.
– (n.d.). “Lionel W. McKenzie Papers”. David M. Rubenstein Rare Book & Manuscript Library, Duke University.
McKinsey, John C. C. (1952). *Introduction to the Theory of Games*. McGraw-Hill, New York, NY.
McLean, Iain (2002). “William H. Riker and the Invention of Herestetics”. In: *British Journal of Political Science* 32.3, pp. 535–558.
– (2009). “In Riker’s Footsteps”. In: *British Journal of Political Science* 39.1, pp. 195–210.
– (2015). “The strange history of social choice, and the contribution of the Public Choice Society to its fifth revival”. In: *Public Choice* 163.1-2, pp. 153–165.
McLean, Iain, Alistair McMillan, and Burt L. Monroe, eds. (1998). *The Theory of Committees and Elections by Duncan Black and Committee Decisions with Complementary valuation by Duncan Black and R.A. Newing*. Springer Science & Business Media, New York, NY.
Medema, Steven G. (2000). “"Related Disciplines": The Professionalization of Public Choice Analysis”. In: *History of Political Economy* 32.supplement, pp. 289–324.
– (2009). *The Hesitant Hand. Taming Self-interest in the History of Economic Ideas*. Princeton University Press, Princeton, NJ.
– (2011). “Public Choice and the notion of creative communities”. In: *History of Political Economy* 43.1, pp. 225–246.
Merriam, Charles E. (1923). “Recent Advances in Political Methods”. In: *The American Political Science Review* 17.2, pp. 275–295.
Mirowski, Philip (1992). *More heat than light*. Cambridge University Press, Cambridge and New York.
– (2002). *Machine dreams: Economics becomes a cyborg science*. Cambridge University Press, Cambridge and New York.
Mitra, Tapan and Kazuo Nishimura (2009). “Lionel W. McKenzie”. In: *Equilibrium, Trade, and Growth: Selected Papers by Lionel W. McKenzie*. Ed. by Tapan Mitra and Kazuo Nishimura. MIT press, Cambridge, MS.
Morgan, Mary S. (1990). *The history of econometric ideas*. Cambridge University Press, Cambridge and New York.

Morgenstern, Oskar (1976a). "Perfect foresight and economic equilibrium". In: *Selected economic writings of Oskar Morgenstern*. Ed. by Andrew Schotter. New York University Press New York, pp. 169–83.
– (1976b). "The Collaboration of Oskar Morgenstern and John von Neumann on the Theory of Games". In: *Journal of Economic Literature* 14, pp. 805–16.
– (n.d.). "Oskar Morgenstern Papers". David M. Rubenstein Rare Book and Manuscript Library, Duke University.
Morrell, Jack B. (1972). "The chemist breeders: the research schools of Liebig and Thomas Thomson". In: *Ambix* 19.1, pp. 1–46.
Moscati, Ivan (2018). *Measuring utility: From the marginal revolution to behavioral economics*. Oxford University Press, Oxford.
Mosteller, Frederick (1974). "The role of the Social Science Research Council in the advance of mathematics in the social sciences". In: *Items* 28, pp. 17–24.
Nasar, Sylvia (1998). *A Beautiful Mind: A biography of John Forbes Nash Jr.* Simon & Schuster, New York, NY.
Nash, John F. (2002a). "Non-Cooperative Games (1950)". In: *The Essential John Nash*. Ed. by Harold W. Kuhn and Sylvia Nasar. Princeton University Press, Princeton, NJ.
– (2002b). "The Bargaining Problem (1950)". In: *The Essential John Nash*. Ed. by Harold W. Kuhn and Sylvia Nasar. Princeton University Press Princeton, NJ.
– (2002c). "Two-Person Cooperative Games (1953)". In: *The Essential John Nash*. Ed. by Harold W. Kuhn and Sylvia Nasar. Princeton University Press, Princeton, NJ.
Neumann, John von (1928). "Zur Theorie der Gesellschaftsspiele". In: *Mathematische Annalen* 100, pp. 295–300.
– (1945). "A Model of General Economic Equilibrium". In: *The Review of Economics Studies* 13.1, pp. 1–9.
Neumann, John von and Oskar Morgenstern (1944). *Theory of Games and Economic Behavior*. 2007th ed. Princeton University Press, Princeton, NJ.
Niemi, Richard (2021). "May 27th 2021". Interview with the author.
Olson, Mancur (1965). *The Logic of Collective Action: Public Goods and the Theory of Groups*. Harvard University Press, Cambridge, MS.
Ordeshook, Peter C. (1980). "Political Disequilibrium and Scientific Inquiry: A Comment on William Riker's "Implications from the Disequilibrium of Majority Ruler for the Study of Institutions"". In: *The American Political Science Review* 74.2, pp. 447–50.
– (1990). "The emerging discipline of Political Economy". In: *Perspective on Positive Political Economy*. Ed. by James E. Alt and Kenneth A. Shepsle. Cambridge University Press, Cambridge and New York, pp. 9–31.
– (1996). "Engineering or Science: what is the study of politics?" In: *The Rational Choice Controversy*. Ed. by Jeffrey Friedman. Yale University Press, New Haven, CT, pp. 175–188.
– (2007). "The Competitive Solution Revisited". In: *Positive Changes in Political Science: The Legacy of Richard D. McKelvey's Most Influential Writings*. Ed. by John H. Aldrich, James E. Alt, and Arthur Lupia. University of Michigan Press, Ann Arbor, MI, pp. 165–86.
– (2021). "I'll begin where I began". Manuscript sent to the author.
Orozco Espinel, Camila (2020). "How mathematical economics became (simply) economics: The mathematical training of economists during the 1940s, 1950s,

and 1960s in the United States". In: *Center for the History of Political Economy at Duke University Working Paper Series*.
Osborne, Martin J. and Ariel Rubinstein (1994). *A Course in Game Theory*. MIT press, Cambridge, MS.
Palfrey, Thomas (2005). "Richard Drummond McKelvey (1944-2002)". In: *National Academy of Sciences. Biographical Memoirs* 87.
Pearce, David G. (1984). "Rationalizable Strategic Behavior and the Problem of Perfection". In: *Econometrica: Journal of the Econometric Society* 52.4, pp. 1029–1050.
Persson, Torsten and Guido Tabellini (2002). *Political economics: explaining economic policy*. MIT press, Cambridge, MS.
Poole, Keith T. and Howard Rosenthal (1985). "A Spatial Model for Legislative Roll Call Analysis". In: *American Journal of Political Science* 29.2, pp. 357–384.
Punzo, Lionello F. (1991). "The school of mathematical formalism and the Viennese circle of mathematical economists". In: *Journal of the History of Economic Thought* 13.1, pp. 1–18.
Qualter, Terence H. (1980). *Graham Wallas and the great society*. Palgrave MacMillan, London.
Raiffa, Howard (1992). "Game Theory at the University of Michigan, 1948-1952". In: *Toward a History of Game Theory*. Ed. by E. Roy Weintraub. Duke University Press, Durham, NC.
Ranney, Austin (1974). "The Committee on Political Behavior, 1949–64, and the Committee on Governmental and Legal Processes, 1964–72". In: *Items: Newsletter of the Social Science Research Council* 28, pp. 37–41.
Reiss, Julian (2013). *Philosophy of Economics. A Contemporary Introduction*. Routledge, New York and London.
Rellstab, Urs (1992). "New Insights into the Collaboration between John von Neumann and Oskar Morgenstern on the *Theory of Games and Economic Behavior*". In: *Toward a History of Game Theory*. Ed. by E. Roy Weintraub. Duke University Press, Durham, NC.
"Report on Political Science for Committee on Social Science" (1960). Box 32, LWMP.
"Research in Theoretical Economics" (1960). Box 32, LWMP.
Riker, William H. (1953). *Democracy in the United States*. Macmillan, New York, NY.
– (1957). "Events and situations". In: *The Journal of Philosophy* 54.3, pp. 57–70.
– (1958a). "Causes of events". In: *The Journal of Philosophy* 55.7, pp. 281–291.
– (1958b). "Contributions of Game Theory to Political Theory (mimeo)". WHRP, Box 18, Folder 2.
– (1959a). "A Test of the Adequacy of the Power Index". In: *Behavioral Science* 4.2, pp. 120–131.
– (1959b). "Supplementary Statements, Riker to Tyler, December 4th, 1959". Box 10, Folder 1, WHRP.
– (1960). "Inventory of Personal Interests". Box 10, WHRP.
– (1961). "Voting and the summation of preferences: an interpretive bibliographical review of selected developments during the last decade". In: *The American Political Science Review* 55.4, pp. 900–911.
– (1962a). "The Calculus of Consent by James M. Buchanan and Gordon Tullock". In: *Midwest Journal of Political Science* 6.2, pp. 408–411.

Riker, William H. (1962b). *The Theory of Political Coalitions*. Yale University Press, New Haven, CT.
– (1966). "A New Proof of the Size Principle". In: *Mathematical Applications in Political Science II*. Ed. by Joseph L. Bernd. Southern Methodist University Press, Dallas, TX, pp. 167–74.
– (1968). "Report to MSSB on Summer Course in Mathematical Political Analysis". Box 29, LWMP.
– (1969). "Arrow's Theorem and Some Examples of the Paradox of Voting". In: *Mathematical Applications in Political Science I*. Ed. by John M. Claunch. Southern Methodist University Press, Dallas, TX, pp. 41–60.
– (1970). "Rational Behavior in Politics: Evidence from a Three Person Game". In: *The American Political Science Review* 64.1, pp. 48–60.
– (1971). "Comment on Butterworth, "A Research Note on the Size of Winning Coalitions"". In: *The American Political Science Review* 65.3, pp. 745–47.
– (1977). "The Future of a Science of Politics". In: *American Behavioral Scientist* 21.1, pp. 11–38.
– (1980). "Implications from the Disequilibrium of Majority Ruler for the Study of Institutions". In: *The American Political Science Review* 74.2, pp. 432–446.
– (1982). *Liberalism against Populism: A confrontation between the theory of democracy and the theory of social choice*. Freemand and Company, San Francisco, CA.
– (1984). "The Heresthetics of Constitution-Making: The Presidency in 1787, with Comments on Determinism and Rational Choice. Presidential Address, American Political Science Association 1983". In: *The American Political Science Review* 78.1, pp. 1–16.
– (1986). *The art of political manipulation*. Yale University Press, New Haven, CT.
– (1990). "Political Science and Rational Choice". In: *Perspective on Positive Political Economy*. Ed. by James E. Alt and Kenneth A. Shepsle. Cambridge University Press, Cambridge and New York, pp. 163–82.
– (1992). "The Entry of Game Theory into Political Science". In: *Toward a History of Game Theory*. Ed. by E. Roy Weintraub. Vol. 24. Duke University Press Durham, NC.
– (1996). *The strategy of rhetoric: Campaigning for the American Constitution*. Yale University Press, New Haven, CT.
– (1997). "The Ferment of the 1950s and the Development of Rational Choice Theory". In: *Contemporary Empirical Political Theory*. Ed. by Kristen R Monroe. University of California Press, pp. 191–201.
– (n.d.). "William Harrison Riker Papers". D.262, Rare Books, Special Collections, and Preservation, River Campus Libraries, University of Rochester.
Riker, William H. and Steven J. Brams (1972). "Models of Coalition Formation in Voting Bodies". In: *Mathematical Applications in Political Science*. Ed. by Joseph L. Bernd. Vol. VI. University Press of Virginia.
Riker, William H. and Peter C. Ordeshook (1973). *An introduction to Positive Political Theory*. Prentice-Hall Englewood Cliffs, NJ.
Riker, William H. and Kenneth Shepsle (1979). "Interview to William Harrison Riker by Kenneth Shepsle". Box 85, WHRP.
Rizvi, Abu Turab S. (1994). "Game theory to the rescue?" In: *Contributions to Political Economy* 13.1, pp. 1–28.
Robbins, Lionel (1932). *The nature and significance of economic science*. Routledge, London.

Robinson, James A. (1963). "Decision-Making and Coalition Building". In: *The Journal of Conflict Resolution* 7.4, pp. 763–8.
Roos, Charles F (1933). "Constitution of the econometric society". In: *Econometrica: Journal of the Econometric Society*, pp. 106–108.
Ross, Dorothy (2009). "Anglo-American Political Science, 1880-1920". In: *Modern Political Science: Anglo-American exchanges since 1880*. Princeton University Press, Princeton, NJ, pp. 18–36.
Roth, Alvin E. (1988). "Introduction to the Shapley Value. Essays in honor of Lloyd S. Shapley". In: *The Shapley Value*. Ed. by Alvin Roth. Cambridge University Press, Cambridge and New York, pp. 1–27.
Roth, Alvin E. and Robert B. Wilson (2019). "How Market Desigh Emerged from Game Theory: A Mutual Interview". In: *Journal of Economic Perspectives* 33.3, pp. 118–143.
Rubinstein, Ariel (1982). "Perfect Equilibrium in a Bargaining Model". In: *Econometrica: Journal of the Econometric Society* 50.1, pp. 97–109.
– (2007). "Afterword". In: *Theory of Games and Economic Behavior*. Princeton University Press, Princeton, NJ.
Samuelson, Paul A. (1947). *Foundations of Economic Analysis*. Harvard University Press, Cambridge, MS.
Scaff, Lawrence A. (2011). *Max Weber in America*. Princeton University Press, Princeton, NJ.
Schelling, Thomas C. (1980). *The Strategy of Conflict [1961]*. Harvard University Press, Cambridge, MSniversity press, Cambridge, MS.
Schofield, Norman (1996). "Rational Choice and Political Economy". In: *The Rational Choice Controversy*. Ed. by Jeffrey Friedman. Yale University Press, New Haven, CT.
Schotter, Andrew (1992). "Oskar Morgenstern's Contribution to the Development of the Theory of Games". In: *Toward a History of Game Theory*. Ed. by E. Roy Weintraub. Duke University Press, Durham, NC.
Schumpeter, Joseph A. (1942). *Capitalism, Democracy and Socialism*. Harper & Brothers, New York, NY.
– (1987). *History of Economic Analysis [1954]*. Routledge, New York and London.
Selten, Reinhardt (1965). "Spieltheoretische Behandlung eines Oligopolmodells mit Nachfrageträgheit". In: *Zeitschrift für die gesamte Staatswissenschaft* 121, pp. 301–24.
– (1975). "Re-examination of the Perfectness Concept for Equilibrium Points in Extensive Games". In: *International Journal of Game Theory* 4, pp. 25–55.
Serrano, Roberto (2005). "Fifty Years of the Nash Program, 1953-2003". In: *Investigaciones Económicas* XXIX.2, pp. 219–258.
Shackle, George L. S. (1967). *The years of high theory: invention and tradition in economic thought 1926-1939*. Cambridge University Press, Cambridge and New York.
Shapley, Lloyd S. (1953). "A Value for n-Person Games". In: *Contributions to the Theory of Games*. Ed. by Harold W. Kuhn and Albert Tucker. Vol. II. Princeton University Press, Princeton, NJ, pp. 307–317.
Shapley, Lloyd S. and Martin Shubik (1954). "A method for evaluating the distribution of power in a committee system". In: *The American Political Science Review* 48.3, pp. 787–792.
Shepsle, Kenneth A. (1974). "On the Size of Winning Coalitions". In: *The American Political Science Review* 68.2, pp. 505–18.

Shepsle, Kenneth A. (1979). “Institutional Arrangements and Equilibrium in Multidimensional Voting”. In: *American Journal of Political Science* 23.1, pp. 27–59.
– (2021). “May 17th 2021”. Interview with the author.
Shubik, Martin (1954). *Readings in Game Theory and Political Behavior*. Vol. 9. Doubleday, London.
– (1961). “Referee’s report on 'The Theory of Political Coalitions" by William H. Riker”. Box 8, MPS.
– (1984). “Game Theory Models and Methods in Political Economy”. In: *Handbook of Mathematical Economics*. Ed. by Kenneth J. Arrow and Michael D. Intriligator. Vol. 1. North Holland, Amsterdam.
– (1992). “Game Theory at Princeton, 1949–1955: a personal reminiscence”. In: *Toward a History of Game Theory*. Ed. by E. Roy Weintraub. Duke University Press, Durham, NC.
– (2002). “Game Theory and Operations Research: Some Musings 50 years later”. In: *Operations Research* 50, pp. 192–6.
– (n.d.). “Martin Shubik Papers”. David M. Rubenstein Rare Book and Manuscript Library, Duke University.
Simon, Herbert (1945). “Review to The Theory of Games and Economic Behavior”. In: *The American Journal of Sociology* 50.6, pp. 558–60.
– (1947). *Administrative Behavior*. Macmillan, New York, NY.
– (1996). *Models of My Life*. MIT press, Cambridge, MS.
Smith, Mark (2009). “A Tale of Two Charlies: Political Science, History, and Civic Reform, 1890–1940”. In: *Modern Political Science: Anglo-American Exchanges Since 1880*. Ed. by Robert Adcock, Mark Bevir, and Shannon C Stimson. Princeton University Press Princeton, NJ, pp. 118–36.
Smith, Vernon L. (1992). “Game Theory and Experimental Economics: Beginning and Early Influences”. In: *Toward a history of game theory*. Ed. by E. Roy Weintraub. Duke University Press, Durham, NC.
Social Sciences (UR), Committee on the (1961). “Minutes of the Meeting, October, 2, 1961”. Box 32, LWMP.
– (n.d.). “Minutes of the Meeting, November 6th, 1961”. Box 32, LWMP.
Solovey, Mark (2013). *Shaky Foundations. The Politics-Patronage-Social Science Nexus in Cold War America*. Rutgers University Press, New Brunswick, NJ.
Somit, Albert and Joseph Tanenhaus (1967). *The development of political science: From Burgess to behavioralism*. Allyn and Bacon, Boston.
Spencer, Roger W. and David A. Macpherson, eds. (2014). *Lives of the Laureates*. MIT press, Cambridge, MS.
Taylor, Michael (1971). “Mathematical political theory”. In: *British Journal of Political Science* 1.3, pp. 339–382.
Tribe, Keith (2022). *Constructing Economic Science. The Invention of a Discipline 1850-1950*. Oxford University Press, Oxford.
Truman, David B. (1951). *The governmental process*. Alfred A. Knopf, New York, NY.
– (1965). “Disillusion and regeneration: the quest for a discipline”. In: *American Political Science Review* 59.4, pp. 865–873.
Tullock, Gordon (2004). “Economic Imperialism [1972]”. In: *Virginia Political Economy. The Selected works of Gordon Tullock*. Ed. by Charles K. Rowley.
Tyler, Ralph W. (1956). “Study Center for Behavioral Scientists”. In: *Science* 123.3193, pp. 405–8.

"University of Rochester Undergraduate Bulletin 1961-2" (1961).
"University of Rochester Undergraduate Bulletin 1967-8" (1967).
Wald, Abraham (1947). "Theory of Games and Economic Behavior by John v. Neumann and Oskar Morgenstern". In: *The Review of Economics and Statistics* 29.1, pp. 47–52.
Wallas, Graham (1920). *Human nature in politics [1908]*. Constable and Co., London.
Ward, Benjamin (1972). *What's wrong with economics?* Palgrave MacMillan, London.
Weintraub, E. Roy (1983). "On the existence of a competitive equilibrium: 1930-1954". In: *Journal of Economic Literature* 21.1, pp. 1–39.
– (1991). *Stabilizing dynamics: Constructing economic knowledge*. Cambridge University Press.
– ed. (1992). *Toward a History of Game Theory*. Duke University Press, Durham, NC.
– (2002). *How Economics became a Mathematical Science*. Duke University Press, Durham, NC.
– (2008). "Mathematics and Economics". In: *The New Palgrave Dictionary of Economics*. Ed. by Steven Durlauf and Lawrence E Blume. Palgrave MacMillan, London.
Weintraub, E. Roy and Philip Mirowski (1994). "The pure and the applied: Bourbakism comes to mathematical economics". In: *Science in Context* 7.2, pp. 245–272.
Wilson, Robert (1971). "Stable Coalition Proposals in Majority-Rule Voting". In: *Journal of Economic Theory* 3, pp. 254–71.
– (1972). "The Game-Theoretic Structure of Arrow's General Possibility Theorem". In: *Journal of Economic Theory* 5.14-20.