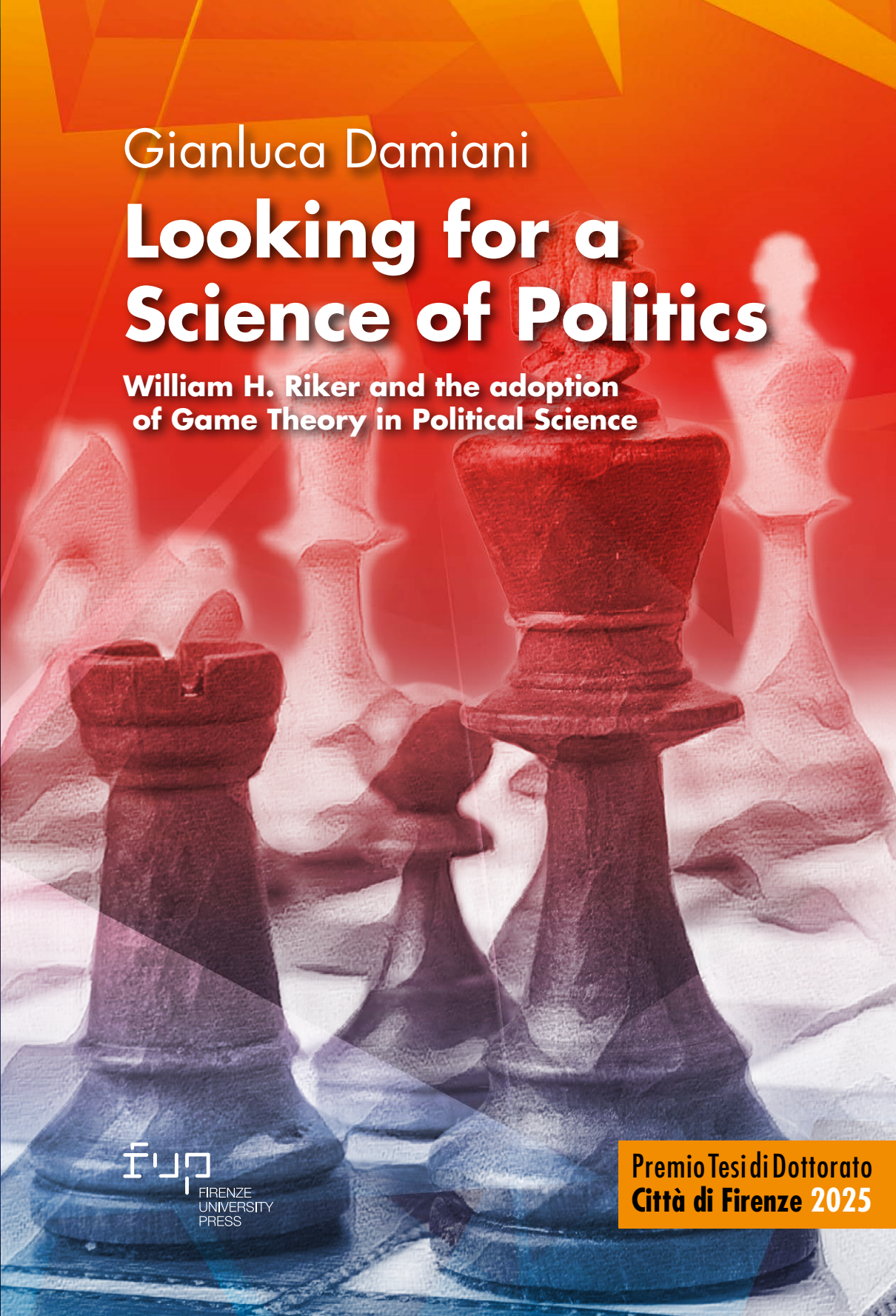
Gianluca Damiani

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 Eighties and Nineties, Game Theory became one of the most dynamic fields of economic theory. Its extraordinary success can also be appreciated by considering the widespread diffusion of notions that are now part of everyday language, such as "zero-sum," "games," or even the "prisoner's dilemma."

In the words of one of its early historians, E. Roy Weintraub, the simplistic view of the history of Game Theory in economics can be summarized as follows: "[...] von Neumann wrote a paper in the late Twenties 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." (Weintraub 1992, p. 7) However, more than thirty years of historical research have shown that this narrative is superficial and misleading, and that Game Theory struggled significantly before entering the economists' toolbox.

This work aims to further contribute to the history of Game Theory by showing how the Theory of Games crossed the disciplinary boundaries of economics and entered political science. This process was largely driven by the American political scientist William H. Riker. Although trained as a traditional political scientist, in the second half of the Fifties Riker became deeply committed to game theory and to the development of a formal (that is, mathematical) political science. His activities were pivotal in shaping a subfield of contemporary political science now known as "Positive Political Theory."

Moreover, Riker's activities were not limited to advocating the use of Game Theory. He was also an exceptional "intellectual entrepreneur," whose efforts ranged from providing game-theoretic analyses of political phenomena to actively promoting his research program within political

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science as an established discipline. This was made possible by Riker's role in founding and developing a department—most notably a Ph.D. program—at the University of Rochester (NY), explicitly devoted to analytical models of politics and to the recruitment and training of scholars working within this framework.

Riker is the central figure of this research, and it will be therefore devoted extensive attention to his life and his position within postwar American political science, a field that underwent profound transformations in both scope and methodology. However, I will largely overlook Riker's early work as a "traditional" political scientist, as well as his important contributions to the study of federalism (Riker 1953; for an overview of Riker's theory of federalism, see Filippov 2005). In addition, there will be only a brief mention of what are perhaps Riker's most well-known contributions among political scientists: his political theory of liberalism versus populism and his theory of "heresthetics," namely the strategic manipulation of political agendas and voting outcomes (Riker 1982).

In short, this work is neither an intellectual biography of Riker nor a comprehensive overview of his scientific output.[1] Nor is it a review of the many epistemological, methodological, and philosophical critiques directed at rational-choice approaches to politics. Rather, it is a history of contemporary economics and political science centered on a specific issue: the entry of Game Theory into the toolbox of political science.

Over the past thirty years, no scholar has probably devoted more effort than Weintraub to reflecting on how the history of economics can and should be written. He identified several approaches to the historiography of economics (Weintraub 2002, 256 et ss.). The first assumes that economics, like other sciences, progresses cumulatively, allowing for a rational reconstruction of how ideas evolved from primitive analyses to more advanced ones. This view is clearly naïve, and its shortcomings are only partially addressed by what Weintraub calls "critical rationalism." This approach rejects a fully cumulative account of scientific development, yet still focuses on "exemplars of good science." "Good science" can be falsified (in the Popperian sense) or can constitute a "progressive research program" (following Imre Lakatos), while "bad science" is characterized as a "regressive research program." Kuhnian notions of "scientific revolutions" similarly generate narratives of progress and decline. As Weintraub observed in the case of economics, these approaches provide "a sense of vitality of economic science" (Weintraub 2002, p. 260). Nonetheless, they continue to grant centrality to the idea of science as a cumulative enterprise. From a historical perspective, a more comprehensive view is offered by the so-called "science studies approach," whose 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" (Weintraub 2002, p. 267).

Weintraub's taxonomy parallels more traditional historiographical dis-

[1] A definitive, though introductory, appraisal of Riker's academic career and impact on political science is provided by Maske and Durden 2003, which also includes a citation analysis of his work and commentaries by colleagues and students.

tinctions, particularly Joseph Schumpeter's famous dichotomy between "analysis" and "vision," and Lakatos's distinction between "internalist" and "externalist" histories of science (Schumpeter 1987; Lakatos 1978). In brief, Schumpeter's history of analysis and internalist approaches aim to reconstruct the internal content of theories, whereas externalist histories focus on the intellectual, institutional, and social contexts—journals, universities, research centers—that shape scholarly work. Schumpeter's notion of "vision" instead refers to the "preanalytic cognitive act that supplies the raw material for the analytic effort" (Schumpeter 1987, p. 39).

During the same period (roughly the Fifties to the Sixties) in which Game Theory struggled to gain recognition among economists, it attracted the attention of other social scientists, including political scientists. Part of this story is well known, particularly the application of Game Theory to international relations and military strategy. Substantial attention has also been devoted to the role of game theory in the context of "Cold War rationality" and to the institutions that shaped it, such as the RAND Corporation (Amadae 2003; Erickson et al. 2015). By contrast, less attention has been paid to how Game Theory diffused within political science and how this diffusion reshaped the discipline's methodological canon through its interaction with emerging subfields such as Public Choice, Political Economy, and Positive Political Theory.

It is precisely this latter story that exhibits a number of features worthy of closer examination.

The label "Positive Political Theory" was coined by Riker in the late Fifties and referred to what he regarded as the superior descriptive power of political theories grounded in Rational Choice and Game Theory. In his view, these methods finally allowed political science to move "from the purely inductive to include the deductive as well" (Riker and Ordeshook 1973, p. xi). Riker embraced a positivistic conception of political science as a progressive enterprise aimed at developing a "genuine science of politics." Deductive, axiomatic theory, he argued, made it possible to define political phenomena more precisely and thus to subject them to systematic empirical investigation capable of yielding meaningful predictions, in a manner analogous to postwar economics.

As this work will show, however, such a bold vision was beset by substantial theoretical and epistemological difficulties. A more contemporary and less heroic definition of Positive Political Theory is therefore 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 work seeks to explain why Riker's original conception was problematic, or at least overly optimistic, with respect to the actual explanatory and predictive power of Game Theory. As long-standing debates in the philosophy of economics have shown, the primary strength of Game Theory lies in its capacity to provide powerful modeling tools, rather than in offering strong normative guidance or robust predictive claims (see Reiss 2013; Rubinstein 2007). This more modest understanding is clearly reflected in

the latter definition of Positive Political Theory.

The analysis also includes a discussion of the formal aspects of Riker's work. Notably, Riker relied primarily on cooperative game theory rather than on non-cooperative approaches, thereby drawing directly on the original framework developed by John von Neumann and Oskar Morgenstern. This work argues that, for Positive Political Theory, a much closer theoretical and institutional affinity existed with the von Neumann–Morgenstern tradition than with the later Nash-based revolution. Non-cooperative Game Theory would exert a substantial influence on the study of politics only much later, beginning in the late Seventies and early Eighties, roughly coinciding with its broader acceptance within economics.

At the same time, Riker's analysis was mathematically flawed. Setting aside biographical considerations, most notably Riker's limited formal training in mathematics, it is offered an epistemological account of how Riker employed Game Theory and how this usage differed from that of economists.

Closely related to these issues is the controversial notion of "economic imperialism." The question of whether the extension of economic analysis beyond its traditional domains constitutes a legitimate intellectual endeavor has often been used to assess the epistemological coherence of rational choice and game-theoretic approaches when applied to disciplines such as sociology, political science, and history.

Methodologically, this work provides a historical reconstruction of Riker's early work, with particular emphasis on *The Theory of Political Coalitions* (1962). It combines both internalist and externalist elements without fully committing to either approach. It is internalist insofar as it examines Riker's specific use of Game Theory and offers critical assessments, beginning with his conception of rationality. At the same time, it relies heavily on historical sources, including archival material and interviews, and seeks to reconstruct the external forces that supported Riker's efforts to institutionalize Positive Political Theory.

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On a more personal note, I benefitted by talking with several friends (and scholars as well) in these years. Among them, discussing with Jacopo Marchetti and Edoardo Peruzzi always provided a lot of exciting intellectual stimuli and was worth all the time devoted.

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Chapter 1

The parallel paths of Economics and Political Science: from the early XXth Century to the Postwar

The interwar and second postwar years witnessed dramatic changes in the history of the social sciences. The following pages aim to briefly reconstruct this history with reference to the cases of political science and economics.

In economics, the main development consisted in its transformation into a mathematical discipline. The evolution of economics toward its contemporary mathematical form has been explored by many scholars, beginning with Weintraub's pivotal studies (e.g., Weintraub 1983; Weintraub 2002; Ingrao and Israel 1987; Mirowski 1992; Mirowski 2002; Giocoli 2003b; Giocoli 2003a, among others). A central role in these analyses is played by the increasing level of abstract mathematical formalism, which progressively turned economics into a mathematical science. As a result, both the conception and the teaching of economic theory were radically transformed within a relatively short period (roughly 1930–1960). This transformation entailed a shift from what Giocoli defines as the traditional image of economics as a "system of forces" toward a new image of economics as a "system of relations"(Giocoli 2003b;Giocoli 2009a). According to the first view, economics analyzes economic processes generated by market forces, including but not limited to equilibrium outcomes. 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).

As an illustration, Nobel laureate Robert Fogel reported a remark made to him by his colleague Lionel W. McKenzie, one of the most prominent postwar mathematical economists, during the Sixties, when both were members of the faculty of the University of Rochester's Department of Economics. Commenting on the existence and uniqueness of general economic equilibrium, McKenzie stated: "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]

[1] McKenzie's role as chair of the Rochester economics department from the late Fifties will be discussed in a separate chapter of this dissertation, since Rochester was also

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Giocoli's distinction thus highlights the emergence of radically different questions generated by the progress of the discipline.

From a historical perspective, the steady advance of mathematical economics also affected how the history of the discipline came to be understood by its practitioners. It implicitly legitimized a view of economic theory as progressing through the continuous refinement—and creation—of new mathematical tools to address novel problems or to shed new light on existing ones. Gérard Debreu (Nobel Prize winner in 1983), perhaps the most "extreme" representative of mathematical economics, defended the use of mathematics in economics (particularly the axiomatic method) by repeatedly emphasizing four advantages: generality, weakness of assumptions, clarity of expression, and freedom from ideology (Düppe 2010). Of these, the latter is particularly relevant in the case of economics. It can be associated with a "Whig" perspective that emphasizes ongoing theoretical refinement and progress. In reconstructing the history of economic theory, this perspective makes it possible to detach theoretical developments from the institutional and social contexts in which they emerged. As a consequence, Debreu's own assessment of mathematical economics would seem to hold: "[...] 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 Weintraub 2014b, p. xiii).

Important transformations also occurred in political science during the twentieth century. However, unlike economics, these changes were not accompanied by a comparable narrowing of methodological pluralism. The development of political science can instead be described as an "epistemic shift" from what some authors have labeled "developmental historicism" to "modernist empiricism." By the former, Robert Adcock and Mark Bevir refer to several nineteenth-century traditions in the social sciences, including philosophical idealism, positivism, Whiggism, and early evolutionary theories. "Modernist empiricism," by contrast, is commonly understood 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" (Smith 2009, p. 118; Adcock and Bevir 2010). In the Fifties, efforts to establish a more systematic political science culminated in the Behavioralist movement, which became the dominant approach in political studies during that decade and part of the next. These efforts entailed extensive debates concerning the proper methods and scope of a scientific analysis of politics.

Thus, constructing a "real science of politics" also required addressing the appropriate relationship between political science and political philosophy, political theory, history, and constitutional law—disciplines that had traditionally been devoted to the study of political order. Defining this relationship has been one of the central challenges in the development of political science as an autonomous discipline. In this sense, the history of political science can be understood as the history of successive answers to

the institution where Riker established his formal approach to political science, and there existed a certain proximity between the two departments.

questions concerning the nature of political phenomena. As in the histories of other disciplines, political science appears as a sequence of attempts to address different problems through the elaboration of distinct theoretical and methodological approaches.

In the following chapter, it will be offered a general, though not exhaustive, reconstruction of some key features in the development of economic theory from the interwar period to the Fifties. Its purpose is to provide the background necessary to analyze the development of Game Theory and other formal approaches beyond the boundaries of economics from the Fifties onward.

A "linear narrative" of the development of political science, focusing primarily, though not exclusively, on the American case, is presented. In doing so, two widely shared interpretations are implicitly adopted: first, the use of Behavioralism as a key lens for understanding the development of political science; and second, the narrative of the "Americanization" of the discipline (Somit and Tanenhaus 1967).

Given the pivotal role of the behavioral movement in shaping political science in the second postwar period, a common framework for the discipline's history is the periodization into "Pre-Behavioralism, Behavioralism, and Post-Behavioralism."[2] Consequently, the "Behavioral Revolution" occupies a central place in how contemporary political scientists conceptualize their discipline's past. The narrative of "Americanization," for its part, holds that American scholars pioneered the institutionalization of political science as an autonomous discipline (Adcock and Bevir 2010, p. 71). While American scholarship was undoubtedly shaped by transnational exchanges of ideas and individuals, for nearly half a century the existence of an autonomous discipline of political science, as well as independent scholarly associations, remained a North American anomaly. In most other countries, political science emerged as an autonomous discipline primarily in the second postwar period (Easton, Graziano, and Gunnell 2002). By way of illustration, the American Political Science Association was founded in 1903, and the first issue of its journal, the *American Political Science Review*, appeared in 1906. At the same time, the American Economic Association had already been established in 1885, and the Royal Economic Society in England in 1890. By contrast, the British Political Studies Association was founded only in 1950, following the UNESCO Symposium on "Contemporary Political Science" (Kenny 2009).

Both interpretations, however, have been challenged by a number of scholars and historians. Adcock, for example, has convincingly argued that the belief in the revolutionary role of Behavioralism must be qualified (Adcock 2009). While Behavioralism was undoubtedly innovative in both character and impact, some of the changes it introduced were neither immediate nor radical, contrary to what is often implied. Moreover, Behavioralism lacks a precise definition, and interpreting it as a scientific revolution is highly problematic (Dahl 1961). John Dryzek reconstructed

[2] Despite the lack of a precise definition of Behavioralism, its features and meanings will be discussed in the second section.

the history of American political science in terms of five different "revolutions without enemies" (Dryzek 2006). In his account, the notion of revolution, in a Kuhnian sense, is largely metaphorical, since none of these developments qualifies as a genuine "paradigm shift." According to Dryzek, only two revolutions can be considered successful, and their success is explained by the absence of organized resistance rather than by radical intellectual rupture (Dryzek 2006, p. 487). That Behavioralism was one of these two "successful" revolutions (the other being Statism) is insufficient to justify interpreting the history of political science exclusively through this lens. Unlike mathematical formalism in economics, Behavioralism did not fundamentally transform the overall "image" of the discipline.

Finally, it would also be misleading to portray political science in the Fifties and Sixties as being entirely dominated by Behavioralism. As it will be shown, Riker's formal approach was developed during this period partly in opposition to Behavioralism, though not exclusively so (Adcock 2009). Other scholars have also demonstrated that, at least up to the Fifties, the historical development of political science followed broadly similar trajectories in the United States and Great Britain (Ross 2009). This observation could be taken to justify writing the history of political science primarily as a function of the American experience. However, alongside the functionalist interpretation of Behavioralism as the unique and narrow path leading to the contemporary discipline, the main risk remains that of producing a "Whig" narrative of the history of political science.

Despite the fairness of many of these critiques, nonetheless retaining an incremental view of the development of political science is justified by the aim of the present chapter, which is not to provide a comprehensive history of the discipline, but rather to outline a simplified, yet sufficiently detailed, framework of the institutional and disciplinary status of political science up to the Fifties.

1.1 The Development of Economics

1.1.1 The rise of Mathematical Economics

This section presents some features of the dramatic changes that occurred in economic theory in that period, culminating in the creation of the Theory of Games and in the broader mathematization of economics. However, issues such as business cycles, theories of utility, and theories of costs will not be discussed. Instead, the focus will be on selected aspects of the mathematical transformation of economics and on its role in reshaping economic theory in the axiomatic neo-classical fashion of the second postwar.[3]

[3] Usually, comprehensive histories of economic thought end with Keynesian economics, offering only a glance at the most contemporary theories or at Mathematical Economics. By contrast, the works devoted specifically to these topics are often extremely detailed historically, yet they do not always provide a broader account of the development of economic theory. It therefore often seems difficult to read them properly without mastering the earlier literature. A classical general work that remains highly detailed on the development of economics between 1870 and

In the Thirties, despite the so-called marginal revolution of the last quarter of the nineteenth century having introduced increasing levels of abstraction to deal with counter-intuitive concepts such as marginal utility or marginal productivity, significant methodological differences still separated distinct "schools." These differences concerned the use of mathematical reasoning, statistics, and competing views about the meaning and scope of economics. In the "years of high theory" (Shackle 1967), the main issues at stake included consumer theory, business cycles, production, and capital theory; and, from a methodological standpoint, whether economics was an a priori discipline or could instead adopt a neo-positivist perspective. An important issue, largely absent from Shackle's narrative but addressed by Weintraub and by Ingrao and Israel (Weintraub 1983; Ingrao and Israel 1987), was the adoption of the axiomatic method to tackle fundamental economic problems such as modeling individual rationality and, on that basis, establishing early results on general economic equilibrium. Moreover, these scientific and theoretical efforts were deeply embedded in parallel transformations in mathematics and theoretical physics.

Some authors have described the radical mathematization that followed 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 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)

Other scholars, by contrast, have preferred to avoid this label, given the double ambiguity of the concepts of "revolution" and "formalist" (Giocoli 2003b, p. 6). Yet, whatever the terminology, the change in economic theory and practice between the Thirties and the Fifties is undeniable.

Debates over the appropriate role of mathematics in economics date back to the mid-nineteenth century, to authors such as Augustine Cournot, William Whewell, Hermann Heinrich Gossen, Jules Dupuit, and Johann Heinrich von Thunen, among others. The marginal revolution after the 1870s strengthened the relationship between mathematics and economics, while also introducing a new element. The standard justification of mathematical economics rested on the intrinsically quantitative nature of economic phenomena (a position still broadly associated with authors such as William Stanley Jevons and Leon Walras). However, more mathematically trained scholars, such as the American Irving Fisher, explicitly defended the use of mathematics in economics on different grounds: as a means of assessing the

1930 (and whose knowledge helps to understand more specific results) is Blaug 1997. Another classical work, although much less general than Blaug, is Shackle 1967. Particular issues are treated in Ingrao and Israel 1987; Düppe and Weintraub 2014b (General Economic Equilibrium); Morgan 1990 (Econometrics); Weintraub 1991 (Economic Dynamics); Weintraub 2002 (Mathematical Economics); Giocoli 2003b (Game Theory and Rationality); Leonard 2010 (Creation of Game Theory); Moscati 2018 (Utility Theories)

internal consistency of a theory (Fisher 1892). Fisher's view anticipated positions that would become customary decades later.

In 1909, the Irish-born, Oxford-based Francis Ysidro Edgeworth offered a synthesis of both perspectives. For Edgeworth, economics possessed the essential condition for applying mathematics, namely the constancy of quantitative relations (his example being the law of diminishing returns) (Edgeworth 2008). At the same time, he argued that the usefulness of mathematics was not limited to expressing quantitative relations. As he put it: "[. . .] 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 emphasized the risks of abusing mathematical reasoning, particularly its tendency to be overrated. He therefore maintained that mathematical treatment is unlikely to be helpful unless a problem has first been studied and analyzed economically.

From the Thirties onward, mathematics acquired a new role within economic theory. Historians, philosophers of science, and practitioners have offered different interpretations of this transformation. The common challenge is to explain how changes in economics reshaped its relationship with mathematics, up to the incorporation of certain features of the latter (Weintraub 2002). In this regard, the radical transformations of late nineteenth- and early twentieth-century mathematics,especially axiomatization and David Hilbert's formalist program, played an important role. The key question is therefore how axiomatic mathematical theory entered economics, and how this differed from earlier uses of mathematics (such as calculus). More broadly, the issue is closely tied to debates in the philosophy of science about the nature of scientific knowledge, particularly within the German-speaking intellectual, scientific, and philosophical world (including neo-positivism).

Closely related is the question of how "mathematical economics became only economics" (Orozco Espinel 2020), that is, how mathematical methods became dominant within the discipline, eventually marginalizing most theoretical approaches other than the mathematical ones.

A historian may thus propose the following rational outline of the interwar debates in economics. Proper definitions and concepts were needed to provide reliable knowledge of economic phenomena. One potentially helpful concept was equilibrium, which in turn raised new issues: how equilibrium might be reached and how it should be defined. These discussions also involved rationality and the analysis of economic action, and thus broader questions about the nature of economics as a science. Psychological and hedonistic explanations, it was argued, had to be set aside insofar as they could not be adequately demonstrated. The same was said of theories relying on perfect foresight. For some authors, this undermined the general validity of equilibrium approaches altogether (a route followed by Friedrich August von Hayek: Hayek 1937). Others, while raising concerns similar to

Hayek's (see, for instance, Morgenstern 1976a), pursued instead a possible way out through the increasing mathematization of economic analysis. Philosophical discussions concerning the status of economics as a science, and, more generally, the nature of scientific knowledge, also provided a favorable assessment of the mathematical approach, beyond the standard appeal to quantitative analysis.

In addition, contingent political and social concerns played a role. As Robert Leonard has shown in the case of John von Neumann, concrete political developments such as the disruption of the European political order after the Nazi rise to power were central to his reflections on the concept (and meaning) of equilibrium and, more generally, on the concept of solution in his work (Leonard 2010). Similar considerations apply to figures such as the Austrian mathematician Karl Menger, whose role in connecting mathematics and economics, and the communities of their practitioners, was pivotal.

To appraise these developments properly, historians of economics have explored the evolution of economic theory, mathematics, and the philosophy of science, as well as the personal and institutional connections among scholars across different places. 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." (Weintraub 2008, p. 462)

Giocoli, as noted above, summarizes this process as a transformation of the image of economics from a "system of forces" to a "system of relations," in which general equilibrium and its "existence theorem" occupy a central position. More specifically, Giocoli and Weintraub apply the historian of mathematics Leo Corry's dichotomy between "body of knowledge" and "image of knowledge" to twentieth-century neo-classical economics (Corry 2003; Weintraub 2002; Giocoli 2009a). In Corry's view, the "body of knowledge" comprises a discipline's theories, facts, methods, and open problems. The "image of knowledge," by contrast, concerns attitudes about what counts as an open problem, what kinds of arguments are relevant, how disagreements are adjudicated, and which methodological standards apply. It also includes the emergence of institutions devoted to evaluating intellectual contributions, new academic curricula, and related developments (Giocoli 2009a, p. 23).

According to Giocoli, a major shift in the image of economics occurred after the Second World War, with significant consequences for its body of knowledge. The two poles of this transformation were, on the one hand, the traditional view of economics as a discipline dealing with a system of forces (an image associated with conventional economics and embraced by early marginalists) and, on the other, the new view of economics as a discipline dealing with a system of relations. This shift in the image of the discipline also helps explain the modern conception of rationality in economics, that

is, Rational Choice Theory.

The movement toward the "system of relations" image was influenced by new philosophical concerns about the epistemological foundations of science and the logical foundations of mathematics. Equally important, however, was the perceived stalemate of neo-classical theory in the interwar years. This stalemate arose from a series of unsatisfactory attempts to explain how equilibrium could emerge from the adjustment of individual agents' expectations and plans. Neo-classical economists struggled, on the one hand, to explain equilibrium as a balance of forces, namely "as the rest point of the system's dynamics," and, on the other hand, to incorporate into their models mental variables—that is, "the actual forces which determine the process of adjustment toward an equilibrium." (Giocoli 2003b, pp. 368–9)

Neo-classical economic theory followed two distinct, though not strictly parallel, paths in addressing these theoretical and empirical challenges. In Giocoli's terms, these were an "escape from psychology" and an "escape from perfect foresight." The first refers to freeing economic analysis from hedonistic accounts of valuation. Closely related is the adoption of ordinal utility in place of cardinal utility (see the classic contributions of Vilfredo Pareto, John Hicks, and Roy Allen). More broadly, psychology-free analysis—particularly in the work of Paul Samuelson, also enabled a more explicitly empirical foundation for economic theory through an operationalist approach (Samuelson 1947. On Samuelson's early life in the Thirties and Forties, see Backhouse 2017). The second path refers to attempts to develop a more comprehensive and purely theoretical framework, namely "a pure logic of choice," in which economic agents face informational problems without perfect knowledge.

These developments were also deeply influenced by debates in the philosophy of science. In this respect, the spread of neo-positivism, its commitment to a scientific conception of the world (including the possibility of a unified science), and its emphasis on logical-mathematical deduction had a profound effect on how economic theory was conceived. It is not accidental that many of these debates were concentrated in Vienna, where discussions of mathematical social science intertwined with debates on the logical foundations of mathematics and with the development of new forms of pure mathematics.[4]

Neo-positivism focused on the relation between mathematical propositions and the empirical verifiability of scientific statements as a basis for knowledge of the external world. Empirical verifiability, together with an emphasis on logical consistency, underpinned the key feature of logical positivism: the distinction between scientific and metaphysical assertions.

[4] In the case of economic theory, these debates involved figures such as Karl Menger, Abraham Wald, Morgenstern, and Karl Schlesinger, and also, albeit briefly, von Neumann. They engaged primarily with Walras's 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 attempts to establish the logical foundations of mathematics (Punzo 1991). The postwar debates about the existence and properties of General Economic Equilibrium built on results developed in Vienna.

In debates on economic methodology, these issues can be summarized by the tension between a deductive, aprioristic approach grounded in reasoning and an empirical approach. Propositions in "pure economics," derived from generalized claims about human behavior in order to establish basic principles (such as diminishing utility or maximizing behavior), belong to the first. The opposing position instead emphasized verification as the relevant standard for economic claims and conclusions.[5]

As noted above, the development of mathematics between the nineteenth and twentieth centuries profoundly affected its later employment in economics. In the last decades of the nineteenth century, mathematics underwent a deep process of foundational reconstruction, which changed how it was understood, developed, taught, and applied. By 1900 the image of mathematics had changed, as had its practice. New problems emerged, particularly those concerning the foundations of mathematics, and new concepts such as infinity and the continuum of real numbers became central. Physics, too, went through a crisis, with new problems such as quanta and relativity. Addressing these new physical concerns required new mathematics.

New mathematics was also needed to overcome logical weaknesses in foundational questions, and one of its main outcomes was the elaboration of the axiomatic method. Under this approach, rigorous arguments are built from explicitly stated foundations and axioms. This logical grounding enabled the development of new branches of mathematics and provided new conceptual tools for producing mathematical knowledge. The notion of "rigor" acquired fundamental importance, both for explorations in mathematical logic and for supporting an autonomous notion of progress within mathematics. This process reshaped the relationship between mathematics, physics, the natural sciences, and the social sciences (Weintraub 2002. See also Weintraub 2008).

The central feature of this transformation was the development of the axiomatic method, largely associated with David Hilbert and his so-called "formalist program" for mathematics. With Hilbert's commitment to proof theory or "meta-mathematics," twentieth-century mathematics profoundly changed its face.[6] Historians of mathematics have offered different interpretations of the "formalist program," especially regarding Hilbert's aims and broader ambitions. In any case, it raised fundamental

[5] The champions of these positions are usually identified as two English economists: Lionel Robbins (for the abstract-deductive view) and Terence Hutchinson (Robbins 1932; Hutchison 1938). From a history of ideas perspective, this contrast is not unproblematic. It seems clear that mathematical economics, given its reductionism about economic action, and its reliance on mathematically convenient postulates about behavior, is closer to the "pure economics" view. Indeed, some scholars have emphasized the influence of Robbins's methodological views and of his famous definition of economics on postwar axiomatic economics (Backhouse and Medema 2009). However, as those scholars also recognize, Robbins, although not opposed in principle to mathematical economics, was not an advocate.

[6] Note, however, that Hilbert's conviction that all mathematics can be reduced to axiomatic form was undermined by Kurt Godel's incompleteness theorems (Weintraub 2002).

questions about the relationship between mathematical knowledge and reality, and about how the former could successfully represent the latter. In the development of twentieth-century mathematics, these issues generated intense philosophical debates among mathematicians; they also shaped how mathematics would later be employed in economic analysis. In Debreu's own view, a pivotal role was played by von Neumann and Morgenstern's *Theory of Games and Economic Behaviour* (1944), which opened the way for axiomatic economic theory (Debreu 2008; Neumann and Morgenstern 1944).

If, in the late Thirties and early Forties, "classical" mathematical economics reached a peak through the use of differential calculus and linear algebra to address economic problems framed as optimization problems (see, for instance, the work of John Hicks, Paul Samuelson, and Maurice Allais), *Theory of Games* opened a new course by introducing logical rigor into economic reasoning and, at the same time, new mathematical tools, primarily convex analysis and algebraic topology. Convex analysis complemented real vector analysis, through separation theorems, as a standard technique for obtaining implicit prices. Moreover, one of the most famous examples of algebraic topology in economic theory is von Neumann's 1937 generalization of Brouwer's fixed-point theorem (Neumann 1945). Letting aside the later reception of Game Theory within the economics profession, von Neumann and Morgenstern's work occupies a pivotal place in the history of postwar economic theory. Indeed, theoretical economists in the Forties 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 and Morgenstern's work, a postwar generation of young economists, trained largely in U.S. economics departments and research foundations, assimilated new mathematical tools and encountered a new approach to mathematical economics and to economic theory.[7]

Game theory, also because of the role occupied by von Neumann (a mathematical genius deeply fascinated, at least in his earlier years, by Hilbert's formalist program), is among the most important examples of how the development of mathematics reshaped economic theory (Israel and Gasca 2009). Indeed, one hallmark of the "system of relations" image of economics is the emphasis on consistency at the expense of empirical truth. Such consistency can be demonstrated only by proving the absence of internal contradiction, in a manner closely analogous to mathematical proof.[8]

[7] An exception was the development of mathematical economics in the USSR, which followed a trajectory not dissimilar to that of the United States, though, unsurprisingly, its focus was not competitive markets but problems of decentralized production and planning. Nevertheless, from a mathematical standpoint, there was no technical difference.

[8] 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

Finally, the radical Bourbakist approach represented a further step. "Bourbaki" was the pseudonym adopted by a group of young French mathematicians beginning in the Thirties, whose aim was the complete refoundation of mathematics on a strict axiomatic basis. According to this collective, mathematics was not only a discipline concerned with formal axiomatic systems but, above all, an autonomous subject, separated from the outside world and from applications. In this sense, Bourbakism radicalized Hilbert's attempt to clarify the status of mathematical truth. Bourbakists aimed in particular to elaborate multivalent theories, achieving unity within mathematics by exploiting the full power of axiomatization. Philosophically, Bourbaki's mathematics also avoided many "foundational issues," thereby sidestepping the demise of Hilbert's formalist program following Kurt Godel's critiques (Weintraub and Mirowski 1994).

Bourbakism viewed mathematics as a storehouse of abstract forms, and this view, largely through Debreu, profoundly influenced mathematical economics in the second half of the twentieth century. Debreu's *Theory of Value* (1959) is perhaps the most striking example of Bourbakism in economics. In this short work, the French economist (who had personal connections to "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 the Bourbaki group, see 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 developed to ground economic theory. The action of an economic agent, for instance, can be described by an input vector and an output vector for each commodity, that is, by a point in commodity space (a finite-dimensional real vector space). A social system can be divided into different states, each described by listing the actions chosen by each agent. Each agent selects an optimal action given the actions of all others. Listing these reactions yields a new state, and thereby defines a transformation of the set of states of the social system into itself. A state of the system is an equilibrium if and only if it is a fixed point of that transformation. The construction of a formal model can thus appear as "an inexorable process in which rigor, generality, and simplicity are relentlessly pursued." Debreu summarized the approach 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

by contradiction (for instance, 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 kinds of mathematical proof, and the "intuitionists," who rejected non-constructive proofs. In economics, there has been a progressive abandonment of constructive proofs in favor of non-constructive ones (Giocoli 2003b; Leonard 2010).

> 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)

This radical image was soon challenged, after its emergence in the Fifties, by a more applied view of mathematical economics (Weintraub 2008). These challenges notwithstanding, the Bourbakist-axiomatic image remained central for decades in shaping how economic theory was produced. Economists have been socialized into it, most notably through advanced training such as Ph.D. courses, thereby providing a striking illustration of a persistent difference between economics and other social sciences.

1.1.2 Institutions and Communities

The interwar and postwar changes also reshaped, in profound ways, the institutional and social history of economics. At the same time, these transformations were facilitated by specific institutional arrangements. In a sense, the mathematization of economics required the development of specialized institutions, courses, and curricula in order to become effective. In turn, the emergence of such structures strengthened the position that mathematical economics came to occupy within the discipline.

Whereas the late nineteenth century witnessed the definitive institutionalization of economics through the creation of new faculties, economics departments, journals, and national associations, the years from the Thirties onward saw the rise of the United States as the most important location for economic research. This "Americanization" of economics was certainly accelerated by the rise of Nazism in Germany (and the annexation of Austria in 1938), after which a large number of scholars (not exclusively Jews) sought refuge in the United States (Leonard 2010; Hagemann 2011). For instance, in Ingrao and Israel's view, the development of modern General Economic Equilibrium theory represents perhaps the clearest example of how Nazism dramatically impoverished Europe's scientific culture, contributing to the subsequent, largely uncontested, supremacy of the United States (Ingrao and Israel 1987, p. 245). After the Second World War, the United States assumed the role previously occupied by Great Britain, but with a crucial difference: the increasing mathematization of economics substantially reduced the distinctiveness of national traditions (the "national schools"), replacing them with a unified methodological orientation, neo-classical economic theory grounded in the modeling of individual behavior through concepts such as preferences and endowments.[9]

To this development one must add the rise of highly sophisticated statistical methods, namely econometrics. Thus, despite the persistence of

[9] 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

internal differences in policy stances or even in technical matters[10], from the second postwar onward economics increasingly represented itself as a cohesive discipline, leaving little room for earlier "verbal" controversies such as those concerning the meaning of 'utility.'

This radical transformation unfolded, at least, in two distinct steps. The first involved re-framing standard economic problems in formal terms and elaborating (often creating) new theories, or reshaping existing ones—and new methods to address them (for example, the formalization of General Economic Equilibrium as a topological problem, or the development of linear programming). The second step, which followed, concerned the training and diffusion of these new theories and methods.

This second process became particularly important from the late Fifties onward, especially through projects promoted by institutions such as the Social Science Research Council.

The development of mathematical economics in the interwar years, particularly in the Thirties, was therefore facilitated by the establishment of institutions and associations, often initially animated by only a few scholars, devoted quite exclusively to these scientific endeavors and supported by private funding. One cannot understand the "scientific" turn in economics without considering institutions such as the Cowles Commission, the RAND Corporation, the Econometric Society, and the National Bureau of Economic Research (NBER), as well as key sites such as Princeton's Institute for Advanced Study. Yet, iron curtains did not isolated these institutions from each other. On the contrary, they were frequently intertwined, whether through shared affiliates or through overlapping funding sources.

The Cowles Commission for Research in Economics was founded in 1932 through the funding of Alfred Cowles III, a very wealthy investor and businessman interested in stock market forecasting. Despite this practical origin, with the assistance of the mathematician Harold T. Davis, Cowles involved a broad range of academic and professional economists. The Commission's agenda was soon expanded beyond business forecasting to include issues such as employment and the development of mathematical methods and quantitative analysis in economics more generally. From its inception, the Commission was closely connected to the Econometric Society (still the most prestigious association of mathematical economists within the discipline). Among its early academic supporters were figures such as Fisher, president of the Society, and Charles Roos, a mathematician who served as secretary-treasurer 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 creation was promoted chiefly by Roos, Fisher, and the Norwegian economist Ragnar Frisch (later the 1969 Nobel laureate).[11] Fisher served

[10] For instance, among econometricians, the proper relationship between theories and measurement (Dimand 2019). A point briefly addressed below.

[11] 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.

as its first president, while the Society's Council included nine members, American and European, drawn from both mathematics and economics.[12] In the Constitution of the Econometric Society , adopted at the first meeting (1930) and published in the first issue of the Society's journal, *Econometrica* (1933), the 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) The charter further stated explicitly that any activity intended to foster such unification between theoretical and quantitative studies fell within the Society's sphere of interest (ibidem). Thus, the Cowles Commission and the Econometric Society in parallel and, while formally distinct, frequently intersected. Alfred Cowles also financed the publication of *Econometrica* and served as treasurer and secretary of the association during the Thirties. As Robert Dimand notes, Cowles's role as a private benefactor of research in economic theory, mathematical economics, and econometrics in the Thirties United States is comparable to the better-known role of Alfred Loomis in American physics (Dimand 2019). Later, in the Forties and Sixties, the Commission also benefited from additional sources of funding, alongside Cowles's initial support, including the Rockefeller Foundation, the RAND Corporation, and the Office for Naval Research.

Initially, the Cowles Commission was based in Colorado Springs (CO), where it remained until 1940, before moving to Chicago (1940–1955) and later to Yale University, where it remains active as the Cowles Foundation for Research in Economics at Yale University. During the Colorado years, its principal research interest was the study of economic fluctuations and business cycles using advanced statistical tools. After the move to Chicago, where it was affiliated with the University of Chicago's Department of Economics, the Commission broadened its focus toward the foundations of economic theory. This shift was largely associated with the influence and leadership of the Russian Empire-born economist Jakob Marschak, director of Cowles from 1940 to 1948.[13] In a statement outlining the Commission's activities, Marschak emphasized the importance of developing statistical

[12] 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).

[13] Marschak (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 Arrow 1991; Hagemann 2011

techniques distinct from those typically employed in other empirical disciplines, as well as 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) Alongside the refinement of econometric techniques, Cowles also became an incubator for strictly mathematical treatments of theoretical economic problems in the late Forties and early Fifties. To that end, Marschak defended a shift away from an exclusively empirical and quantitative emphasis toward "the general theory of mathematical development." Among the theoretical strands that flourished at Cowles in this period were new formal approaches to General Economic Equilibrium, Social Choice Theory, and Rational Choice Theory.

In 1949, the Commission organized the well-known "Conference on Activity Analysis of Production and Allocation," whose importance, also from a historical and network, analysis perspective—should not be underestimated. As Düppe and Weintraub argue, "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 Weintraub 2014a, p. 454)

Finally, in the Fifties, much of the major progress in the Theory of Games was produced by mathematicians, primarily at Princeton University. Yet Marschak was among the first to provide a comprehensive review of von Neumann and Morgenstern's theory, and to offer critical discussions of expected utility and rational choice during the Fifties (Marschak 1946; Herfeld 2018).[14]

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). Its most notable accomplishment was the first estimate of U.S. national income in 1934, followed by the construction of other indices of economic activity, including GDP (Fogel et al. 2013). Although devoted to broadly similar research concerns, the approaches of NBER and Cowles diverged sharply, especially in the Forties and Fifties. The divergence stemmed from the prominence that Cowles assigned to economic theory, to the point that, in a famous exchange with NBER affiliates Arthur Burns and Rutledge Vining, the Cowles economist (and future 1975 Nobel laureate) Tjalling Koopmans famously characterized NBER's approach as "measurement without theory." The controversy also involved, at least implicitly, differences in perceived policy orientations, with NBER often associated with more conservative positions and Cowles more open to debates over economic planning (Dimand 2019; Levy and

[14] For enjoyable (although necessarily historically incomplete) internalist histories of the mathematical economics at Cowles, see: Arrow 1983; Debreu 1983

Peart 2020).

If the Cowles Commission remained primarily an academic institution (especially after its affiliation with Chicago, and later Yale), other research centers that played a crucial role in shaping postwar formal approaches to economics and the social sciences did not. The most famous and influential is certainly the RAND Corporation, "the Think Tank Icon of Cold War America." (Amadae 2003) Accordingly, RAND's close connections with the U.S. military apparatus have frequently been used—often in a derogatory tone—to emphasize the intimate relationship between mathematical social science and Cold War politics (Mirowski 2002; Erickson et al. 2015). RAND was founded in 1946 as a private research enterprise, partly funded by the U.S. Army Air Force and the Douglas Aircraft Company. Its mission was to conduct research and development for military purposes, as well as for civilian applications (a comprehensive, though controversial, history of RAND's early years is provided in Amadae 2003). For the purposes of this work, two points deserve emphasis. First, despite its practical orientation, RAND also financed theoretical research; among its affiliates, at different times, were pure mathematicians such as John Nash and Lloyd Shapley. Kenneth Arrow's Ph.D. research on social choice also benefited from RAND funding, as well as from the connections between RAND and the Cowles Commission (Arrow 1951b, p. ix; Arrow 1983). Second, and more than at Cowles (and in a different manner from Princeton's mathematics department), RAND researchers developed applied implications of game theory, particularly concerning optimal strategic behavior and its use in military and international political contexts (Leonard 2010).

Finally, some attention must be devoted to the role of specific academic departments. For the development of game theory, the central location was certainly Princeton's Department of Mathematics and, to a lesser extent, Princeton's Institute for Advanced Study.[15] John von Neumann obtained his U.S. academic affiliation at the IAS when he moved permanently to the United States in 1933 (though he spent a relatively small amount of time in New Jersey, especially after the beginning of World War II). The Institute itself had been established in the same year through the efforts of the progressive educator Abraham Flexner. Despite its intended interdisciplinarity, it is best known for its School of Mathematics, which hosted internationally renowned figures such as Albert Einstein, Hermann Weyl, Oswald Veblen, and Kurt Gödel, in addition to von Neumann.[16] Yet, well before the establishment of the Institute, Princeton University

[15] Given the scope of this work, the focus is on the intellectual development of game theory, and then no emphasis will be given to such otherwise essential places for the development of mathematical Economics like Paul Samuelson's MIT Economics Department (Backhouse 2017; Weintraub 2014).

[16] 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. Hirschman, the international relations scholar and former ambassador George Kennan, among the others.

already possessed a leading mathematics department, with strong interests in recent developments in pure mathematics—such as topology—and strong connections with continental European mathematicians and institutions, especially Göttingen, Hilbert's university.

Similarly, Oskar Morgenstern, after the Anschluss, obtained a faculty appointment in Princeton's Economics Department in 1938, where he spent most of his career (Leonard 2010). Thus, despite clear earlier influences from French and German-speaking mathematical traditions, game theory was, in an important sense, a product of Princeton. Some of its applications were extensively studied at RAND, and Cowles also devoted attention to it, even if Cowles affiliates soon shifted their focus toward the broader framework of general equilibrium. However, it was at Princeton in the Fifties—after Cowles theoreticians largely lost interest, and in parallel with RAND activity—that some of the most important results in game theory, and the foundations for later developments, were produced.

The most prominent example is the concept of Nash equilibrium, the topic of John Nash's Ph.D. dissertation in mathematics (1950). Among the scholars affiliated with Princeton's mathematics department who made pivotal contributions to game theory (and beyond) were Albert Tucker, Harold Kuhn, Lloyd Shapley, Nash, Sam Karlin, and David Gale.

Martin Shubik offered a first-person account of Princeton as a birthplace of Game Theory in the Fifties. Although enrolled as a graduate student in economics, Shubik developed close connections with Nash and Shapley (Shubik 1992) and he was among the few economists in the Fifties who expressed a genuine interest in the Theory of Games and were willing to employ it. In his account, Shubik emphasized the differences he perceived between the economics department, where he was formally affiliated, and the mathematics department. In economics, despite the presence of renowned theorists such as William J. Baumol and Jacob Viner and a broadly favorable attitude toward mathematical economics (especially on Baumol's part), Game Theory attracted remarkably little interest. As Shubik remarked:

> "[. . .] [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 and others, this new mathematical bag of tricks was of little relevance to economics.[17] 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

[17] An example is Schumpeter's attitude, as reported in Giocoli 2003b, 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 Thirties (Backhouse 2017; Swedberg 1991).

> it be of use in the study of economic life, which is considerably more complex than chess." (Shubik 1992, p. 152)

More generally, the attitude of scholars such as Viner toward game theory resembled that of other senior figures (Viner was born in 1892), even when they were not hostile to the mathematization of economics as such. In Shubik's narrative, moreover, the difference extended beyond Game Theory and reflected a broader contrast in the intellectual orientation of the two departments. 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, the institutions and sites discussed above, namely, the Cowles Commission, the RAND Corporation, and Princeton's mathematics department, were among the most crucial settings (though not the only ones) in which the dramatic changes in economic theory, briefly outlined in the opening paragraph, took place. Yet, as noted, these developments were not the final stage but rather an early phase in the broader disciplinary transformation of economics. A further step was needed: the effective "conquest" of the discipline, that is, the broad adoption of these approaches by most scholars, students, and instructors.

This process was largely successful in economics. The situation was different in the other social sciences. There, the entry of quantitative analysis and statistical methods was comparatively successful and followed a path partly analogous to economics, though in different terms, especially in the second stage, owing to the role of the SSRC and the board for mathematical social sciences. However, a revolution in theory-building comparable to postwar neo-classical microeconomics (for instance, axiomatic general equilibrium) and to Game Theory did not occur. The closest attempt, yet far less successful than in economics, was that of Riker.

1.2 The Development of Political Science

1.2.1 Early debates on Political Science

Political science emerged in the late nineteenth and early twentieth centuries as a specialized field of study related to, but distinct from, disciplines such as constitutional law, history, and political philosophy. In particular, its

link with historical study was strong both intellectually and institutionally. For example, the American Political Science Association emerged from the American Historical Association. Moreover, until the Thirties, the overlap between history and political science remained substantial. In countries other than the United States, this close affiliation persisted into the second postwar period (Farr 2009).

Methodologically, history was regarded, until the Thirties and Forties, as the principal field through which political conceptions could be observed and tested. The obvious consequence was that political analysis remained inseparable from normative and prescriptive implications. Although anecdotal, evidence of this connection lies in the wide circulation (in both the United States and England) of the Oxford historian Edward A. Freeman's aphorism: "History is past Politics, and Politics is present History." This sentence became the motto of the Johns Hopkins University Studies in Historical and Political Science (Farr 2009, p. 68; Ross 2009).[18] This intertwining of political and historical analysis reflected the fact that the former was closely tied to liberal politics, through narratives of the historical development of modern polities often framed in evolutionary terms, with "whiggism" (in its political and historical sense) as a central tenet (Ross 2009). It also suggests that the discipline's institutional trajectory shaped its intellectual production, reinforcing a persistent tension between normative and positive theory (Adcock and Bevir 2010).

Within this section's deliberately simplified narrative, a few broad—and often intertwined—dichotomies can be advanced to delineate the development of "pre-behavioralist" political science. These include the (apparent) opposition between historical and empirical research, between normative and positive theory, and finally between competing conceptions of the proper object of inquiry, statism or pluralism. These distinctions remain very general, and in practice they frequently overlap.

The first dichotomy is not, strictly speaking, an opposition. By empiricism, one should not think primarily of sophisticated debates in the philosophy of science, but rather of a broader appeal to the "scientific method." Despite the prevalence of historicism, early political science was not devoid of methodological discussion concerning its scientific status, that is, what "science" could mean in the analysis of political phenomena. The emergence of political science was indeed conceived by its early proponents (including Lord Bryce, Lawrence Lowell, and others), both in the United States and Great Britain, as a "scientific" project whose model was the natural sciences. Consequently, central issues included the impartiality of the scholar—at least in a broad sense compatible with the normative implications of much work—and questions of "method." Method, in turn, encompassed the application to the moral sciences of the empirical approach articulated by John Stuart Mill in his *System of Logic* (1843), as well as

[18] Another anecdote is in the reminiscences of the political scientist Charles Hyneman, whose mentor at Indiana University in the Twenties, 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)

the Rankean historical method based on archival research.

The influence of more sophisticated versions of positivism was limited, though not negligible. There were debates about how impartiality and objectivity might be secured through quantitative methods and statistical tools. In the early twentieth century, the growing use of such techniques—still rudimentary—became a distinctive feature of American political science. Yet significant exceptions existed, such as the British scholar Graham Wallas, a professor at the London School of Economics and Political Science.[19]

In discussing the method of political reasoning, Wallas explicitly rejected a priori deductive approaches grounded in "personifications and uniformities" of political abstractions and types, and in "large and untried generalizations," and defended instead the use of quantitative analysis (Wallas 1920, pp. 138–9). Wallas's case is instructive. In his major work, *Human Nature in Politics* (1908), he developed a critique of the "intellectualist fallacy" associated with a priori deduction, which, in his view, overstated the power of human reason and neglected psychological features such as "impulses and instincts." His argument drew on psychological traditions (notably William James and the "Psychologie de la foule" theorists such as Gabriel Tarde and Gustave Le Bon) as well as on evolutionary reasoning (following Darwin).

Although elements of Wallas's discussion are anecdotal (not least given his own inclination toward political engagement), the core of his argument is systematic. Against the long-standing tendency in political analysis to search for "[. . .] '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 proposed a method aimed at "finding as many relevant and measurable facts about human nature as possible," and making them serviceable for political reasoning.

This method, explicitly compared to that of the biologist, is threefold: first, the description of such facts; second, their quantitative analysis; and finally, the analysis—both descriptive and quantitative—of the environment into which individuals are born, and of its effects on behavior. For Wallas, descriptive analysis remained inseparable from psychological considerations. For this reason, he criticized works such as those of Moises Ostrogorski and Lord Bryce, arguing that both authors attempted to fit their observations of political reality in the United States and Great Britain to preferred conceptions of "ideal political men." He went so far as to claim 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)

Wallas grounded the role of quantitative methods by connecting them, first, to recent developments in "mathematical biology," namely biometrics, drawing on Karl Pearson's contributions to statistical analysis (Wallas 1920, p. 132), and, second, to developments in contemporary economics. In the

[19] 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

latter case, Wallas attached particular importance to the shift "from the abstract deductions about ideal economic men" (which he associated with classical political economy and linked to Walter Bagehot) toward a method based "upon the variety and not the uniformity of individual instances," which he associated with William Stanley Jevons and Alfred Marshall. Jevons, in particular, "[. . .] 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) For Wallas, this approach was no longer purely abstract, because it mirrored the process through which real individuals reached practical outcomes; moreover, it could be extended by statistical analysis.

Wallas's discussion of economics may appear at first glance to sit uneasily with his earlier emphasis on the psychological dimensions of human nature. Indeed, neo-classical economics is often criticized for excessive a priorism, abstraction, and deductivism in its account of individual behavior. Yet Wallas's focus was on the quantitative relations involved in political issues of very different kinds, from public finance to legislative politics. Thus, although his discussion is framed in an unmistakably marginalist language, it reflects, in my view, his adoption of a "psychological" interpretation of marginalism of the kind common among contemporary British economists.[20] For this reason, it would be misleading to treat Wallas as a precursor of an "economic approach" to political science. Nevertheless, his analysis anticipates several distinctive features of subsequent developments. Accordingly, despite being largely unsuccessful in Britain, his quantitative and "psychological" orientation became one of the trademarks of proto-behavioralist analysis (Merriam 1923; Adcock and Bevir 2010).

Methodological debates also encompassed disputes over the proper normative or positive content of political science. Political science, understood as a scientific study of politics and society, was also regarded as a tool either to defend the existing order (for instance, nineteenth-century British liberal institutions) or to advance reform programs. Many prominent scholars were themselves politically involved (as in the cases of Bryce, Ostrogorski, Wallas, and Charles Merriam, among others). Moreover, although the classical Weberian account of a "vocational" stance toward science entered the American scholarly community mainly in the second postwar period, questions concerning impartiality were already widely discussed, together with the appropriate balance between political engagement and political analysis.[21]

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

[21] 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)

For example, in a review of recent advances in political method, Charles Merriam, founder of the so-called "Chicago School of Political Science" (see below), explicitly distinguished between two 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, in his view, began only with the work of Francis Lieber. The first attitude belongs to political actors, the second to scholars; the two may occasionally coexist in the same individual, but they must remain distinct (Merriam 1923).

A further perspective on the early state of the discipline, its methodological concerns, and its central objects of inquiry is provided by Lawrence Lowell's presidential address to the "American Political Science Association" in 1910 (Lowell 1910).[22] According to Lowell, political studies suffered chiefly from a lack of precision and from the "imperfect development of the means of self-expansion." Although different aspects of political and social life could in principle be identified with precision, their fundamental importance was often neglected. This was particularly true, he argued, for political science, which he defined as the "physiology of politics," that is, the study of how government actually works. Accordingly, debates 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) At the same time, Lowell did not reject a normative purpose, namely the support of reform movements. In his view, only the scientific study of politics could provide the knowledge necessary for effective 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)

With respect to the object of analysis, in the years before the First World War and throughout the interwar period, American political science was shaped by two broad tendencies, commonly labeled "statism" and "pluralism." Here, too, the distinction should not be overstated: the field

[22] 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

was not organized around rigid partitions or exclusive analytical frames. Topics such as political parties, electoral mechanisms, voting procedures, and related issues were already explored to some degree (for an extensive list of relevant problems, see Lowell 1910, pp. 11–3). Still, the contrast between "statism" and "pluralism" reflects, on the one hand, the discipline's inheritance from constitutional law and, on the other hand, its growing interaction with adjacent social sciences. In this sense, neither statism nor pluralism constituted a unified intellectual movement with a single manifesto; rather, they were intellectual tendencies, articulated through multiple texts, often sharing broad aims and, at times, a common political agenda.

Statism was closely connected to a conception of the state derived from European legal and political philosophy, especially German *Staatwissenschaft*. During the so-called Progressive Era, this conception was developed within American political thought through historical analysis and empirical classification, often with a strongly normative purpose: to help establish a rationally organized state and political community. At times, political science aligned with progressive agendas on issues such as race, immigration, women's labor, eugenics, and others (Leonard 2016).

In reaction to what was perceived as the monism of statism, some authors, such as Arthur Bentley and Charles Beard, increasingly emphasized the role of groups and organizations with divergent interests. The existence of such groups suggested that the state was, in practice, a plural polity. With pluralist theories, the relation between normative and positive analysis took on new forms. For instance, scholars such as the British Harold Laski (prior to his conversion to Marxism in the Thirties) viewed pluralist analysis as a way to defend and justify social differentiation against the unifying aspirations of the modern state.[23]

At the same time, debates about pluralism encouraged some authors, while maintaining a normative role for the state as the locus of liberal democracy and as an instrument for solving social problems, to develop more systematic approaches to the study of group politics.

A particularly influential role was played by the so-called "Chicago School of Political Science," which developed from the Twenties to the Fifties around the central figure of Charles Merriam. Merriam's importance in shaping both the Chicago School and broader developments in political science cannot be overstated. In the Twenties, his contributions included methodological and historical reflections on the state of the discipline, the new direction of Chicago's political science department, and the creation of institutions and scholarly associations, notably the "Social Science Research Council," of which Merriam was the first chairman (1923), as well as the first National Conference on the Science of Politics (Merriam 1923). In 1926, Merriam also served as president of the American Political Science Association.

At Chicago, a group of scholars influenced by Merriam produced seminal

[23] On Laski's "normative pluralism" influence in American scholarship, see: Dryzek 2006; Gunnell 1995

work on voting behavior, African American politics, political psychology, urban politics, comparative politics, political parties, methodology, and related topics (Heaney and Hansen 2006). These studies employed advanced empirical techniques, including survey research, content analysis, field experiments, factor analysis, and innovative combinations of quantitative and qualitative methods. Chicago graduates later became among the principal forerunners of the Behavioral Revolution in the second postwar period, including V.O. Key Jr, Harold D. Lasswell, Gabriel Almond, Herbert Simon, and David B. Truman.

The distinctiveness of the Chicago approach can be further captured by a recurring theme in the oral histories of influential American political scientists: the contrast between Chicago and Harvard as alternative models of the scientific study of politics (Baer, Jewell, and Sigelman 1991). These were not the only graduate programs in political science during the interwar period. Yet, in many later narratives about the discipline's development, they came to symbolize two different orientations toward political inquiry.

On the one hand stood Chicago's social science milieu, shaped by Merriam and by scholars such as Harold Lasswell, Leonard White, and Harold Gosnell, all of whom had been Merriam's students. On the other stood Harvard's School of Government, commonly associated with a stronger historical and theoretical orientation, represented by figures such as the historian of political thought Charles McIlwain and the German scholar Carl Joachim Friedrich. A particularly clear reminiscence is offered by David Easton, who completed his Ph.D. at Harvard in the Forties but later obtained 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) At the same time, "you could not graduate from the program without being sensitive to the importance of theory in political research." (ibidem) Harvard shaped Easton's orientation toward theory grounded in empirical concerns; yet, in his own perception, it did not provide training in what he understood as political science. Easton ultimately remarked: "By the time I left Harvard, I just didn't know what political science was all about." (Baer, Jewell, and Sigelman 1991, p. 199)

By contrast, Easton described Chicago as "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." (Baer, Jewell, and Sigelman 1991, p. 201) Easton arrived in Chicago in the early Fifties, when, according to several accounts, many of the most distinctive features of the School were already beginning to fade, following Merriam's death and the departure of many of his students, who were replaced by scholars with markedly different orientations such as Hans Morgenthau and Leo Strauss (Heaney and Hansen 2006). Nevertheless, this decline coincided with the nationwide diffusion of many Chicago-style

themes and practices and with the Behavioral Revolution in political science, of which Merriam was undoubtedly among the principal forerunners.

1.2.2 The Behavioral Revolution

In the Thirties and Forties, American political scientists brought to completion the "epistemic shift" that had begun in the late nineteenth and early twentieth centuries. A further step occurred in the Fifties with the genesis and development of the "Behavioral Revolution." The growth of empirical work that distinguished interwar American political science provides an essential starting point for understanding the subsequent Behavioral Revolution. Behavioralism's transformative aspirations lay in the departures it prescribed in order to make political science more systematic. Political scientist Robert Dahl defined Behavioralism as a "protest movement" within the discipline, carried forward by scholars dissatisfied with the historical and juridical approach and seeking a more scientific analysis (Dahl 1961). Such a commitment was already present in authors such as Wallas and in the Chicago approach, and American political science began a significant shift in its disciplinary orientation and methodological commitments in the Forties. In this sense, Behavioralism can be interpreted as the culmination of a longer and more complex development, as shown in the previous section. However, it also introduced genuinely innovative elements, to the point that some practitioners described it as an instance of "Kuhnian revolution" (Truman 1965; Almond 1966). Although this view has been contested by contemporaries as well as later practitioners and historians of political science, it remains the case that leading proponents of the new approach were inclined to emphasize difference and to define themselves in explicitly revolutionary terms.[24]

The following pages do not aim to describe or contend with all these interpretations in detail. Rather, they will highlight some of the main features of Behavioralism, relying primarily on how its practitioners perceived it. The focus will be on the role of theory in Behavioralism.

In a well-known paper on the origins and characteristics of Behaviouralism, Robert Dahl pointed out one of its most important features, namely the absence of 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." (Dahl 1961, p. 249) Furthermore, according to another behavioralist, 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, for Dahl, Behavioralism can be understood 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,

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

conventions, and assumptions of modern empirical science." (Dahl 1961, p. 256)

It is evident that such a definition is very broad, potentially encompassing almost any attempt to understand politics through empirical theories and approaches. Dahl further noted: "Those who were sometimes called 'behavioralist' [...] shared a mood: a mood of skepticism about the current intellectual attainments of political science, a mood of sympathy toward 'scientific' modes of investigation and analysis, a mood of optimism about the possibilities of improving the study of politics." (Dahl 1961, p. 255) He therefore concluded that " [...] 'the behavioral approach' might better be called the 'behavioral mood' or perhaps even the 'scientific outlook.'"(Dahl 1961, 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 common vision of a new political science. This vision stood in contrast not only to that articulated by figures such as Hans Morgenthau, Leo Strauss, or Eric Voegelin (often European by origin), but also to that of political scientists satisfied with the existing discipline (Adcock 2009, p. 188).

Dahl identified six factors behind the behavioral approach in American political science, all rooted in specific features of American culture. First, the role of Merriam and the influence of European emigration. Second, the Second World War, which pushed many scholars to participate in administrative and planning institutions. A third, and, in Dahl's view, even more significant, impetus, also related to the war, came from the Social Science Research Council, whose presidency was assumed by the political scientist Edward Pendleton Herring. In particular, Herring helped create the Committee on Political Behavior in the Social Science Research Council. This committee emphasized individuals' behavior in political situations by examining political relationships among individuals, with the aim of formulating and testing hypotheses about behavioral uniformities across different institutional settings. The remaining factors were the 1949 conference on political behavior, which connected several groups of scholars committed to these issues; the rapid development of survey methods and statistics, which increasingly presented themselves as "scientific"; and finally, the influence of institutions and private foundations such as Carnegie, Rockefeller, and Ford.

Political behavior became a central object of research in the Fifties. Although political scientists' attention to behavior pre-dated that decade, it was only then that it assumed a dominant place, aided by the consolidation of quantitative analytical techniques.[25] In this context, Behavioralism can be treated as a comprehensive approach that combined a research focus on political behavior, a methodological plea for science, a political message

[25] Up to the point that in a "state of the discipline" volume put together by the American Political Science Association in the Forties it was affirmed that political behavior had largely replaced legal structures as the cardinal point of emphasis among political scientists (Griffith 1948).

centered on liberal pluralism, and the organizing concept of a political system. Behavioralists emphasized individuals and groups and, according to some historians, can even be described as a relatively coherent group, with founding texts and a quasi-manifesto, such as Easton's *The Political System* (Easton 1953; Garceau 1951). Yet, as Easton stressed, the movement did not appear clearly defined to those who were labeled "Behavioralists." As he explained: "[...] 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 behavior. Others would define it as an excessive emphasis upon quantification. Others as individualistic reductionism." (Baer, Jewell, and Sigelman 1991, p. 207)

Despite this definitional ambiguity, a shared sense of Behavioralism as a transformative enterprise was common among its proponents—a "rallying cry to promote change." In this sense, the label functioned less as a description of an already accomplished intellectual shift than as an instrument intended to produce one. This point was explicitly recognized by Riker, who, in the Fifties, never joined the movement, while nonetheless sharing several of its critical impulses (Riker 1997).

In one of the early "post-behavioral" reconstructions of the discipline, Albert Somit and Joseph Tanenhaus listed a set of features that they described as the "key behavioralists articles of faith" (Somit and Tanenhaus 1967, pp. 177–9). These included an emphasis on prediction and explanation grounded in observation and data collection; the pursuit of interdisciplinarity and "self-conscious criticism" of methods and results; and an orientation toward "pure research," leaving aside normative aspirations to establish the "truth or falsity of values" such as democracy, freedom, or equality, which were regarded as beyond scientific validation. Theoretical development also played a central role in orienting and directing research. Behavioralists believed that systematic science depends on the cumulative interplay between theoretical innovation and empirical inquiry, and they set out to reshape both sides of this interplay. Therefore, according to Robert Adcock, the behavioralist agenda for disciplinary transformation comprised two strands. The first pursued an "agenda of empirical theory," in which empirical validation is embedded within theoretical development. Easton's work is perhaps the most important example of this strand. It also included works such as Lasswell and Abraham Kaplan's *Power and Society*, and Truman's *The Governmental Process* (Lasswell and Kaplan 1950; Truman 1951).

The second strand focused on the development and use of more sophisticated empirical research techniques. It involved a shift from a relatively "low-key" empiricism, with no strong commitment to quantification, toward a more methodologically ambitious approach. In doing so, it reshaped what it meant to be "scientific" in political science, namely, to be "systematic." This strand proved more successful than the attempt to build a cumulative and unifying political theory. It encompassed methods and analytical tools, often borrowed from disciplines such as sociology and psychology, for in-

stance, survey research applied to the study of voting behavior and public opinion (Dahl 2005; Campbell et al. 1980).

As noted above, the behavioral movement remains central to how political scientists conceptualize their discipline's past. For Adcock, Behavioralism was revolutionary in character and impact, but many changes were neither immediate nor as radical as simplified narratives suggest. For example, Dahl's emphasis on the empirical character of Behavioralism can be misleading. It obscures the importance attributed to theoretical analysis and the fact that empirical research existed well before the Fifties. More fundamentally, the core of the movement concerned systematization—how to study politics systematically—rather than a change in object of inquiry. In this sense, the most transformative aspects of Behavioralism lay in its call for a new kind of theoretical work and for more advanced empirical techniques. The movement's "revolutionary" character thus reduces, in large part, to the adoption of novel and more sophisticated research techniques (Adcock 2009). Yet, interpreting the Behavioral Revolution primarily through technical innovation risks obscuring the movement's more complicated—and often disappointing—impact on political theory. Behavioralism's conception of political theory rested on using "self-conscious" abstraction to produce analytical frameworks and generate scientific progress; empirical research was essential insofar as it was considered the key to cumulative advancement. However, the results were frequently judged unsatisfactory: no foundational general theory emerged, and neither a unified methodological nor a unified theoretical framework came to be accepted by most practitioners.

1.2.3 Political Theory and Political Science

David Easton's *The Political System* (1953) has been seen by some as the "manifesto" of the new intellectual tendencies toward exact reasoning and empirical analysis in political science during the Fifties (Dryzek 2006). However, Behavioralism does not occupy a large space in this work. Rather than offering a new comprehensive political theory or presenting novel methods for political analysis, Easton provided a detailed discussion of "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)

Accordingly, his analysis is filled with discussions about the history of the development of political science and about scholars' attitudes toward scientific debate. For instance, one of the most famous chapters of the book addresses "the decline of modern political theory" (Easton 1953, 233 et ss.). Easton attributed this decline to historicism, by which he meant the almost exclusive interest in the history of political doctrines that characterized American political theory (especially the work of Dunning, McIlwain, and George Sabine).[26]

Consequently, the "value aspects" of political theory, as well as its empirical orientation, were overshadowed by historical considerations about political ideas, with the result that this kind of political theory proved scarcely able to yield a valuable understanding of political realities. Clarifying the relationship between the "value aspects" of theory and their empirical basis lies at the core of Easton's 1953 work and of the subsequent development of his analysis.

Easton began by acknowledging the disappointment surrounding political science, ("a discipline already twenty-five hundreds of years old" [sic]) due to its failure to clarify the 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 argument in politics was anything but unfavorable.

His concerns about theoretical developments in political science originated not from a possible "empirical drift" of social science away from theorizing, but rather from "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 this as a "flight from scientific reason." (Easton 1953, p. 6) Thus, 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 in which this anti-scientific attitude manifested itself. The first was a movement away from a rational attitude toward life and toward dispositions such as emotion, faith, or tradition. The second was an increasing tendency to develop critical arguments against scientific methods. He associated the first attitude with thinkers such as the English writer and political theorist Michael Oakeshott. According to the latter, any rational approach to understanding politics separated from the "act of politics" was useless, because reason was not merely a set of tools or techniques, but also embodied the "greater wisdom of prejudice, tradition and accumulated experience knowable largely through history." (Easton 1953, p. 19)

From these views derived a tendency to diagnose a crisis in the methods of the social sciences and to blame the scientific attitude for the failures to understand contemporary political facts. Unlike the repudiation of reason in the name of faith and tradition, these discussions focused on the alleged failure of the scientific method in the social sciences. Indeed, despite the efforts made by many scholars to define scientific reasoning in political

[26] This chapter is a reprise of a previous Easton's paper. Easton 1951

inquiry, many other social scientists and philosophers remained skeptical about the social sciences, their methods, and their scope.[27] Easton did not reject such criticisms altogether; rather, he rejected their most radical conclusion, namely the alleged impossibility of a reason-based analysis in political science.

In his view, the development of political science as a discipline had "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" over the development of verifiable theories (Easton 1953, p. 37). He famously labeled this tendency "hyperfactualism" (Easton 1953, 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." (Easton 1953, p. 89)

One of the most troubling aspects of the discipline's development, according to Easton, was the imprecision with which even its most critical concepts were defined, making it difficult for scholars to judge adequately between conflicting statements. To overcome this problem, he called for 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." (Easton 1953, p. 46) An explicit conception of scientific verifiability, and of the possibility of cumulative knowledge, was therefore central to his program.

This "empirical" stance did not undermine the importance of theoretical orientation. By "theory," Easton did not mean normative judgments about values, customary in political philosophy (which he termed "value theory"), but rather attempts to identify relations among political facts. In this sense, he spoke of "causal theory." (Easton 1953, 52 et ss.)

The two are not opposed: one is implicated in the other, and the distinction between them concerns primarily their internal logic. "Causal theory" can be regarded as a proxy for the stage of development of any science, insofar as it enables cumulative knowledge through increasing insight into relationships among facts. However, given the complexity of social life, defining a "fact" itself presupposes theoretical reasoning. Facts therefore cannot be separated from theoretical assumptions about their definition: "A fact is a particular ordering of reality in terms of a theoretical interest." (Easton 1953, p. 53) This point is relevant because Easton's position parallels, in part, arguments advanced by scholars who denied a strict "distinction between facts and values," and thus questioned the possibility of a purely objective social science, such as Leo Strauss and his followers. Easton's position, however, was not to reject the scientific project

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

in toto, but to defend it against what he regarded as its most damaging implications.

For Easton, the development of theory is central to the establishment of political science because "pure technical refinements", for instance, improved data collection or advances in 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." (Easton 1953, p. 63)

Easton identified three domains of problems confronting theoretical research: the basic concepts needed to orient political inquiry, the categories of data that must be considered, and the role of value judgment in theory construction. By "orienting concepts," he meant those concepts that allow one to distinguish political science from other social sciences (such as economics, sociology, or psychology). Perhaps the most significant contribution of the book falls within this category: the idea of a "political system" (pp. 96 et ss.).[28] Under this label, Easton included "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 earlier theories grounded in overly simple conceptions of "State" or "power." Closely related is his famous definition of politics as "the authoritative allocation of values for a society." (Easton 1953, 29 et ss.), which he presented as a "convenient and rough approximation to a set of orienting concepts" for political analysis. Moreover, this definition opens the way to the task of specifying the meanings of concepts such as "policy," "authority," and "society."

The construction of theory also requires discussion of the types of data that must be examined. In the development of American political science, Easton distinguished between approaches emphasizing institutional aspects and those emphasizing psychological issues. Within the "institutionalist group," he further differentiated between scholars focusing on "governmental institutions" and those emphasizing non-governmental groups. The "behavioral" approach, by contrast, focused on "political behavior" and, in his view, represented the genuinely novel route toward political science as a systematic discipline.

Easton's third problem concerned the "moral foundations of theoretical research," namely the relation between objective facts and value judgments. For Easton, the impossibility of political research wholly free from values called for a thorough exploration of the "moral premises" at the core of scientific inquiry. This applied especially to political science, given its ethical inspiration: "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 twentieth century and the second postwar period had witnessed a rejection of the positivist faith in the possibility of total moral neutrality. Yet, for Easton, "the mere statement [...] that

[28] 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)

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." (Easton 1953, p. 225)

Such influence, in his view, does not provide the criterion for evaluating scientific work, which instead depends on correspondence with reality. Nonetheless, since the relation between facts and values is inextricable, the task of social scientists is to improve the reliability of knowledge by making underlying values explicit, as a "moral prelude to our main empirical theme." (Easton 1953, p. 228) This requires what he termed 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 does not consist only in formally stating relevant values; it also involves "the positive task of constructing an image of the political system flowing from these moral premises." (Easton 1953, p. 231) In this sense, Easton proposed not merely an appeal for transparency about values, but an attempt at constructive synthesis between values and facts, aimed at clarifying their full meaning.

The only discernible suggestion of a broad theoretical framework on the horizon of empirical research is, according to Easton, the "theory of political equilibrium." With this idea, he referred to two modes of analysis: first, a way to understand processes within a political system (what he called "general equilibrium"); second, a way to describe the system ("constitutional equilibrium"). Because general equilibrium, in his view, implicated both normative and descriptive issues, the key difference was that constitutional equilibrium could be defined more straightforwardly as an equilibrium determined by institutions, whereas general equilibrium concerned an equilibrium among political parties and actors. Thus, in Easton's words, "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, by contrast, entailed two distinct ideas: the interdependence of all elements within a political system (already subsumed in the concept of "political system"), and the tendency of these elements to act and react upon one another until reaching a point of stability. If the first idea was not wholly novel (Easton traced its presence to the rise of pluralism and its emphasis on the multiplicity of social forces, especially groups), the second was oriented toward developing far-reaching conclusions in political analysis, including normative conclusions about outcomes within a given political system.

The main difficulty of these concepts lay in the entanglement of normative and descriptive premises and aspirations. From a descriptive standpoint, the question was whether the concept of general equilibrium could be developed into a full-fledged conceptual framework for political inquiry, comparable to its role in economics (even though Easton recognized that in economics the concept functioned primarily as an analytical tool rather than a "substantive description" of the system:(Easton 1953, p. 274). An adequate model of general equilibrium would require consistent quantitative

analysis as well as "mental operations" to reconstruct possible relations among interdependent political variables, in a manner analogous to economics.

Despite these difficulties, Easton argued that "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." (Easton 1953, p. 306) By contrast, the advantage of "constitutional equilibrium" lay in its relative ease of use as a descriptive tool for characterizing political systems in terms of institutional arrangements.

In conclusion, Easton warned that a divide was emerging between different conceptions of political theory, namely between "political theory" and the "empirical part of political science." The way to overcome this division was the pursuit of "reliable knowledge" about political life. But such 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." (Easton 1953, pp. 317–8)

Easton's pages contain perhaps one of the most substantial discussions of the place of political theory in modern political science written by a scholar who, while pursuing originality, remained committed to the new empirical and quantitative developments of the discipline. This does not exhaust the issue of theory's place within the scientific orientation of political science—neither historically (for instance, debates also concerned the development of "comparative politics," Adcock and Bevir 2010; Almond 1966) nor methodologically and philosophically.[29] Nonetheless, Easton's work can be read as capturing a broader mood within the behavioral movement. Behavioralists were not strictly a-theoretical; rather, they favored developing a positive political theory. The main difference with respect to political theorists *à la* Strauss was their acceptance of empirical methods. Moreover, the difference between behavioralists and those authors (starting from Riker) who adopted the label "Positive Political Theory" lies primarily in their commitment to different analytical tools for political theory and political analysis.

1.2.4 The SSRC and the mathematical social sciences in the second postwar

As noted above, some scholars, most notably Dahl, have attributed considerable importance to the role played by the Social Science Research

[29] 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.

Council in shaping second postwar American political science, particularly emphasizing the executive leadership of the political scientist Pendleton Herring, who became its chairman in 1948 (Baer, Jewell, and Sigelman 1991).

The Council was established in 1923 by a heterogeneous group of academics and intellectuals affiliated with the American Political Science Association, the American Sociological Society, the American Economic Association, and the American Historical Association, and later joined by representatives of the American Statistical Association, the American Psychological Association, and the American Anthropological Association. The Council's purpose was to foster an institutional environment conducive to the development of systematic social science and to encourage cross-fertilization among disciplines, explicitly aiming to generate new insights into contemporary social problems. This ambition was primarily pursued through the improvement of research methods.

This aspiration was closely intertwined with the intellectual project of Merriam and the Chicago school of political science. As noted above, Merriam was among the founders of the Social Science Research Council. As explicitly stated by figures such as the economist Wesley C. Mitchell, the council endorsed a "union-of-science viewpoint," rejecting both conservative approaches to the social sciences and rigid distinctions between social and natural sciences. From this perspective, scholarly research was to be analytically separated from moral, social, and political goals, while at the same time acknowledging that contingent social and political problems provided essential inputs for social scientific inquiry.

The Second World War marked a decisive turning point in the history of American science. During and after the war, annual federal support for scientific research increased dramatically, reaching approximately 500 million dollars per year. Moreover, the second postwar period witnessed the establishment of the National Science Foundation as the primary federal funding agency for science education in the United States.[30]

With regard to the social sciences, the Council continued to pursue its dual commitment to interdisciplinarity and methodological refinement, although its activities became increasingly specialized and detailed.

Pendleton Herring articulated this vision of the social sciences in the inaugural issue of the Council official journal, *Items*, published in 1947. In this essay, Herring addressed fundamental questions concerning the nature of social science, the responsibilities of social scientists, their role in modern society, and the requirements for their further development. He adopted a deliberately general tone, speaking on behalf of the social sciences as a whole rather than distinguishing among individual disciplines. According to Herring, the social sciences "represent the approach to human

[30] The history of the often conflictual relationship between natural and social scientists over the role of the latter within the National Science Foundation has been the subject of detailed historical reconstruction. This history also includes internal debates among social scientists within the Social Science Research Council concerning the opportunities and risks associated with federal funding, particularly with respect to academic freedom. (See Solovey 2013, 27 et ss.).

relationship that emphasizes analysis rather than force" (Herring 1947, p. 2, italics in the original). Their importance thus lay in their capacity to enhance understanding of social forces and human relationships, enabling societies to confront real-world problems. Social sciences were not expected to deliver "early solutions for such an enormous range of problems," but rather 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" (Herring 1947, p. 3). To fulfill this role, Herring emphasized the need for conceptual clarification and "careful selection and rigorous training" of social scientists. Their task was not "to determine public purposes or humanistic objectives," yet "the work of social scientists can make great contributions to the commonweal" (Herring 1947, p. 4). At the same time, Herring explicitly acknowledged a central risk associated with these scientific ambitions, namely "the danger that social science, by perfecting manipulative skills, can be turned to anti-social purposes in the hands of unscrupulous leaders" (ibid.). Consequently, he stressed the necessity of reciprocal interaction and transparency between social scientists and the broader public. He concluded by outlining eight broad objectives for the "growing drive for sound scientific practice in the field of human relations" (Herring 1947, p. 6): (1) identifying the distinctive contributions of the social sciences; (2) encouraging scientific inquiry; (3) promoting acceptance of the social sciences among scholars, public authorities, and the general public; (4) fostering cooperation within and across disciplines; (5) focusing on methodological research problems; (6) applying social science to practical social problems ("social engineering"); (7) improving the training of social scientists; and (8) securing governmental subsidies, private support, and academic recognition.

The final objective was clearly connected to ongoing debates over federal funding. The preceding points, however, exemplify a broader postwar American attitude toward the social sciences that some authors have criticized as "scientism," with Hayek offering one of the most prominent critiques (see, e.g., Hayek 1952).

To advance these aims, the Council continued to operate through conferences and the establishment of specialized subcommittees, typically organized in a multidisciplinary fashion. Two of these are particularly relevant for the present analysis: the Committee on Political Behavior and the Committee on the Mathematical Training of Social Scientists.

An initial committee focused on political behavior was established in 1945 and included figures such as Herring, Charles Hyneman, and V. O. Key Jr.. However, the fully fledged interdisciplinary Committee on Political Behaviour was created only in December 1949, following a conference on political behavior held at the University of Michigan from August 27 to September 2 of that year, sponsored by the Council (Ranney 1974).[31] The conference brought together 29 social scientists: twenty political scientists, two sociologists, three anthropologists, three psychologists, and one

[31] A report on the conference was published in the final 1949 issue of *Items* (Heard 1949).

statistician. The topics discussed ranged from methodological issues in the study of political behavior to more specific subjects such as governmental organizations, social authority, community structures, and political history. Particular emphasis was placed on the psychological foundations of political behavior and on emerging research techniques, such as field inquiries, exemplified by V. O. Key's *Southern Politics* (Key 1949).

It is tempting to compare the significance of this conference with that of the contemporaneous "Conference on Activity Analysis of Production and Allocation," sponsored by the Cowles Commission. However, closer examination reveals important differences. The Cowles conference represented a genuine "who's who" of mathematical economics (Düppe and Weintraub 2014a), including contributors such as Arrow, Dantzig, Dorfman, Gale, Koopmans, Kuhn, Morgenstern, Samuelson, Simon, Tucker, and Hurwicz.[32] Moreover, the conference played a decisive role in shaping participants' intellectual trajectories and their understanding of disciplinary development (Arrow 1983).

By contrast, the Michigan conference included relatively few figures who would later become central to the historiography of political science, aside from Truman, Key, and the quantitative sociologist Paul Lazarsfeld, in addition to Pendleton Herring himself. This contrast may reflect the divergent paths pursued by economics and political science. The Cowles conference presented tools that were immediately applicable and technically transformative for economic research.[33] By contrast, the conference on political behavior was embedded in a scholarly environment primarily oriented toward reflecting on the premises, scope, and objectives of political inquiry. It neither represented a radical methodological rupture nor provided ready-made tools for advancing research. Rather, it signaled a broader intellectual mood within the discipline.

In other words, while economists and political scientists shared a common ambition, rendering their disciplines more scientific, they pursued this goal through different questions and along distinct trajectories. A young economist attending the Cowles conference in 1949 would have encountered novel theoretical frameworks and computational tools that contrasted sharply with prevailing practices in economics departments. A political scientist interested in systematic methods, by contrast, could often pursue such interests outside the Michigan conference, engaging with subcommunities that did not necessarily converge there.

The Committee on Political Behaviour, established in December 1949, was chaired initially by Key (1949–53) and subsequently by Truman (1953–64). Its central concern was "the development of theory and improvement in methods which are needed if social science research on the political process is to be more effective" (quoted in Ranney 1974). Among its concrete outcomes were major initiatives in survey research on U.S.

[32] This group included five future Nobel laureates in economics, as well as foundational contributors to mathematics such as Dantzig, Kuhn, and Tucker.

[33] Perhaps the most notable outcome was Dantzig's first proof of the simplex algorithm for solving linear programming problems.

presidential elections, beginning with the 1952 election and extending retrospectively to earlier contests, as well as efforts to refine analytical methods through conferences and summer seminars. The committee also played a key role in promoting comparative politics, which in 1954 became the focus of a separate committee, the "Committee on Comparative Politics".

While the Committee on Political Behavior concentrated on a substantive domain within political science, the Council also devoted considerable attention to the training of social scientists. In the early Fifties, this commitment took the form of a renewed emphasis on mathematical education, culminating in the establishment of the Committee on the Mathematical Training of Social Scientists in 1952.[34] The committee's activities and objectives were later reconstructed by Frederick Mosteller in a retrospective account published in 1974 (Mosteller 1974). In addition to organizing conferences and summer schools, the committee issued policy recommendations aimed at improving mathematical training for social scientists.

A policy statement published in *Items* in 1955 identified three strategies for addressing the mathematical needs of social scientists: the creation of specialized mathematics curricula for social science students; the introduction of dedicated sections within existing mathematics courses; and the reform of general undergraduate mathematics curricula to provide a common foundation for all students (Scientists 1955). The rationale for enhanced mathematical training lay both in the capacity to model social problems formally and in the ability to assess the usefulness and limitations of mathematical methods.

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" (Scientists 1955, 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." (Scientists 1955, 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.

Focusing mainly on specialized curricula, the committee argued that "the traditional undergraduate curriculum in mathematics—college algebra, trigonometry, analytic geometry, and calculus—does not afford satisfactory preparation to social scientists" (Scientists 1955, p. 14). Alternative training

[34] The original members were William G. Madow (mathematician), E. P. Hutchinson (sociologist), Jacob Marschak, George A. Miller (psychologist), Frederick Mosteller (statistician), and Robert M. Thrall (mathematician) (Scientists 1955).

pathways were then proposed and it was encouraged closer integration between mathematics and the social sciences, including greater mathematical literacy among social scientists and greater social science exposure among mathematicians. 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." (ibidem)

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 Council sponsored three summer institutes on mathematics in the social sciences in 1953, 1955, and 1957: the first at Dartmouth College, the second divided between the University of Michigan and Stanford, and the third, focused on advanced training, at Stanford.[35] Participants were drawn primarily from economics and psychology, with more limited representation from sociology and very few from political science. The committee's activities continued into the Sixties, increasingly concentrating on specialized topics (Orozco Espinel 2020).

Mosteller's 1974 retrospective can be read as a declaration of "mission accomplished," opening and closing with the observation that the mathematical training of social scientists was the product of a long and deliberate process rather than organic evolution:

> "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 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)

This assessment underscores the decisive role played by the SSRC in early postwar American social science, not only in fostering interdisciplinary collaboration but also in reshaping the very image of the social sciences. The case of mathematics is emblematic in this regard. The activities of the Committee on Mathematical Training paralleled, and were partially reinforced by, the development of mathematical economics at institutions such as the Cowles Foundation and RAND. Although mathematical economics was already far more advanced than parallel developments in other disciplines, the initiatives and summer institutes of the Social Science Research

[35] For a detailed overview, see Mosteller 1974; Orozco Espinel 2020.

Council contributed to the broader diffusion of mathematical methods within economics as well (Orozco Espinel 2020).

Yet a striking feature of the committee's activities in the Fifties is the marginal role played by political science. Few political scientists appear to have participated in these training programs, at least as suggested by available reports. This absence may be explained by the simultaneous engagement of the discipline's most active figures in the Behavioral Revolution, which pursued the ideal of systematic social science along paths other than formal mathematical modeling. At the same time, this context renders those political scientists who did choose to pursue formal approaches to political analysis all the more distinctive, and their intellectual efforts correspondingly more deserving of historical investigation.

Chapter 2

Theory of Games and Formal Political Theory: Before Riker

This chapter will provide a general discussion of the development of the Theory of Games and of some formal theories of politics in the Fifties.

The broader climate of transformation in American social science framed the development of these theories. Whereas the first chapter discussed the so-called "Behavioral Revolution," which radically reshaped American political science from the Fifties onward, the following pages will also show how economic analysis addressed political issues, such as voting behavior, through formal modeling. Previously it has been highlighted the pivotal role of *Theory of Games and Economic Behavior* in shaping the mathematical outlook of postwar economics. Now it will be discussed how the theory of games moved across domains other than economics.

Accordingly, this story is about political science and the history of Game Theory. For Game Theory, its cross-disciplinary use is far from unproblematic and raises broader questions about its own evolution. Indeed, the delayed acceptance of Game Theory into mainstream economic theory dates only to the early Eighties. Moreover, what ultimately entered economists' toolbox was John Nash's approach (together with major refinements by John Harsanyi, Reinhardt Selten, Robert Aumann, Robert Wilson, David Kreps, among others), rather than the theory von Neumann and Morgenstern presented in the Forties. Yet the first attempts to use game theory to address social and political issues in the Fifties drew on both approaches.

American political scientist William Harrison Riker was a crucial figure in the history of how game theory entered political science because he did not merely adopt game-theoretic notions. Rather, the theory of games also served 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 explicitly game-theoretic. Thus, rational choice and mathematical modeling matched the expectations of a scholar like Riker, who was searching for new methods of political analysis.

Riker's efforts also paralleled related developments in economics. In the Fifties, scholars such as Kenneth Arrow, Duncan Black, Anthony Downs ,

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and Martin Shubik (with Lloyd Shapley), as well as James M. Buchanan and Gordon Tullock, produced formal analyses of collective choice, power, and voting, and new research fields such as "Social Choice" and "Public Choice" took shape. Although many of these works were not, strictly speaking, game-theoretic, they relied on the same behavioral model—often grouped under the label "Rational Choice Theory", that flourished in the wake of von Neumann and Morgenstern's 1944 work.

From this perspective, Game Theory plays a pivotal role. It influenced economics and the social sciences in two related ways. On the one hand, it provided the first fully consistent mathematical model of individual behavior (through von Neumann's work on utility under uncertainty), shaping subsequent models of rational choice. On the other hand, it formalized strategic behavior and offered early models of interaction. More generally, both developments affected the discourse on rationality and rational decision-making across the social sciences.

The first section of this chapter provides a somewhat detailed review of analytical aspects of the theory of games, focusing mainly on von Neumann and Morgenstern's framework. The second section discusses results in social choice (Arrow), analyses of voting and elections (Black and Downs), and their relationship with game-theoretic ideas.

2.1 The "Theory of Games and Economic Behavior"

Many works (starting from Weintraub 1992) have sought to reconstruct and discuss the indisputable impact of Game Theory on contemporary (post-Eighties) economics. Robert Leonard (Leonard 2010) showed how the concept emerged in the first half of the twentieth century and how it was embedded in the intellectual climate surrounding formalism and axiomatization in mathematics. 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.

A second stream of influence, also discussed in Leonard's work, came from debates on the nature of the social sciences, and, more in general, on scientific knowledge, which animated the Viennese intellectual milieu until the Anschluss (1938). In particular, the *Theory of Games* reflected the concerns and aspirations of its creators, the Austrian economist Oskar Morgenstern and von Neumann. Before leaving Austria in 1938, Morgenstern was among the most active scholars engaged in such discussions. In 1928, von Neumann published a mathematical result defining rational behavior in a "game" with two players and opposed interests (Neumann 1928). The two met at Princeton in early 1939. Morgenstern arrived there in 1938. Von Neumann had joined the Institute for Advanced Study shortly after its establishment (1932), although he continued to spend research time in Europe at least until political circumstances made this impossible.[1] Jointly,

[1] 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

they published in 1944 *Theory of Games and Economic Behavior* (Neumann and Morgenstern 1944).

Leonard's historical work explores the invention of Game Theory. Other scholars have focused on different issues, most notably the place that the original conception of Game Theory, as articulated in the 1944 founding text, occupies in the broader history of twentieth-century neoclassical economics. (Giocoli 2003b)

One striking feature of Game Theorys's history is that, although its importance for postwar economics cannot be denied, its influence on the discipline differed sharply from von Neumann's and especially Morgenstern's expectations, and its adoption was slower than they had hoped. What the postwar generation of young economists found in the 1944 text was, on the one hand, a concise yet detailed mathematical toolbox relevant to optimization theory and, on the other, a rigorous characterization of rational utility under uncertainty. These, especially the latter, filled a major gap and generated a large literature from the Fifties onward. Yet even a quick look at *Theory of Games*'s table of contents suggests that these topics do not occupy the central place in the book.[2] Much of the work is devoted to strategic situations involving many players (more than two) and allowing for agreement and coalition formation. This analysis builds on von Neumann's pivotal 1928 demonstration of an equilibrium solution for a two-person zero-sum game, the Minimax Theorem (for which *Theory of Games* also provides a refined proof). Yet the most distinctive contribution of the book lies in its analysis of coalition formation. From this standpoint, much of *Theory of Games* differs substantially from what later became standard in game theory textbooks.

The reasons for the delayed rise of Game Theory in economics are multiple. They largely concern features of von Neumann and Morgenstern's framework itself. For decades, it was unclear what Game Theory was meant to accomplish for economic theory and how it could do so. It addressed strategic behavior, but the behavioral foundations of coalitional games (the kind emphasized in *Theory of Games*) were often regarded as unclear. Only the theory of two-person games of pure opposition (two-person zero-sum games) offered a comparatively well-defined framework.[3] This raised a basic question: what is the purpose of a theory of coalitions? It is difficult to treat it as prescriptive, since complex real-world situations rarely fit a well-specified game structure. For similar reasons, it also seemed hard to treat it as predictive.[4]

von Mises (to which Morgenstern belonged in the Twenties), 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. Leonard 2010 contains these and other biographical details. For von Neumann's intellectual and scientific biography: Israel and Gasca 2009

2 As a matter of example, the two authors added the detailed axiomatic proof of their utility function only in the second edition of *Theory of Games*, published in 1947

3 Henceforth 2ZPSG

4 One interesting page of the development of Game Theory 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

From the standpoint of technical economics, Game Theory, both von Neumann and Morgenstern's original work and the refinements developed by Princeton scholars such as Nash, Tucker, Kuhn, and Shapley contributed substantially to economic theory in terms of tools, solution techniques, and theorems. For example, most textbooks of mathematical economics through the Seventies included at least a brief discussion of the Minimax theorem, particularly after it was shown in the late Forties that it corresponds to a linear optimization problem. Another important example concerns fixed-point theorems, a topological tool first employed by von Neumann in the only work he devoted explicitly to economic theoy (Neumann 1945).[5], then by Nash in his dissertation, and later by Arrow and Debreu and by McKenzie in their proofs of the existence of general equilibrium.[6]

Nonetheless, Game Theory remained peripheral in economics until the Eighties. Economists widely employed Rational Choice under certainty and uncertainty, and most theoretical problems were framed in terms of optimization. Moreover, the discipline increasingly adopted mathematical modeling. Yet only a small group of scholars took Game Theory seriously and remained committed to its core conceptual and theoretical problems.[7] This situation persisted until the late Seventies and early Eighties. Only then, partly due to diminishing enthusiasm for general equilibrium theory and the emergence of powerful ways to model richer economic environments as games (e.g., alternative models of competition), did Game Theory become central within economics.[8]

This simplified narrative broadly captures the case of economics. How-

(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 Sixties 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 Game Theory was only a particular case of the general approach.

5 A history of this contribution is contained in Weintraub 1983

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

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

8 For a historical-review paper quite challenging of the adequacy of Game Theory in providing an effective solution to Industrial Organization problems, see: Fisher 1989. Similarly, Abu Turab Rizvi contended that Game Theory 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. The main reason lies in the arbitrariness of the models' assumptions, with special regard to the role of information (Ingrao and Israel 1987; Rizvi 1994).

ever, Game Theory was never confined to that discipline.[9]

Although Game Theory was deeply embedded in neoclassical economics' theoretical problems, like equilibrium, rationality or prediction, it was also, in important respects, more "social" and less narrowly "economic" than is often presumed. This connects to another well-known aspect of Game Theory's history: military strategists, international relations specialists, government officials, and scientists used game-theoretic ideas to address urgent Cold War problems.

This is, in some respects, a curious development. The initial limited uptake of Game Theory in economics was tied to a persistent question: what could modeling a game explain about the real world that existing models could not? The difficulty of answering this question pushed many economists away from the theory. Yet researchers concerned with more concrete and high-stakes problems than, for example, modeling market-clearing manifolds, often found the theory useful.

One plausible answer is the following: Von Neumann and Morgenstern's theory of n-person games did not capture the full range of issues involved in general equilibrium or individual rational action. Non-cooperative Game Theory proved more serviceable to many researchers, but it was not easy to integrate with existing rational choice approaches.[10] By contrast, when the number of players is small (for example, two), simpler uses of Game Theory, such as representing strategies and outcomes in matrix form, can be advantageous. Since much of two-person game theory was already developed in the Fifties, it is reasonable to infer that Game Theory could be functional for certain political phenomena even when economic problems were, at the time, more effectively treated using the dominant tools of economics in the Fifties and Sixties.

A different, more sociological answer would emphasize the distinctive intellectual commitments of the institutions that shaped the so-called "Cold War rationality" (see Erickson et al. 2015; Erickson 2015).

When one thinks about Game Theory in political science, the immediate association is often a strangelovesque connection to Cold War strategy, nuclear deterrence, and related topics. Less well known is that Game Theory entered political science through another route, namely through Riker's scholarly activities. Unlike many international relations theorists or "Cold War warriors," Riker sought to establish an entire subfield of political research in which the Theory of Games was not merely an instrument but the core of theoretical development. The historian Sonja Amadae and the political scientist Bruce Bueno de Mesquita wrote 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

[9] This point is familiar to historians of ideas and sociologists of science who emphasize how economic techniques crossed disciplinary boundaries. It is not the purpose here to engage fully with that literature. Something will be said in the conclusions and the text when dealing with specific issues but only to note that, in the case of Game Theory, the story is less linear than is often assumed.

[10] Especially because Game Theory was by far less intuitive in its theoretical premises. See the following paragraph

economists grasped its promise for grounding a new mathematics of the market." (Amadae and Mesquita 1999, p. 278). This statement is perhaps somewhat too generous. Game Theory never ceased to be studied at places such as RAND, even during the height of general equilibrium theory in the Sixties and Seventies. Moreover, among economists, the reception of Riker's game-theoretic work was close to zero, at least in the Sixties.

Nevertheless, Riker's role in the cross-fertilization between game theory and other social sciences cannot be ignored. He drew on elements of von Neumann and Morgenstern's original framework—the very approach that many economists had set aside and that was not central in international relations scholarship. He did so with unusual commitment and with strong confidence in Game Theory's capacity not only to model but also to generate predictions about political outcomes. The institutional and intellectual consequences of Riker's advocacy became especially visible after the Sixties, when, scholars like Richard McKelvey and Peter Ordeshook (among 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-Sixties.[11]

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

At the core of Game Theory lie the notions of "game" and "rationality." The idea of developing an algorithmic procedure to determine the best way of behaving in situations such as parlor games attracted some attention among mathematicians in the first half of the XXth century. Chess offers the best-known example of a pure-opposition game.[12] At the same time, economists were struggling to provide a theory of human behavior that could clarify the rationale behind economic choices. In the Thirties, as seen, debates over equilibrium, perfect foresight, and how to model economic phenomena were particularly intense.

In 1928, von Neumann wrote the first mathematical paper on Game Theory (which he labeled *Gesellschaftsspiele*, i.e., games of society). In that work, he studied two-player games of pure opposition, showing how rational players should behave in such settings: the "Minimax Theorem." (Neumann 1928) In his joint work with Morgenstern, he extended this approach to n-person games, where a natural opposition of interests arises not between individual players but between groups of them, or "coalitions." In the 1944 work, the problem of rational behavior is the starting point from which the authors develop their analysis. Accordingly, beyond the discussion in

[11] These topics will be part of this dissertation's final part.

[12] 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 Game Theory 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 Kuhn and Tucker 1953).

the first (verbal) chapter, they offered a mathematical characterization of individual rational action in two ways: first, by providing an axiomatic treatment of individual utility—one of the long-standing issues in the economic theory of their time (Moscati 2018)[13]; and second, by reformulating the minimax more accessibly (while also introducing the mathematical notions needed to understand the general result).

Starting with the former, they axiomatically derived an "Expected Utility Function," namely a real-valued function of the form:

$$\sum_{i=1}^{n} p_i u_j(x) \quad \text{for} \quad i, j = 1 \dots n,$$

where p denotes the probabilities associated with each possible outcome $u(x)$. That is, for each individual, utility is computed as the sum of each possible disjoint event multiplied by the probability of its occurrence.[14] After their work, it became customary for economists to define rationality as consistency with axioms concerning properties such as orderings and preorderings between alternatives (or preferences), and to use these properties to construct continuous and differentiable utility functions.[15]

The reception of von Neumann and Morgenstern's utility function was immediate. It paved the way for a long sequence of debates on the empirical plausibility of utility theories (for instance, using psychology and laboratory experiments) as well as on the formal characterization of utility theory and rational choice. However, within von Neumann and Morgenstern's Game Theory, utility theory played a limited role. It mainly served to justify

[13] 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 Thirties. 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 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.

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

[15] Arrow described Rational Choice as the choice that satisfies the properties of connectedness and transitivity (see Arrow 1951b, 17 et ss.). Debreu's classical proof 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 "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 Expected Utility Theory, its properties, and developments (like Leonard Savage's theory, and Aumann-Anscombe's, can be found in any advanced microeconomics textbook

the notion of payoffs as quantities representable by a single number and, therefore, to ground strategic action. To identify the best course of action in a game, a stronger characterization of each player's rational behavior was needed—namely, that provided by the Minimax. This result, originally demonstrated by von Neumann in 1928 and reprised in the 1944 work, states that in two-player situations of pure opposition (2PZSG), each player should choose the strategy that minimizes the maximum payoff the opponent could obtain. (Neumann and Morgenstern 1944, 85 et ss. Neumann 1928)

The Minimax is a clear mathematical result that relies on a fundamental property of the theory of functions: the existence of "saddle points." Namely, $\max_x \min_y F(x,y) = \min_y \max_x F(x,y)$ holds when there exists a saddle point (x_0, y_0) satisfying $F(x,y_0) = \max_x F(x,y)$ and $F(x_0,y) = \min_y F(x,y)$. Von Neumann employs this result to solve a version of the two-person game in which players adopt only "pure" strategies (there is no chance) and interests are perfectly opposed (the game is "zero-sum"). The "zero-sum condition" is: $F_1(s_1,s_2) + F_2(s_1,s_2) = 0$, from which $F(s_1,s_2) = -F(s_1,s_2)$.[16] Player 1 wants to maximize $F(s_1,s_2)$, while player 2 wants to maximize $-F(s_1,s_2)$, which is equivalent to minimizing $F(s_1,s_2)$. Therefore, each player's behavior can be written as:

$$\max F(s_1,s_2) = \min F(s_1,s_2)$$

Conceptually, the difficulty is that each player controls only her own strategy, but the opponent's behavior is also payoff-determining. Von Neumann addresses this 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, the "good way" to play for player 1 is to choose a strategy that maximizes the function $\min_{s_2} F(s_1,s_2)$. Indeed, player 2, acting after player 1, will choose an s_2 that minimizes $F(s_1,s_2)$, i.e., will deliver $\min_{s_2} F(s_1,s_2)$. Therefore, for player 1, the value of the game is: $\max_{s_1} \min_{s_2} F(s_1,s_2) = v_1$

A symmetric argument holds for player 2 (the "Majorant Game"), except that he acts after player 1. Knowing that player 1 will maximize his payoff, player 2 chooses a minimizing strategy. If v_1 and v_2 are equal, then a saddle point exists for $F(s_1,s_2)$ and the game has a solution. This result relies on the existence of a pair of strategies that are saddle points. Of course, their existence is not automatically guaranteed for pure strategies. Von Neumann showed that it is guaranteed when one allows mixed strategies (where players choose probabilities over strategies).[17]

This conclusion, which avoids reference to players' psychology, offers a clear prescriptive justification for action and an objective criterion for what counts as rational strategic choice. For this reason, many scholars

[16] 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 simpler notation than that used by von Neumann.

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

have linked the result, and the proof offered in the 1944 work, to von Neumann's broader aims in developing Game Theory. Giocoli, for instance, argues that von Neumann was especially interested in the prescriptive content of rationality, a point he connects to this author's preference for a "direct proof" in their work.[18] Leonard instead frames this interest in von Neumann's concerns about the disruption of European political and social order, threatened by Communism and Fascism (Giocoli 2003a; Leonard 2010).

The normative orientation of von Neumann's concept of rationality is apparent in the formula he and Morgenstern used in their work: the "best way of playing a game." The two authors devote substantial space in the first chapter to clarifying what rational behavior means; yet, ultimately, they rely on the mathematical force of the Minimax. They attribute the absence of a satisfactory analysis of rational behavior in the social sciences to "the failure to develop and apply suitable mathematical methods to the problem." (Neumann and Morgenstern 1944, p. 11)[19]

This point deserves further elaboration. A rational individual, in the simplest case (the classical "Robinson Crusoe"), seeks to obtain the maximum of something (e.g., utility or money). However, this "maximum problem" takes on new features in social settings, which involve 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) Characterizing rational behavior in a positive sense is therefore difficult because rational decision-making requires forming expectations about what others will do. Morgenstern had already argued in the Thirties that this can generate paradoxes that undermine equilibrium notions premised on perfect foresight (Morgenstern 1976a). By contrast, the minimax breaks this regress by specifying each player's rational strategy when the opponent is also rational.

As a behavioral criterion, however, the Minimax was subject to criticism, especially for being overly defensive and thus weakening von Neumann's aspiration to provide a substantive normative justification of rational choice

[18] 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 the Nash Equilibrium is the game's solution, and it does not matter how it is reached or why people pursue an equilibrium strategy.

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

(how and why agents are rational). The Minimax secures for each player a payoff regardless of what the opponent does: assuming the opponent is rational, choosing the strategy that minimizes the maximum the opponent can obtain delivers a security payoff. Yet it is often seen as a poor description of actual behavior because it does not exploit possible deviations by the opponent, but instead focuses on guaranteeing a minimum outcome.

A more general characterization of rational choice would therefore be desirable, one not necessarily tied to a "negative" criterion like Minimax. John Nash famously supplied such a solution: the "Nash Equilibrium." (Nash 2002a)[20] He also showed that in 2PZSG the Minimax and the Nash equilibrium coincide. Yet the Nash Equilibrium, the central solution concept in much of contemporary economics, is less intuitive as a prescriptive account of rational behavior than von Neumann's. This points to a persistent substantive tension in Game Theory and Rational Choice Theory: if treated as prescriptive, they may not be predictive, because the prescriptions can be difficult to follow in real interaction; if treated as predictive, they are hard to use prescriptively, because it is rarely possible to establish with certainty whether a real-world situation corresponds to a Nash Equilibrium.[21]

A further issue concerns the taxonomy of games. Indeed, the foundational distinction between "Cooperative Games" and "Non-cooperative" games also shapes competing views of rational behavior. Even if rational choice is comparatively intuitive in cases of pure opposition and a small number of players, it becomes more complex when the number of players increases, the rules permit binding agreements, or the temporal horizon of the game is ill-defined.

In *Theory of Games and Economic Behavior*, 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 is the degree of cooperation permitted among players. Yet they did not explicitly addressed this point. In their framework, every n-player game is solved through the formation of coalitions, and they propose a specific solution concept, the "stable set" (see below). The possibility of forming coalitions thus yields a distinct approach to solution.

[20] This idea refers to strategic or "non-cooperative" games . 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).

[21] In other words, it is intuitive why a Nash Equilibrium 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 Nash Equilibrium 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. Nash Equilibrium is then a rationalizable strategy, but not all rationalizable strategies are Nash Equilibria. Douglas Bernheim and David Pearce first introduced this notion separately in 1984 (see Bernheim 1984; Pearce 1984)

The explicit distinction between "Cooperative" and "Non-cooperative" games was introduced by Nash, with reference to the degree of communication permitted among players (Nash 2002b; Nash 2002a). In settings where communication is impossible, each player chooses the course of action she believes will yield the highest payoff (a case that resembles how economists traditionally modeled competitive markets). Nash established the fundamental theorems for such games, most notably the "Nash Equilibrium," i.e., a strategy profile in which no player has an incentive to deviate given the strategies chosen by the others.[22] Although the Nash Equilibrium is the most influential result in Game Theory, it applies to "non-cooperative" settings; and Nash's original framework required subsequent refinements to handle richer environments, such as multistage interaction or imperfect information—refinements that, in some cases, appeared only decades later.

In 1953, Nash returned to this distinction in his discussion of bargaining theory. He analyzed a setting in which two agents have opposed interests (although "neither completely opposite nor completely coincident," Nash 2002b, p. 99) and an incentive to reach an agreement through negotiation. This setting 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)[23] After Nash, von Neumann and Morgenstern's framework came to be labeled "cooperative."

In *The Theory of Games and Economic Behavior*, the authors had already introduced several notions that later became standard in Game Theory, such as matrices for representing two-person games and game trees for representing multistage interaction (respectively, games in "normal form" and in "extensive form").[24] They also showed how games can be represented through set-theoretic notions, such as partitions, to address the degree of information among players; and they offered a precise characterization of "strategy" as a function that associates each possible course of action with an expected payoff (Neumann and Morgenstern 1944, 79 et ss.). Yet their most detailed analysis concerns games that allow coalition formation and whose solution is therefore represented by a set of admissible arrangements, rather than, as in Nash's later framework, by a strategy profile. This highlights the difference between solving a "cooperative game" and identifying a Nash equilibrium in a non-cooperative game.[25]

[22] Not all non-cooperative games have a Nash Equilibrium. To provide the existence of a Nash Equilibrium, the set of actions of each player must be non-empty, compact, and convex, and the preference relation on this set must continuous and quasi-concave (to explore this point, see any intermediate textbook of Game Theory, for instance, Osborne and Rubinstein 1994).

[23] 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)

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

However, it should also be noted that the distinction between "cooperative" and "non-cooperative" games has, to some extent, been softened, beginning with Nash himself. In his last decisive contribution to Game Theory, he attempted to provide "non-cooperative" foundations for "cooperative games." (Nash 2002c) [26] He thus developed a bargaining model in which the bargaining solution is derived from a non-cooperative threat game. This initiated a research program, the "Nash Program", that expanded especially after the Eighties, in part through pivotal refinements of Nash equilibrium by John Harsanyi and Reinhardt Selten (Serrano 2005; Binmore and Dasgupta 1987).

Finally, another crucial distinction is about the number of players. This is important especially for "cooperative games" because each player's strategic reasoning includes the evaluation of joining alternative coalitions. In such cases, rational behavior can mean very different things: interaction with a single opponent or interaction with multiple players, where a wider set of options is available. For a 2PG, the distinction between "cooperative" and "non-cooperative" settings can sometimes be blurred. In n-person games, by contrast, it is fundamental: it changes solution concepts, as well as the meaning and potential applications of the theory. Indeed, when discussing three-person zero-sum games, 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)

Whereas "pure opposition of interests characterizes 2PZSG," an n-person game involves the possibility of allying with other players; in other

[25] Note also that there are different definitions and domains of Cooperative Game Theory. 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)

[26] Nash devoted the remnant part of the Fifties 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 Eighties 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 Kuhn and Nasar 2002

words, a "parallelism of interests" can arise. According to the authors, this makes cooperation desirable and increases the scope for agreement. As they conclude: "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 meaning of rational behavior. Here the dilemma concerns not only the "positive vs. normative" status of rational choice, but also the substantive content of what players do. Under some solution concepts—especially when they yield sets with many elements—asking "what does it mean to behave rationally" risks becoming a question with an indeterminate answer.

2.1.2 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, finding a solution to such a game implies identifying a set of players' payoffs that displays some form of "stability."

Since the authors defined a game as the "set of rules which describe it," a solution is the set of rules for each participant specifying how to behave in every situation that may arise. More specifically, a solution can be described as a "set of imputations" (which, in the case of 2PZSG, includes only one element). By "imputation," they meant the outcome for each participant, namely the behavior satisfying reasonable requirements for "optimum behavior." Consider, for example, an n-person zero-sum game: players can combine to exclude the remaining ones; a solution must therefore account for the gains of each player within a coalition, while also recognizing that coalitions may break and that players can join new ones if they can secure higher yields.

Unlike the Minimax discussed above, a solution for the n-person zero-sum game also embodies "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)[27]

[27] These words, and the next paragraph refers to the first chapter of *The Theory of Games and Economic Behavior*. 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

Generally speaking, a solution V is a set of payoff distributions (or "imputations") x and y such that 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 an n-person game, different arrangements of players are possible, and payoffs can be distributed in different ways. A solution is a set of "distributions" that displays both "internal stability" and "external stability."[28]

Morgenstern explicitly related the notion of imputation to that of partial equilibrium in economic theory. He went further, however, in the case of "sets of imputations," linking them to standard patterns of behavior in social organizations. In a social context, the authors argued, the system of imputations describes the "established order of the society." Moreover, since the set of imputations also has internal 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 weakness of this notion lies in the lack of "uniqueness," i.e., the fact that many (potentially infinite) solutions to a single game can exist.[29] Nevertheless, according to the authors, this is not necessarily a problem. If stability refers to a "standard of behavior," 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 largely verbal character of these pages, together with the analogy between a game's solution and different "standards of behavior," led some scholars to speak of an "institutionalist side" of von Neumann and Morgenstern's thought (Giocoli 2003b; Schotter 1992). This is unsurprising. 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) They thus suggested that Game Theory could offer an alternative framework for the 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

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 creating the Theory of Games see: Morgenstern 1976b; Schotter 1992; Rellstab 1992; Leonard 2010).

[28] "Internal stability" and "external stability" are habitual definitions nowadays for referring to the conditions above (see Shubik 1984) However, neither von Neumann and Morgenstern nor early textbooks, like Luce and Raiffa's, adopted them. "Domination" refers to the fact that imputations are vectors. 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.

[29] But to fully grasp this aspect, the verbal definition is not sufficient anymore, and the mathematical treatment is necessary

nature of things, most opinions thus far could hardly have been proved or disproved within the field of social theory." (ibidem) This also helps explain why many scholars—including von Neumann and Morgenstern—took for granted that game theory's natural boundaries extended to the social sciences in general, not only to economics.[30]

The discussion presented above is developed rigorously in the sixth chapter of the 1944 work (Neumann and Morgenstern 1944, pp. 238–90). Here, the reader is guided through a quasi-textbook, step-by-step procedure for defining coalition concepts formally, namely, each coalition's payoff and the distribution of that payoff in a way that makes the outcome acceptable to each player.

The first step is to reduce the complex possible outcomes of a game to a single number. In mathematical terms, the device adopted is a real-valued function defined on the set of all subsets of $N = 1, ..., n$ (the set of all players), i.e., the set of all possible coalitions. This function, called the "characteristic function" and denoted $v(S)$ for each $S \subset N$, assigns a numerical value to each coalition. To determine the characteristic function, von Neumann treats each n-person game as a 2-person game, where players 1 and 2 are, respectively, the coalition and its complement. One can then apply the minimax theorem and determine a single value for each coalition. (Neumann and Morgenstern 1944, pp. 239–40)

The characteristic function captures "everything that can be said about coalitions between players, compensations between partners in every coalition, mergers or fights between coalitions." (Neumann and Morgenstern 1944, p. 240) At the same time, it does not specify how to divide the coalition value among its members. From a mathematical point of view, $v(S)$ is not simply the sum of individual payoffs within the coalition. Rather, it is defined so as to account for the options each player has to leave one coalition and join another. Mathematically, $v(S)$ has three main properties:

$$v(0) = \emptyset$$

$$v(S) = -v(S)$$

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

Intuitively, the first means that the value of a coalition without players is zero. The second indicates that the value of a coalition is the negative of its complement (by reduction of the n-person game to a 2-person game and the zero-sum condition). The third means that the value of a coalition formed by the union of two disjoint coalitions cannot be less than the sum of their values. From these properties, others follow logically:

$$v(N) = 0$$

[30] 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)

$$v(S_1 \cup \cdots \cup S_n) \geq v(S_1) + ... + v(S_n)$$

if $S_1 ... S_n$ are pairwise disjoint subsets of N. And

$$v(S_1) + ... + v(S_n) \leq 0$$

if $S_1, ..., S_n$ are pairwise disjoint subsets of N with the sum N.

The core of von Neumann's argument is that the analysis of any n-person zero-sum game can be carried out by means of characteristic functions.[31] To show this, he introduces the concept of "strategic equivalence." If in two games Γ and Γ', the strategic possibilities are the same and the only differences consist in fixed payments to each player,[32] then the two games are equivalent. He also introduces the notion of the "reduced form" of $v(N)$, i.e., $\bar{v}(S)$.[33]

A further aspect is the distinction between "inessential" and "essential" games.[34] If $\bar{v}(S) = 0$ for all S, the game is "inessential": no coalition generates a surplus, because each player can obtain alone what he can obtain in coalition with others. The opposite case, where $\bar{v}(S) > 0$, defines an "essential" game. Only essential games are of interest for von Neumann and Morgenstern's analysis.[35]

Finally, another key point concerns "symmetry" and, relatedly, "fairness." Symmetry was introduced in the discussion of 2PZSG and means that switching the players' roles does not change the game. Beyond the implications this has for how one conceptualizes players' individuality, symmetry becomes even more important in n-person games.[36]

[31] 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.

[32] 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

[33] 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, pp. 185–9). The role of the inequalities above is further explored in Neumann and Morgenstern 1944, 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 Characteristic Function for every number of elements in S.

[34] 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.

[35] Another way of defining 'inessentiality' in terms of the properties of characteristic function is the following: $v(S \cup T) = v(S) + v(T)$ (Neumann and Morgenstern 1944, p. 251). This is called "additivity". Therefore, essential games have a non-additive characteristic function.

[36] The condition of symmetry has been so defined: "the symmetry [...] requires that

Having established the axiomatic foundations of the characteristic function, von Neumann derives solution concepts for n-person games, beginning with the simplest case of a 3-person zero-sum game (Neumann and Morgenstern 1944, pp. 260–3). In this setting, the structure of the game is determined by two-player coalitions. Assuming the reduced form ($\gamma = 1$), the following characteristic functions for different coalitions are possible:

- $v(0) = 0$
- $v(1) = -1$
- $v(2) = 1$
- $v(3) = 0$[37]

Considering two-player coalitions, these are $\{1,2\},\{2,3\},\{1,3\}$. Accordingly, for each coalition the following payoff distributions 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, while the excluded player receives -1 (the value of one-member coalitions). These distributions, labeled "imputations," exhaust the game's strategic possibilities. The solution is the set of all three. Only this set yields a stable equilibrium, but stability is a property of the three distributions taken together. Indeed, each coalition can be circumvented, for instance, if one player defects and forms a new two-member alliance with the previously excluded player.[38]

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

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

and

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

These properties correspond to the intuitive ideas of "individual rationality" and "Pareto optimality" (even if the authors did not label them in this way in the 1944 work). The first states that a player, whether inside a

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)

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

38 In von Neumann and 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)

coalition or alone, should not accept a payment below what he can secure by acting alone. The second implies that the sum of imputations cannot exceed the value of the grand coalition (which, as seen, is zero). At the same time, it cannot be less than zero, because in that case some player could gain without any loss to the others (Luce and Raiffa 1957, pp. 192–3). Mathematically, an "imputation" is a vector in an n-dimensional vector space L_n. A subset $S \subseteq N$ is 'effective' if:

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

An imputation $\vec{a}$ dominates another imputation $\vec{b}$ (i.e., $\vec{a} \succ \vec{b}$) if there exists a set S with the following properties::

$$S \neq \emptyset$$

$$S \quad \text{is effective for } \vec{a}$$

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

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 $\vec{a} \in b$ [39].

The elements in V are precisely those not dominated by any element of the set, and which dominate all imputations outside the set. This is the same definition given above, but now each term has a precise formal meaning.

Yet, although these definitions clarify how the solution of an n-person game can be interpreted as a "standard of behavior" and can display "some kind of stability," the same game rarely has a unique solution. Hence, "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) The authors show, for instance, that in an inessential game there exists precisely one imputation, namely the vector such that $a_i = v(i)$. In an essential game, by contrast, infinitely many imputations exist, though not the one above.[40]

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

[40] 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).

2.1.3 The reception of von Neumann and Morgenstern's Theory of Games

Works as the aforementioned by Giocoli and Leonard explored the reception of *Theory of Games* among economists. To summarize developments after the publication of von Neumann and Morgenstern'swork: scholars interested in mathematical economics widely recognized it as outstanding, but only some of its components drew sustained attention. These include, as seen, the Expected Utility Theory and von Neumann's introductory discussions of topological ideas such as convexity and linearity, and their use in solving optimization problems.

The trajectory of the stable set solution illustrates this pattern. From a general standpoint, this concept was quickly supplanted by the distinct (and conceptually different) notion of Nash Equilibrium. Even within the domain Nash labeled as "cooperative", mathematicians and game theorists developed alternative solution concepts in the Fifties to address those features of social situations that were problematic in von Neumann and Morgenstern's framework.[41]

Indeed, as shown by empirical tests conducted at RAND in 1952, it was not easy to assess the von Neumann–Morgenstern solution because it was not entirely clear what the theory asserted (Leonard 2010, p. 328; Kalisch et al. 1952). Mathematicians and game theorists thus recognized that the solution concept was too broad to yield an adequate predictive or descriptive account of real-world situations.

Von Neumann and Morgenstern treated the possible existence of infinitely many solutions as reflecting the fact that a rational player may face multiple equally adequate courses of action. To avoid this conclusion, which would undermine practical applications of Game Theory, many alternative solution concepts for the same kinds of social situations were proposed. Some, such as the "Core" or the "Shapley Value," were readily extended to political analysis. Yet none seemed decisively superior to von Neumann and Morgenstern's solution, at least in Riker's eyes. Accordingly, he grounded his analysis of political coalitions in a specific subset of their framework (Riker 1962b).

Theory of Games nonetheless attracted a remarkable set of reviewers, including Herbert Simon Leonid Hurwicz (future Nobelists), Jacob Marschak, the statistician Abraham Wald, and mathematicians such as

[41] Duncan Luce and Howard Raiffa's *Games and Decisions* represents the first comprehensive analysis of early game-theoretical contributions up to the mid-Fifties 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 Eighties, the list presented totaled 8 main concept, which partially overlapped that provided by Luce and Raiffa (Shubik 1984).

Arthur Copeland.

Simon's review appeared in the *American Journal of Sociology*, while Copeland's was published in the *Bulletin of the American Mathematical Society*. Both came out in 1945, together with the perhaps most famous review by Hurwicz in *The American Economic Review*. In 1946 and 1947, Jacob Marschak at the Cowles Commission[42] and Abraham Wald published their reviews, the latter partially reprinted in Shubik's collection (Simon 1945; Copeland 1945; Hurwicz 1945; Marschak 1946; Wald 1947; Shubik 1954). Marschak's and Wald's reviews played an important role in the early diffusion of von Neumann and Morgenstern's work among economists, even if the initial enthusiasm among mathematical economists soon gave way to other developments, most notably general equilibrium.[43]

Simon's review had less impact. Yet its perspective is particularly interesting because Simon approached this work as a social scientist rather than as an economist. Copeland's review, by contrast, largely restates the essential points of the theory. Still, Copeland's role in disseminating *Theory of Games* within the mathematical community beyond Princeton, it will be shown, is significant and often neglected.

Herbert Simon is well known among social scientists: he coined the notion of "bounded rationality" and was awarded the Nobel Prize in Economics in 1978. Less often noted is that he was trained neither as an economist nor as a mathematician. He was a political scientist educated in Merriam's "Chicago School," although his research interests centered on policy-making and organization theory (Simon 1996). Simon begins his review by explicitly recognizing the importance of this work for the 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 acknowledged the rise of mathematical economics while also noting that no comparable effort existed in other social sciences, aside from the work of figures such as Talcott Parsons or the quantitative sociologist Stuart Dodd, whose formalism was not mathematical. 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 emphasized the mathematical novelty of von Neumann's approach, namely his use of set theory and topology rather than calculus and differential equations.

In Simon's view, the second chapter, where the authors describe both informally and formally the notion of "game", contains the most important contribution of von Neumann and Morgenstern's work to the 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*

[42] See the first chapter

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

> *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)

Within this scheme, it becomes 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) Simon also pointed to its relevance for a theory of administrative behavior. Turning to the general solution of n-person games, Simon noted that "the concept of 'stability' [...] is perhaps not entirely free from objections in its details" but that "it certainly points in a proper direction," while also offering a precise account of coalition formation (ibidem).[44]

Simon went further, providing a list of sociological and political topics that could potentially be addressed using game theory: "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 a mathematician rather than a social scientist. He therefore offered a long (16-page) and relatively detailed exposition of the *Theory of Games* (Copeland 1945). Copeland's primary research field was probability theory. Writing for a mathematical audience, he limited himself largely to describing the content of the work, including some notation. Unlike Simon, he did not propose specific applications to social-scientific problems.

Copeland nevertheless had a particular interest in voting theory. He proposed a pairwise voting method later labeled after him.[45] More importantly for the subsequent dissemination of Game Theory, Copeland was Howard Raiffa's Ph.D. advisor at the University of Michigan. Raiffa, as seen, later co-authored with Duncan Luce the most influential Game Theory textbook prior to the boom of Game Theory in economics, *Games*

[44] However, in a personal exchange he had with Morgenstern, Simon was more critical on this point. (Leonard 2010, pp. 260–1)

[45] 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

and Decisions. Raiffa recalled that Copeland chaired a small seminar in 1948-9 and 1949-50, devoted to game theory, with particular attention to 2PZSG and their extensive form (Raiffa 1992). Copeland's influence on the development of Game Theory was therefore indirect, but not negligible.

However, when Shubik was asked to edit a collection of essays intended to show possible extensions of game theory to political behavior, he included neither Copeland's comprehensive review nor Simon's suggestive remarks. He opted instead for Wald's equally valuable review, and complemented strictly game-theoretic readings with related theoretical work not strictly in Game Theory, such as Arrow's analysis of social choice and Duncan Black's essay on the unity of economics and political science.

2.2 Formal Theories of Politics in the Fifties

Game theory gave an important impulse to the development of mathematical economics, although the results were, as seen, quite different from the authors' original expectations. Parallel to this scientific development, formal approaches were also extended to the political and social sciences during the Fifties.[46] The fields involved included the study of collective choice and voting behavior, as well as long-debated issues such as "power."

The Fifties opened with the publication of Kenneth Arrow's innovative analysis of Social Choice, *Individual Values and Social Choices*, but already in the late Forties the Scottish economist Duncan Black had addressed similar problems. In 1954, the Princeton and RAND-based game theorist Lloyd Shapley,together with Martin Shubik, published a brief paper in the *American Political Science Review* in which, drawing on Shapley's general solution for n-person games, they proposed an innovative formal analysis of power. Finally, the study of international politics increasingly relied on game-theoretic techniques, aiming at greater analytical precision in the treatment of Cold War issues.

Black's work and that of Anthony Downs became foundational for the field of spatial analysis of voting and elections. In the same period, James M. Buchanan and Gordon Tullock laid the groundwork for what would become known as the "Public Choice" approach in economics (Arrow 1951b; Black 1958; Downs 1957; Buchanan and Tullock 1962).

As shown above, these formal theories did not become mainstream within political science during the Fifties. Only after Riker's intellectual commitment and his appointment at the University of Rochester did the formal approach, what he labeled "Positive Political Theory", emerge as a clearly defined subfield of the discipline. Moreover, there were important differences among the early attempts to develop formal political analysis. The most relevant concerned the role of Game Theory. As will be shown in the next chapter, Riker encountered Game Theory in the second half of the Fifties and became deeply committed to it, adopting at times a distinctly "lobbyist" stance toward its application to political analysis. By contrast, the works of Arrow, Black, and Downs did not employ Game

[46] A few words defining the meaning of "formal" will be spent in the next section

Theory directly, despite drawing on some closely related ideas. The case of Shapley and Shubik is again different and will be discussed in relation to Riker in the following chapter.

2.2.1 Economists and Political Scientists

As economists' language became increasingly mathematical, some scholars came to regard this as the natural, or at least the most effective, language for the social sciences as a whole. With varying degrees of awareness of the long intellectual traditions that preceded them (especially in Arrow's case), they revisited longstanding political questions using the powerful tools of mathematical formalism. At the same time, although the "Behavioral Revolution" profoundly shaped the environment of American political science, it never assumed a formal character. This divergence widened the gap between economics and neighboring disciplines. As a result, contributions made by economists were not easily absorbed into political science during the Fifties.

It is difficult to specify what it means "to be formal" in disciplines other than mathematics. As a large body of scholarship has shown, the development of mathematical economics was characterized by the adoption of a well-defined set of techniques and tools. As discussed earlier, this process was reinforced by explicit connections to a particular conception of mathematics, namely the "formalist program" and its variants (such as Bourbakism). At the same time, however, this path, as well as its outcome, was far from inevitable or unique (Weintraub 2002 and Ingrao and Israel 1987).

In the present discussion, a broader definition of "formal" is adopted. By "formal political theory", it is meant a kind of political theory grounded in individual action, choices, and preferences, that is, broadly speaking, in economic theory. In a letter written in the early Sixties, Riker defined "formal" as meaning that a theory can be expressed in algebraic rather than purely verbal terms ("Supplementary Statements", Riker to Tyler, December 4th, 1959: Riker 1959b; Riker n.d.). This definition also implies that reliance on mathematical and logical proofs is a necessary condition for a well-formulated emerging theory.

A consequence of this definition is that, among the authors mentioned above, only Arrow's Social Choice Theory fully qualifies as formal in this sense. Black's use of mathematical language was less sophisticated than Arrow's, even though he rigorously proved some of his results, most notably the "Median Voter Theorem." Downs's work, by contrast, was almost entirely verbal, despite his training as an economist.

These works were produced within economics by scholars trained as economists (with Arrow representing the cutting edge of mathematical economics).[47] At the same time, these authors were well aware of the existence of an established research tradition in political science. Consequently, they often justified their use of formal analysis through methodological

[47] The only exception is Gordon Tullock, who was trained as a legal scholar

arguments, or by stressing the close affinity between traditional political problems, such as voting, and collective choice issues long studied by economists, particularly in welfare economics.

To address some terminological issues: what has been described above as formal theory was labeled by Riker, already in the late Fifties, as "Positive Political Theory," or "Formal, Positive Political Theory" (Riker 1962b, p. 33). Formalism was its defining feature. Yet authors such as Black, Buchanan, Tullock, and Riker himself never provided a precise definition of what "formal" meant, often implying that it referred to "what economists do." Before the label "Positive Political Theory" became widespread in American political science (and "Public Choice" in economics), other terms were employed. Black spoke of a "Pure Science of Politics" (Black 1950; Black 1958); Tullock referred to a "Strict Theory of Politics" (Buchanan and Tullock 1962); and Riker invoked the idea of a "Genuine Science of Politics" (Riker 1962b). All these expressions indicate a shared ambition to make political science genuinely scientific. Economics, by then the only social science in which formal modeling was widely established, naturally appeared as a model to emulate. However, this relationship with economics proves more problematic, especially with regard to the connection between formal theory and empirical validation in Riker's work.

Political scientists did not begin to look to economics as a model only in the Fifties. One may recall, for instance, Graham Wallas at the London School of Economics (Wallas 1920). Similarly, Charles Merriam, in comparing the methods of political science with those of other disciplines, referred explicitly to economic analysis. In a 1923 article in the *American Political Science Review* on "recent advances in political methods," Merriam,like Wallas, described economics as moving away from a priori reasoning (characteristic of classical political economy) toward the progressive integration of statistical analysis and psychological insights (Merriam 1923). For Wallas and Merriam, however, undoubtedly both central figures in the early twentieth-century push toward a more scientific political science, economics was not to be followed into the realm of "high theory," but rather in the more concrete domain of quantitative and empirical analysis. Moreover, they encouraged political scientists to look not only to economics but also to disciplines such as biology and anthropology, as well as historical and sociological studies. Finally, both emphasized psychological insights in economic models of choice, presenting a somewhat simplified account of the development of economics that would later be echoed in the behavioral revolution.

The dramatic growth of formal modeling in economics from the late Thirties onward made it possible to reframe classical political issues in mathematical terms. The mathematical turn of the Fifties aimed to define political problems with greater precision and to provide internally consistent theories of how political systems operate and resolve conflict. The tools developed within economic theory, starting with Game Theory, appeared well suited to this task. Yet the emphasis on formal reasoning tended to downplay psychological and behavioral considerations, replacing them with concerns about logical consistency. This marked a significant departure from

the views of Wallas and Merriam and highlights the distinctive character of formal modeling in the Fifties and Sixties.[48]

2.2.2 Social Choice and Voting: selected aspects of Arrow, Black, and Downs

This analysis is not intended to be exhaustive. Rather, the focus is on some methodological features of their models. Arrow and Black, as will be shown, made partial use of ideas derived from the *Theory of Games*. In particular, Arrow' treatment of rational choice closely resembles the axiomatization of utility theory developed by the founders of Game Theory. Black, for his part, also addressed strategic considerations in politics, though his conception of rationality differs markedly from Arrow's.

Although his most influential book appeared in 1958, Black's early results on voting theory date back to the late Forties. For this reason, the discussion begins with him. Duncan Black occupies a central position among scholars who analyzed the relationship between individual choice and collective decision-making. Together with the American economist Anthony Downs, he is commonly regarded as a founder of spatial analysis in political science.[49] The "spatial" aspect refers to the representation of voters' preferences as locations in an issues space, often depicted using Cartesian coordinates. Under these assumptions, Black derived a simple but powerful result: in majority-rule decisions on a single issue, the median voter's preference is decisive. Downs generalized this insight to one-dimensional party competition, where parties converge toward the median voter. These results were later extended and formalized, often using game-theoretic tools, from the Sixties onward.

Black's early results are also connected to Arrow's arguments regarding what he called the "General Possibility Theorem for Social Welfare Functions" (Arrow 1951b). Although voting paradoxes had been identified much earlier (most importantly by French eighteenth-century authors such as Jean Charles de Borda and Condorcet) the works of Arrow and Black marked the emergence of a new subfield: the analysis of collective choice through rational choice methods (Black 1958; McLean 2015).[50]

This parallel notwithstanding, there are significant differences between Arrow's work and Black's, beginning with the scope of their research and the degree of generality and mathematical sophistication. Black was closer

[48] Note, however, that these issues were never entirely separated, either in economics or in decision theory. See, for instance, Moscati 2018. As will become apparent in the discussion of Riker's work, this tension also bears on questions of empirical validation and the "positive" aspirations of formal theory.

[49] The American statistician and economist Harold Hotelling anticipated some of these ideas (Hotelling 1929). Hotelling directly influenced Downs, whereas Black focused on committee decisions and voting. However, Hotelling's original contribution concerned a different problem, namely spatial competition in a duopoly with homogeneous goods, but he also tentatively advanced political analogies. Notably, Hotelling was Arrow's Ph.D. supervisor at Columbia University (on Hotelling see Gaspard and Muller 2021).

[50] Black devoted the entire second part of his 1958 book to reconstructing these debates historically, up to Victorian Britain.

to the idea of a "genuine science of politics," later emphasized by Riker, and can therefore be read as a precursor of both "Positive Political Theory" and "Public Choice." Indeed, in the Sixties he became well acquainted with both intellectual communities. Whereas for Arrow voting was one example among many of collective choice based on individual preferences (namely axiomatized social welfare functions), for Black it was a positive phenomenon to be analyzed through formal theory and, crucially, through empirical inquiry.[51]

Black graduated from the University of Glasgow, where he studied mathematics and physics but developed a strong interest in economics and politics. In the Thirties, at the Dundee School of Economics, he formed a lifelong friendship with the future Nobelist Ronald H. Coase.[52]

Because he was trained in the natural sciences more intensively than many economists of his generation and displayed a pronounced interest in analytical methods for the social sciences, Black aimed to develop a "pure science of politics." This was also the title of an early draft of his major work, later published in 1958 under the less ambitious title *The Theory of Committees and Elections* (Black 1958). The 1958 volume collected papers written in the second half of the Forties and published in leading journals such as *Econometrica*, as well as in the *Journal of Political Economy* and the Italian *Il Giornale degli Economisti*(Black 1948c; Black 1948a; Black 1948b). He also produced a more detailed analytical study with the physicist R. A. Newing, titled *Committee Decisions with Complementary Valuation.* In addition, a long series of papers, many unpublished at the time or published only much later, must be included (McLean, McMillan, and Monroe 1998; Brady and Tullock 1996). Black's early papers were written and published precisely when mathematical economics was taking off within the American economics community. It is therefore unsurprising that they attracted the attention of young scholars, most notably Arrow, who later generalized related results in a different formal framework (Arrow 2014).

Black spent his entire career in the United Kingdom, moving between Belfast, Glasgow, and the University of North Wales at Bangor. He nevertheless served as a visiting professor at several 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 within British political science, he maintained long and fruitful exchanges with American scholars interested in voting theory.[53]

51 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. [...]" (Arrow 1951b, p. 79)

52 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 Thirties with a vocational orientation, see Tribe 2022).

53 For biographical information about Black, see Coase's preface to McLean, McMillan, and Monroe 1998 and the first section of the editors' introduction in the same volume, as well as Coase 1994. On Buchanan and the Virginia group in the Fifties, see Levy and Peart 2020.

In Black's view, the scope of political science as a discipline is to develop a method for aggregating preference schedules. Accordingly, the starting point of the analysis is the individual, treated as equivalent to a schedule of preferences (in economics, preferences concern goods; in politics, they concern motions among which choices are made). These ideas appear in a methodological paper published in 1950 and are reprised in his early theoretical writings (Black 1950; Black 1958).

In that 1950 article, Black explicitly advanced "the unity of economics and political science" as a necessary step toward building a "pure science of politics." This view rests on the claim that both disciplines are, in effect, subsets of a broader domain, namely the "Theory of Choices." The core of this scientific approach to politics consists in constructing an appropriate set of formal propositions, beginning with a theory of committee decision-making. Black acknowledged that such a theory would not capture the full workings of all committees, but could yield very general propositions for political analysis. In this sense, he wrote:

> "[. . .] 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, in Black's view, "precisely those of Economic Science" (Black 1950, p. 507), because both disciplines must abstract from empirical complexity in order to handle the social world analytically. What he called "the economic mode of abstraction" proceeds by starting with the simplest problems; only after these are understood can lesser abstraction be introduced. Individual preferences are therefore the starting point of analysis, and these can be represented through preference schedules in the familiar economic style. Although economics concerns situations in which individuals often possess relatively clear information about outcomes (prices and money), Black insisted that "there is no difference in principle between the economic and political estimates which people must make." (Black 1950, p. 511)

Even the central concept of modern economic analysis, equilibrium, can, he argued, be extended to politics. In this context, "political equilibrium" refers to how collective decisions are reached, given individual valuations, through particular voting procedures and institutional adjustments.

The key difference from economics lies in the objects of preference. More specifically:

> "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 nonetheless appears not to have been closely acquainted with Game Theory at the time. Recognizing that a political theory of this kind would eventually need mathematical formulation, he remarked that much existing mathematics had been developed for physical problems and was not well adapted to the human sciences; therefore, "in time a new Mathematics will be invented" (Black 1950, p. 513, footnote 2). This is a classical, and in some respects surprising, claim, echoing older objections to the mathematization of economics (for instance, the Austrian critique associated with Hans Mayer, well known to Morgenstern). It also parallels the objections addressed by Morgenstern in the first chapter of the *Theory of Games*.

In later works, especially in his 1958 book, Black did gesture toward strategic behavior in politics. In particular, his "Median Voter Theorem" can be framed as a non-cooperative game in which the median preference corresponds to the unique equilibrium outcome. Yet this is not how Black developed his account of strategy. Rather, he focused on parties: parties adapt to institutional environments and may dissimulate their preferences, a point that naturally invites game-theoretic analysis. Black, however, did not formalize this in game-theoretic terms. He was not closely engaged with game-theoretical work and appears to have been skeptical about its ability to yield exact results.[54]

Black's conception of rational behavior further confirms his distance from Game Theory. In the *Theory of Games* there was a visible tension between expected-utility foundations and the notion of rational behavior in n-person games. Still, the axiomatic grounding of utility theory became one of this work's most influential legacies, particularly for Arrow. Black's approach differs. His analysis rests on individual rankings of preferences, but it does not provide a rational choice theory or a formal definition of rationality, either axiomatic (as in Arrow) or as utility maximization. He simply maintained that it would be irrational for an individual to choose an alternative that she does not prefer; hence, if a voter is indifferent between two motions, abstention may be assumed (Black 1958, p. 5).

[54] This is suggested by his exchanges with Ronald Coase when Coase was working on the theory of social cost. Black proposed that the relationship between efficient allocation and initial rights assignments could be addressed using game theory, but added that "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)

Black's views on rationality became explicit in a 1969 paper on Arrow's Impossibility Theorem (Black 1969). There he defended his preference for the term "transitivity" over "rationality," writing:

> "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)

As it may be apparent from what said so far, Arrow's work paralleled, and in a certain sense generalized, Black's. Arrow focused on the apparent inconsistency between individual preferences and social choices when the number of possible alternatives exceeds 2 and any admissible ranking of preferences is allowed. As he later recalled, the origins of his interest were twofold: his mathematical training as an economics graduate student at Columbia University under Harold Hotelling, and the stimulating intellectual environments of the Cowles Commission, where he spent time in 1948, and RAND, in 1949 (Arrow 2014). At RAND, scholars such as the philosopher Olaf Helmer conducted game-theoretic analyses with particular attention to international politics. Helmer, 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." (Arrow 2014, pp. 147–8)

Arrow proved that, in an important sense, Helmer's concern was well founded. Bergson had shown how welfare conditions could be derived without summing utilities (and thus without interpersonal comparisons), an approach that contrasted with the earlier Cambridge tradition associated with Marshall and Pigou, and with Pareto's critique. Arrow however, pushed the analysis further and argued that even Bergson's framework was problematic once examined through an axiomatic approach inspired by the mathematician and logician Alfred Tarski.[55] This result, which

Arrow called the "general possibility theorem for Social Welfare Functions," formed the core of his Ph.D. dissertation and was published in 1951.[56]

Although Arrow soon turned to other areas, most notably general equilibrium theory, his elegant argument quickly attracted the interest of young economists, who developed refinements that weakened or modified the conditions yielding inconsistency.[57]

In his 1951 work, Arrow explicitly compared voting and the market mechanism as two forms of collective choice. Both, in this view, are special cases of a broader category of social choice procedures. This parallel notwithstanding, and despite references to political science literature, his work did not belong to political science. Its scope was intentionally general, and its starting point was a critique of welfare economics as a coherent toolset for social policy (Igersheim 2019). Arrow was not primarily seeking to equate political and economic action beyond the fundamental claim that both can be represented as formally defined rational choice.[58]

He summarized the problem 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?"(Arrow 1951b, pp. 1–2)

The simplest illustration is Condorcet's voting paradox. With three voters and three alternatives, each voter's preferences can be transitive, and yet majority rule yields a cycle.[59] Arrow observed: "So the method just outlined for passing from individual to collective tastes fails to satisfy

55 By contrast, Bergson's analysis relied on a more classic calculus-based maximization (Bergson 1938).

56 The better-known label is the "Impossibility Theorem." Arrow stressed, however, that his result also identified conditions under which social choice remains possible; it is therefore less pessimistic than the conventional name suggests.

57 Arrow contributed extensively to general equilibrium theory and, together with Debreu (and McKenzie), provided a foundational existence proof (Arrow and Debreu 1954; Düppe and Weintraub 2014b).

58 Arrow noted that he was not the first to compare economic and political choice. He listed authors such as Herbert Zassenhaus, Howard Bowen, and Frank Knight. His discussion also underscored that earlier comparisons often remained socio-psychological rather than formal. Arrow 1951b, pp. 5–6.

59 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.

the condition of rationality, as we ordinarily understand it." (Arrow 1951b, p. 3). This is also central to the critique of welfare economics.

Arrow introduced simplifying assumptions: individual values (and preferences) are taken as given and not shaped by the analysis; and individuals are assumed to be rational. His argument did not rely on game theory, although he was aware of von Neumann and Morgenstern and recognized possible game-theoretic routes. One route concerns strategic manipulation within voting systems (for example, under plurality voting, sincere preference revelation is not guaranteed). Another concerns "games of fair division," where rules are designed so that rational play yields a fair outcome (Arrow 1951b, p. 7).

Despite his reliance on rationality assumptions, Arrow framed individual choice through "ordering relations" and pairwise comparisons among "social states." His approach therefore differs from von Neumann's Minimax framework and does not proceed via strategy sets and equilibrium existence in the same manner (Arrow 1951b, pp. 19–21). He also rejected interpersonal comparisons of utility and downplayed the welfare relevance of expected utility theory. In discussing von Neumann and Morgenstern, he wrote:

> "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.* (Arrow 1951b, pp. 9–10, italics added)

Arrow concluded that behavior in the relevant setting could be represented with "a preference scale without any cardinal significance" (Arrow

1951b, p. 11).

Without entering into the details of Arrow's formal proof, it is important to briefly highlight his broader methodological view on mathematization. An essay Arrow published as a Cowles Commission working paper and later in a collection edited by Harold Lasswell presents this position clearly (Lasswell and Lerner 1951; Arrow 1951a). For him, the application of mathematics to natural or social phenomena rests on the notion of "model": a class of admissible structures capturing relations, from which mathematics rules out incompatible claims. Mathematics offers "superior clarity and consistency" because it forces explicitness about the components of a model, even while acknowledging the inevitable simplifications of formal representation (Arrow 1951a, 129 et ss.). It also helps address the classic tension between the individual and the collective. Following Koopmans and postwar econometrics, a full characterization of individual behavior logically implies knowledge of group behavior, while empirical analysis can consistently recover aspects of individual behavior (Arrow 1951a, p. 133). Arrow also stressed the need for statistical testing of model implications: formal modeling provides "the opportunity to tap the great resources of modern theoretical statistics as an aid in empirical validations." (Arrow 1951a, p. 133)

To conclude, let's briefly consider Downs. Like Black (though without citing him), Downs wrote as an economist seeking to explain the "rationale of government activity" using the familiar categories of producers and consumers. He described his model as "a study of political rationality from an economic point of view." (Downs 1957, p. 11) He defended the economic approach mainly by listing properties of rationality (completeness, transitivity, maximizing behavior, and intertemporal consistency) though his discussion remained largely verbal.[60] Voters are rational insofar as they maximize their political preferences, and elected officials are rational insofar as they maximize their chances of re-election.

This conception is tied to what Downs called the "self-interest axiom" (Downs 1957, p. 27). His aim was to provide a positive account of political behavior in voting and party competition. In this respect, the assumptions of rationality and self-interest function as simplifying devices for explanation. Unlike Black and Arrow, however, Downs did not engage deeply with cyclical preferences or with social choice theory more generally.

His deductive logic also supports a critique of functionalist explanations of institutions. Following Schumpeter, he stated that "social functions are usually the by-product, and private ambitions the end, of human action" (Downs 1957, p. 29).[61] Accordingly, the logic of voting follows from the self-interest axiom: citizens vote for the candidate they believe will maximize their benefits. If benefits are interpreted as utility, each citizen derives a

[60] These are technical definitions. In practice, Downs's treatment of rationality and self-interest is mostly verbal.

[61] Schumpeter argued that democratic politics is best understood as a "competitive struggle for power and office," which then incidentally fulfills broader social functions, in a way analogous to profit-seeking in markets (Schumpeter 1942, p. 282).

"utility income from governmental activity," and voters act to maximize that utility.

This suffices enough to show that Downs did not explicitly argue for the unity of economics and politics in the manner of Black. The contrast with Arrow is equally clear: Arrow focused on the formal equivalence of different collective choice mechanisms (markets and voting), whereas Downs concentrated on the operation of democratic institutions and the incentives within them.

These differences notwithstanding, it can safely be said that the works of Arrow, Black, and Downs helped pave the way for Riker's later agenda in political science and constituted one of the main strands from which "Positive Political Theory" emerged.

Chapter 3

William H. Riker and the postwar Political Science

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

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

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

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

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

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

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

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

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

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

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

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

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

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

3.1 Riker in the Fifties: from Harvard to Rochester

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

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

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

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

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

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

3.1.1 Harvard and Lawrence College: graduate years and early works

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

In a paper written to reconstruct the history of Game Theory's entry into political science, Riker provided a somewhat different account of how he became acquainted with Game Theory (Riker 1992). He traced this acquaintance to his reading of Shapley and Shubik's paper in the *American Political Science Review* on power distribution in committee systems (Shapley and Shubik 1954; Shubik 1954). This paper, though not

excessively technical, was nonetheless strictly theoretical and rested on cooperative *n*-person solution concepts developed by Shapley. Alongside it, Riker also read Kenneth Arrow's work on Social Choice, and these readings led him back to von Neumann and Morgenstern's foundational text. As he recalled: "There I discovered what I thought that political science needed for constructing theory" (Riker 1992, pp. 207–9).

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

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

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

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

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

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

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

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

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

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

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

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

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

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

3.1.2 1960-1: Research Fellow at Stanford

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

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

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

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

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

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

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

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

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

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

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

[13] I will examine these two works below.

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

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

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

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

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

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

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

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

17 Actually Black was Scottish.

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

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

> "I'm in an ideal position this year to do this. Ken Arrow is at Stanford, and I gather from conversation that he has kept up with the rain of articles occasioned by his theorem (which was published in 1951). Clyde Coombs, who has pondered the problem of summing preferences as much as any psychologist is at the Center this year. And there is a statistician here who can guide me through the literature on inconsistent triads. With the help of these people, I think I could bring together the work in economics, psychology and statistics and focus it on political theory. I don't pretend to be an authority on this subject, of course, although I probably know more about it than any other political scientist except Black. But I'm pretty certain I can interpret for political theorists the significance of the paradox of voting (and of attempts to bypass it).[...] My concern is not so much to praise Black, although he does deserve more notice than he got, as it is to render political theorists aware of the importance and significance of work in this area. Since Black's first articles were published in 1948, there have been, I suspect, about one hundred more on the problem of adding votes or preferences or utiles. Yet only 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 the *American Political Science Review*, Thomas Eliot of Washington University in St. Louis (MO), accepted Riker's proposal promptly. Riker then sent a copy of the project to Duncan Black, asking for bibliographical advice. Black responded supportively, even though he could not offer extensive additional references (Black to Riker, March 13,Riker n.d., Box 5, Folder 1). In the same exchange, Black also noted that he had "began looking into the Theory of Games to try to get a link-up with Voting and made a collection of some of the literature." (ibidem) Consequently, in spring 1961, Riker wrote an "interpretive bibliography" intended to present the post-Arrovian developments in social choice and voting theory, although without heavy mathematical detail.

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

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

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

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

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

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

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

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

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

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

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

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

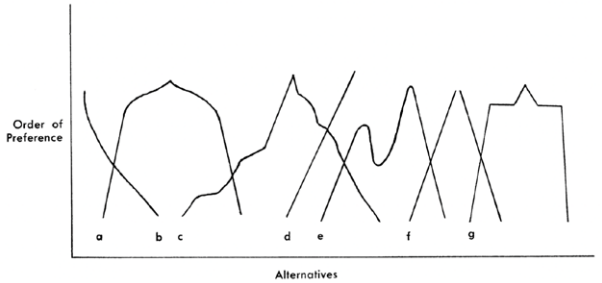

FIG. 1. Examples of preference Curves

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

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

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

As noted above, Black offered one of the most successful strategies for avoiding cyclical majorities by restricting the admissible domain of preferences. In particular, he showed that simple majority voting produces a unique transitive outcome if individual preference orderings are single-peaked. By "single-peakedness," Black meant a curve with a single optimum point. Since each curve represents a committee member's preference ranking, the intuition is that "the further a motion departs to the right (or to the left) of a member's optimum, the less he desires that that motion should be adopted." (Black 1958, p. 10) Riker summarized the condition

as follows: "By 'single-peaked' is meant that all the individual orderings can be represented on a graph with the rank of preference on the ordinate and the alternatives themselves on the abscissa such that each ordering (or preference curve) appears as a curve with one and only one peak." (Riker 1961, p. 906). Single-peakedness is a necessary condition for Black's median voter theorem.[21]

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

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

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

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

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

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

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

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

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

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

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

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

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

Well before he arrived at Stanford, Riker had already begun working on several of the issues mentioned above. Those lines therefore condense the

outcome of a sustained period of reflection and study that led him toward a more explicit commitment to formal analysis.

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

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

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

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

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

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

in his later methodological defenses of formal political theory.

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

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

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

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

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

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

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

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

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

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

Even so, the two positions are not identical. In the *Theory of Games*, the move toward "modest" problems is tied directly to individual behavior

and simple forms of exchange. In Riker's 1957 paper, by contrast, the delimitation of events does not yet imply a substantive theory of agency.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

and connects these outcomes to four interpretive hypotheses:

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

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

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

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

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

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

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

Chapter 4

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. Only in the Eighties did Riker publish other general works, although his focus shifted toward social choice analysis, political theory, and American history.

This book was an extraordinarily ambitious project. Its goal was to construct a theory of coalitions grounded in exact and verifiable assumptions, using an "existing general theory of coalitions (the theory of n-person games)." (Riker 1962b, p. viii) At the same time, is also a demanding and often difficult text, both for contemporary readers and, perhaps even more, for its original audience. Much of its formal analysis is overly verbal, and for this reason the proofs of Riker's claims are not always easy to follow. Compared with Neumann and Morgenstern, whose pages are much more demanding but also far more rigorous, Riker's exposition offers little of the excitement a young scholar comfortable with mathematical reasoning could feel when following Neumann's exact argument. Instead, this book relies on a limited and sometimes superficial use of game-theoretical notions. This shortcoming was already apparent even to a reader less mathematically sophisticated than Neumann, Nash or Shapley like Morgenstern..

Despite this, Riker's effort deserves considerable consideration. In the first half of the book, he argues that political actors will create coalitions just large enough to ensure winning, and no larger. This is the notion of "Minimum Winning Coalitions", from which he derived the "Size Principle": namely that winning coalitions will be constrained in their size.!Minimum Winning Coalition This insight still occupies a central place in the formal study of political behavior and party formation, even if many scholars challenged, as it will be shown, Riker's results on both theoretical and empirical grounds. In the second half of the book, Riker slightly modified von Neumann and Morgenstern's n-person analysis into a set partition of voting members to describe coalition formation as a process, i.e., the strategic steps that precede the creation of a winning coalition. In this part, his ambitions are fulfilled even less than in the first, largely because the analytical problems involved exceeded his technical capabilities. Nevertheless, this section still offers valuable insights into the practical functioning of

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Book References DOI 10.36253/979-12-215-0978-6.references

political systems. In addition, the opening chapter of the book is entirely devoted to methodological issues and to presenting the core features of the model. For all its technical shortcomings, *Political Coalitions* thus stands as the first serious attempt to produce a book-length game-theoretical analysis within political science.

Riker was very explicit about the nature and the scope of his undertaking. Although his analysis systematically exploits von Neumann and Morgenstern's theory of n-person games, it is not ("most emphatically not." Riker 1962b, p. vii) a book about mathematics. He limited himself to employing and adapting some mathematical notation without offering formal proofs. As a result, *Political Coalitions* differs substantially from the high-theory development of Game Theory in the Fifties (such as those associated with Shapley or other RAND theorists). Therefore, it does not occupy a central place in the history of game theory as a mathematical theory. However, it did play an important role in the history of how game-theoretical ideas moved across disciplinary boundaries and beyond economics.

This chapter examine the content of Riker's work, focusing mainly on how he employed the theory of games and by reconstructing his relationship with contemporary game theorists. It will be shown that, although he developed several insightful intuitions about how Game Theory might be integrated into political science, he did not produce new formal theorems. In this respect, his work shares an important feature with that of other scholars who employed Game Theory during the same period, such as Schelling. Unlike Schelling, however, Riker's use of Game Theory was occasionally based on misunderstandings, and some of the analytical tools necessary to fulfill his objectives were sometimes beyond his reach.

This, however, does not undermine the importance of the book. Riker's adoption of cooperative game theory, and in particular of von Neumann and Morgenstern's original concept of the "stable set", was highly original. Moreover, his emphasis on coalition size paved the way for an entire class of models built around the "size principle." Finally, he was among the first to assess several weaknesses in von Neumann and Morgenstern's theoretical framework.

4.1 The genesis of the work

4.1.1 Riker and Game theorists

In the previous chapter, it was examined Riker's academic formation as a political scientist, his dissatisfaction with the state of the discipline in his time, and the circumstances under which he became acquainted with game theory. This section builds on that discussion by focusing more specifically on Riker's early and rather limited relationship with the community of game theorists.

As noted earlier, Riker's only sustained contact with the kind of high-level theoretical work in the social sciences that he aspired to develop occurred during his one-year fellowship in Palo Alto. During his stay in California, he met Arrow and Clyde Coombs, and interacted with several

other fellows who assisted him with the formal aspects of his work, including the statistician David Wallace from the University of Chicago.

With very few exceptions (Riker being the most significant) political scientists and game theorists remained largely separate intellectual communities throughout the Fifties, despite some early attempts to apply game theory to political analysis already in the first half of the decade. In a broad sense, as discussed earlier, such an application is already implicit in von Neumann and Morgenstern's 1944 work (Simon 1945; Shubik 1954). Even so, Riker's position at the intersection of these two communities can be questioned, at least for the Fifties. While he clearly belonged to the community of political scientists, it is doubtful that he can be considered part of the community of game theorists, given both his training and his professional trajectory. Moreover, despite his strong theoretical interests and methodological ambitions, Riker did not align himself with the Behavioral Revolution, the major transformative movement within American political science at the time, choosing instead to pursue an independent theoretical agenda.

During the Fifties, the community of game theorists consisted largely of young mathematicians primarily concerned with conceptual and theoretical developments rather than concrete applications. Institutions such as the RAND Corporation are often described as centers of applied game theory, particularly in relation to strategic and international political problems. In practice, however, RAND executives quickly became strong supporter of pure theoretical research as well. In this context, Riker was, unsurprisingly, very much an outsider. He lacked the advanced mathematical training required to produce original theoretical contributions, and he was never affiliated with RAND, where the few political scientists present focused mainly on nuclear deterrence and strategic studies (the most prominent example being Albert Wohlstetter). Among the institutions that contributed to shaping what has been described as "Cold-War Rationality", Riker's only direct involvement was his fellowship at Stanford's Center.

One might explain Riker's distance from game theorists partly through his early focus on a well-defined problem, namely coalition formation in politics, within a specific framework, cooperative rather than non-cooperative game theory. The latter was instead mainly adopted in international relations. One could also add that Riker was advancing a theoretical agenda in advocating Game Theory that differed substantially from both traditional political science and the Behavioral Revolution. At the same time, his efforts to integrate Game Theory were constrained by his mathematical limitations, which likely deepened his sense of intellectual estrangement within the discipline in the decade.

Riker was not alone in attempting to explain political phenomena through formal analysis derived from economics, even if not always through game theory strictly speaking. As discussed earlier, the same period witnessed the work of Duncan Black, Anthony Downs, James Buchanan, and Gordon Tullock. These efforts were followed in the Sixties by the emergence of a broader community associated with Rochester and institutions such as Carnegie Mellon, where Otto Davis and Melvin Hinich developed influential

models of Downsian spatial electoral competition. Riker established strong intellectual ties with many of these scholars. Nevertheless, the consolidation of this broader community occurred only after his early engagement with game theory and accelerated significantly following his appointment at Rochester.

Riker's recollections in his interview with Shepsle shed further light on his marginal position within the community of game theorists. When asked whether he had ever shared his ideas with contemporary game theorists, Riker recalled only an exchange with Duncan MacRae, whose response, apparently containing detailed criticisms, seems unfortunately not being preserved among Riker's papers stored at Rochester. More revealing evidence of his outsider status, however, can be found in Oskar Morgenstern's papers housed at the Rubinstein Library at Duke University (Morgenstern n.d., Box 83). Although Riker did not mention this episode in the interview, he submitted the manuscript of *Political Coalitions* to both Princeton University Press and Yale University Press. While Yale accepted the manuscript and sent it out for anonymous review to Martin Shubik, as seen, a former student of Morgenstern at Princeton, Morgenstern himself reacted very negatively and advised against publication.

In a letter to Gordon Hubel, then press editor at Princeton, Morgenstern acknoledged that "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, Morgenstern n.d., Box 83) He went on to criticize the manuscript more sharply: "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." (ibidem) He attributed the manuscript's weak mathematical quality to Riker's having worked largely alone, and suggested that Riker either cooperate with a real game theorist or spend time acquiring more specialized training. Before drafting his report, Morgenstern tried to gather information about Riker, his education and research profile but without much success, apparently. He concluded: "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." (ibidem)

Despite Morgenstern's harsh assessment, Shubik's referee report was considerably more favorable, and the manuscript ultimately appeared in print. As will become clear later, however, publication did little to improve Riker's standing among game theorists, since the book lacked the technical sophistication that would have appealed to them. Nonetheless, it succeeded in demonstrating to political scientists that a new way of modeling political phenomena, and of developing political theory more generally, was possible.

4.1.2 Martin Shubik's referee report

Shubik's papers, also preserved at the Rubinstein Library, include the original referee report on Riker's manuscript. Although the report is not especially detailed from a technical standpoint, it nevertheless contains

several important observations. The report consists of four typewritten pages, including a general introduction, a series of mostly stylistic comments, and a brief concluding summary. Equally informative, however, are the issues that the report does not address, as they help clarify the different perspectives from which Shubik and Riker approached political Game Theory.

Yale University Press contacted Shubik at the end of June 1961 with a request to review Riker's manuscript anonymously (Marian Neal Ash to Shubik, 30 June 1961, Shubik n.d., Box 8). It seems that Riker never learned that Shubik had refereed his work, although he suspected it (Riker and Shepsle 1979, p. 14). The choice was unsurprising. Shubik was among the few scholars actively working on n-person Game Theory at the time. Although he divided his efforts between RAND and applied consulting work for large corporations (most notably IBM and General Electric) he was also closely associated with Yale, where he was visiting in 1961 and where he would become a full professor in 1963. He was already well known for his collaboration with Lloyd Shapley on the power index, and in 1954 he had edited, as seen earlier, a volume collecting essays by figures such as Marschak, Wald, Black, and Arrow on game-theoretical approaches to political behavior (Shubik 1954).

In his correspondence with Yale University Press, Shubik accepted the invitation and noted that he was probably already familiar with parts of Riker's work, possibly in the form of earlier drafts (Shubik to Marian Neal Ash, 17 July 1961, Shubik n.d., Box 8). Riker's later interview offers no confirmation of this point. Despite travel commitments, Shubik submitted his report on August 9th, "Referee's report on 'The Theory of Political Coalitions' by William H. Riker," (Shubik 1961 in: Shubik n.d., Box 9). As he explained in his earlier reply, his quick turnaround was facilitated by his prior familiarity with parts of the manuscript.

Shubik began his report with strong praise, describing the manuscript as "well worth publishing" and predicting that it would be "a rather controversial book containing several imaginative ideas." He emphasized that its value lay in "an imaginative insight concerning the application of the methodology of game theory to the subject matter of political science" (Shubik 1961, p. 1). In his view, this methodological innovation could help shield the book from what he anticipated would be unfair criticism from both political scientists and game theorists on purely technical grounds.

This emphasis is telling. Shubik's defense of Riker's work focused primarily on its methodological ambition rather than on its technical rigor. He remarked that the discussion of Minimum Winning Coalitions might be "too verbal" for part of its intended audience while still not being verbal enough for the rest. He also questioned how effectively Riker's use of earlier literature, such as the work of Raiffa and Vickrey on coalitions, translated into a coherent theory of political behavior.

Among Shubik's more substantive criticisms, the most important concerned Riker's heavy reliance on the zero-sum framework. Shubik argued that much of the analysis would remain valid in non-zero-sum settings, since what truly mattered was the potentially indeterminate duration of

political interaction. The zero-sum structure, by contrast, applies only to a limited and finite segment of the interaction. More generally, opposition of interests can arise not only in zero-sum and constant-sum games, but also in "more or less strictly competitive non-constant sum" games (Shubik 1961, p. 2).

Other remarks, though more localized, are equally revealing. Shubik challenged the assumption that "side payments" necessarily involve money, while acknowledging that this misconception was widespread in Game Theory. He also criticized the inclusion of threats as a form of side payment, arguing that this leads to a poor model of human behavior: "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 ultimately recommended publication with only minor editorial revisions and offered a cautious assessment of the book's likely 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).

Shubik's identity as referee was not disclosed, and there is no evidence of correspondence between him and Riker either before or after the review process. Given the general nature of several comments and their attachment to the manuscript rather than to the published text, it is difficult to determine which suggestions Riker adopted, aside from the explicit inclusion of "threats" among side payments. In the published version, "the threat of reprisal" indeed still appears among the types of side payments discussed (Riker 1962b, 109–10).

The report is valuable for several reasons. For the historian, it provides an informed evaluation of Riker's work by one of the most prominent game theorists of the period. Shubik was arguably an ideal referee: as he repeatedly acknowledged, his strength lay more in economic model building than in highly formal mathematical economics, which might have led another reviewer to focus almost exclusively on technical deficiencies. A different referee might, for instance, have emphasized the fact that coalition theory was being developed at the time with far greater mathematical sophistication in general equilibrium research.[1] Such criticism, however, would have largely missed Riker's objective. He was not attempting to construct a mathematically refined theory of political coalitions, but rather to generate new insights into political processes through the modeling of rational behavior, in line with the methodological reflections laid out in the book's opening chapter. Shubik, despite his distance from political science, clearly grasped this intention, as his report shows. Morgenstern, by contrast, apparently did not.

[1] Notice that Shubik himself contributed to these theoretical developments (Cogliano 2019).

4.2 The assumptions of Riker's model

4.2.1 The "main hope for a genuine science of politics"

Riker devoted the first chapter of his work, significantly titled "The prospect of a Science of Politics", to outlining both the methodological premises of his project and the main features of his model. These pages can be read as a complement to his earlier philosophical papers, and they include a broader discussion of formal modeling and the meaning of rationality.

According to Riker, this chapter "is the most important part of the book." (Riker and Shepsle 1979, p. 15) That claim is somewhat surprising, given the theoretical ambitions of his formal analysis. It can be read as an implicit admission of the mathematical weakness of the argument, but also as a signal of what he regarded as essential: the role of rational choice. As Riker put it, this meant "the notion that people make calculations about what is good for themselves, and try to act based on those calculations." (Riker and Shepsle 1979, p. 16) Downs, Buchanan, and Tullock had advanced closely related views of rationality in politics. Riker, however, coupled his account of rational behavior with a more systematic discussion of Game Theory in politics, moving beyond Down's "self-interest axiom" or Buchanan and Tullock's "economic nexus."

He began with a fairly classical appeal to Physics as a role model for every theoretical and applied science. In Riker's view, Physics is characterized by the existence of a "body of related and verified generalizations which describe occurrences accurately enough to be used for prediction." (Riker 1962b, p. 3) Predictions can be formulated by deducing such generalizations from a set of axioms and then verifying them, despite unavoidable difficulties, through experiments and observation.

Unlike the natural sciences, Riker argued, the social sciences face three major difficulties. The first concerns the normative considerations that permeate any attempt to produce a positive analysis of human affairs: the chief consequence is the impossibility of prediction. The second and third difficulties involve themes already discussed in his earlier philosophical essays: the size of events and the problem of causal determinism. Thus:

> "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)

For Riker, any attempt to create a science of politics requires, first, a genuine understanding of scientific method.[2] In line with modern developments in the natural sciences and some of the social sciences, the primary tool for rendering political science "scientific" is 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." (Riker 1962b, p. 5)

In other words, one builds models: sets of axioms (more or less intuitively defensible) from which one can deduce non-obvious general propositions. Once verified, these propositions become both additions to the model and descriptions of its structure. They also allow scholars to reduce, or at least attempt to reduce, normative elements, while simplifying 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 and Psychology, for Riker, offered the most relevant examples of social sciences that had become "scientific." Economics, in particular, had developed models of individual behavior, such as consumer theory. Therefore, an analogous model of political behavior could also be constructed. This, he argued, was the "main hope for a genuine science of politics." (Riker 1962b, p. 9)

If political analysis is to follow the economic approach, it must be grounded in individual action. The first step, however, is to define politics itself. Riker adopted as especially suitable the definition proposed by his former colleague at the Harvard Graduate School, David Easton: politics as "the authoritative allocation of values." (Easton 1953; Riker 1962b, p. 10) Despite methodological divergences, Riker argued that this definition captured—through the notion of "allocation"—a central feature of politics: it is an activity, not merely a static study of governmental forms, law, or history. Moreover, it does not discard older traditions in political science, but rather subsumes them.

Most interestingly, Riker also drew an implicit parallel with the well-known definition of economics proposed by Lionel Robbins in 1932 (even though Robbins is not mentioned in the text), namely economics as "the allocation of scarce resources." (Robbins 1932) The parallel rests on the idea that both economics and political science concern "allocation." Yet, for Riker, allocation is not merely a physical process; it is "the social process of deciding how a physical process shall be carried out." (Riker 1962b, p. 11)

[2] 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." Riker 1962b, p. 7

Political science, in this sense, belongs to the broader family of disciplines concerned with decision-making.[3]

Not all decision processes, however, are political or of direct interest to political scientists. Riker therefore classified "authoritative allocative decisions of values" in the following way:

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

On this scheme, political science is concerned with conscious group decisions. The first case represents the limiting case of a single decision-maker—a dictator. The "quasi-mechanical" group decisions, by contrast, are characteristic of markets and thus belong primarily to economic analysis.

The presence of a group of decision-makers also implies that, once the group's size exceeds 2, coalition formation becomes central: "[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 study of coalitions, Riker argued, "a model is at hand": John von Neumann and Oskar Morgenstern's theory of n-person games, "which is essentially a theory of coalitions" (ibidem).

The key assumptions of that framework are Rational Choice and Zero-Sum, and these are also the assumptions of Riker's own model of political coalitions. He therefore used the final part of the chapter to clarify them. "Zero-sum" denotes pure conflict situations. As Buchanan and Tullock argued in *The Calculus of Consent*, such situations are relatively rare in political life and, in a sense, less theoretically attractive, because a distinctive feature of politics is "that people consent to remain in them, even when they are on the losing side in particular decisions." (Riker 1962b, p. 30)[4] Still, the zero-sum condition is a plausible assumption in many political contexts, such as elections and voting, where outcomes define winners and losers. Moreover, it can also apply when non-zero-sum situations are experienced as pure conflict by the actors involved. Yet, unlike rationality, the zero-sum assumption is not indispensable for formal political analysis as such. For that reason, it was rationality that Riker treated at greater length.

4.2.2 Rationality and the study of politics

The economists dealt with the problem of rationality, both individual and collective, and with its proper definition since the modern development of the discipline. After the Second World War, the view that became prominent was the "consistency view" of rational action. As game theorist

[3] This resembles Duncan Black's discussion of the unity of politics and economics (see Black 1950)

[4] Buchanan and Tullock 1962) was published the same year as Riker's, and he reviewed it in the *Midwest Journal of Political Science* (Riker 1962a).

and philosopher Ken Binmore explains, this amounts to translating the elements that classical and modern philosophy associated with action into a mathematical and logical framework:

> "[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 reinforced by the adoption of the notion of "revealed preferences" in the late Thirties, according to which researchers should infer patterns of choice exclusively from observable decisions (for a general overview, see: Giocoli 2003b; Moscati 2018). Its most significant development, in turn, came with the axiomatization of utility theory following von Neumann and Morgenstern.

Luce and Howard Raiffa, Riker's main reference for Game Theory besides the *Theory of Games*, devoted extensive discussion to utility theory, its axiomatic foundations, and its critical difficulties. In *Political Coalitions*, when he turned to these themes, Riker disputed, in particular, their definition of rational behavior. For Luce and Raiffa, the "postulate of rationality," or the "law of behavior" derived from it, is essentially tautological: it does not describe behavior but merely defines preferences. Hence, "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) The theoretical task, then, is not to prove that individuals maximize money, power, or any other specific objective, but to represent the structure of preference correctly and to investigate it empirically. As they wrote: "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 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)

For Riker, however, the very tautological form of this definition also constituted its main drawback. If every choice is rational by definition, then the concept asserts little more than the existence of agents and alternatives. If rationality is to be useful for modeling political behavior, he argued, one must return to the "cruder and already somewhat discredited" idea of a maximizing economic or political man. The resulting problem 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)

Setting aside the maximization of power, an idea that, in his view, lacked empirical support and depended on an ambiguous notion, Riker proposed a definition of rationality based on winning: "What the rational political man wants, I believe, is to win, a much more specific and specifiable motive than the desire for power." (Riker 1962b, p. 22). More precisely, he defined rationality in the following terms:

> *"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 insisted that not all behavior must be rational, yet rational behavior is crucial for the operation of economic and political institutions. He called this the "summation" argument: institutions should be treated as wholes, in the sense that, just as markets are effectively shaped by those agents who behave as maximizers, political institutions are similarly governed by the agents whose behavior conforms to the rationality condition.[5] Riker found this especially evident in fiduciary relations, a broad category of political and social arrangements. A fiduciary relation links an agent to a principal, and the agent is expected to act on behalf of the principal. Hence, "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)

Riker's rejection of a preference-based, tautological account can be understood as an effort to make the notion of political rationality intelligible to an audience of political scientists. He favored a more substantial and concrete definition over one framed in purely mathematical terms, partly because it could be grasped more easily by readers not comfortable with mathematical sophistication. The difficulty, however, is that this move sits uneasily with his own account of modeling. Refusing the preference-based approach pushed him toward a weaker defense of rational choice: even if not everyone is rational, the most important actors are (the "summation argument"). In doing so, he appears to set aside what the rationality assumption functions as in economics: namely, a way of constraining the beliefs and desires that actors may be assumed to hold so that their actions become explainable. Yet he did not provide a fully adequate alternative that could both explain individual action *and* support formal modeling.

[5] Notice that Downs adopted a comparable move when he treated political parties as single units in his model of elections (Downs 1957).

Restricting the set of actors assumed to be rational does not remove the fact that modeling rational behavior still requires strong assumptions about beliefs, preferences, and their formal structure.[6]

A useful parallel can be drawn with von Neumann's characterization of rational behavior as prudence. In his 1928 paper and in *Theory of Games*, rational behavior is represented by the "security payoff" generated by the minimax strategy: a player need not know what an opponent will do, but by playing Minimax he maximizes his expected payoff (Neumann 1928). As discussed earlier, this idea addresses the paradox of perfect knowledge that Morgenstern and Friedrich Hayek raised in different ways (Morgenstern 1976a; Hayek 1937). Like von Neumann, Riker isolated a single feature of behavior, in his case the preference for winning outcomes. By avoiding an overly narrow account of rationality (for example, the claim 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 provide a foundation for a theory of political coalitions grounded in von Neumann and Morgenstern's formal framework.

Interpreting Riker in this way helps clarify why he adopted this strategy. Yet, as suggested above, it remains inconsistent with his own conception of modeling. There is, in principle, no contradiction between formal analysis and a conception of rationality that captures concrete features of action. Von Neumann's solution was criticized for equating rationality with prudence, but this was not what ultimately limited its acceptance. Rather, the central obstacle was the mathematical difficulty of extending comparable results beyond 2PZSG, across both cooperative and non-cooperative settings. That obstacle was later addressed by the development of Nash Equilibrium.

Similarly, Riker's non-mathematical account of political rationality did not, by itself, rule out a more formal approach. In fact, a common move in formal political science, particularly in spatial models of voting, is to treat politicians as vote maximizers and to interpret winning as the relevant objective. In those models, however, the idea is embedded in a formal structure, typically framed through Euclidean distance (Davis and Hinich 1966). Developing that line of work required a stronger mathematical background than Riker possessed in the early Sixties.

To conclude, Riker's treatment of rationality may be best understood as consistent with an instrumentalist posture, one that resurfaces in his discussion of the "size principle." In this respect, his position parallels Milton Friedman's famous argument about the realism of assumptions and the aim of models, above all their predictive power (Friedman 1954). If firms or agents behave *as if* they were maximizers, then the rationality hypothesis can function as an adequate proxy for describing institutional outcomes. The same logic can be extended to political institutions that "[. . .] select

[6] Riker also implicitly relied on a preference-based conception of rationality when adopting the V solution of von Neumann and Morgenstern. In particular, the notion of "imputation," central to their V-solution and used by Riker, presupposes core elements of individual and collective rationality in preference terms. (Luce and Raiffa 1957, pp. 192–3)

and reward with success behavior which is apparently motivated by the intention to maximize power." (Riker 1962b, p. 21) Riker's proximity to this position is also reflected in his emphasis on verification and prediction, rather than on the advanced theoretical developments produced by highly technical mathematical economists.[7]

Riker did not explicitly refer to Friedman in his work on political coalitions. Nonetheless, a relatively straightforward instrumentalist stance persisted in formal political science even after the adoption of more robust mathematical tools, notably in the work of Peter C. Ordeshook, Otto A. Davis, and Melvin Hinich (Davis 1968; Davis, Hinich, and Ordeshook 1970; Riker and Ordeshook 1973). This continuity may mark the central line connecting Riker's early "quasi-formal" posture with the later, fully formal approaches that followed him, united by the same commitment to "the prospect of a science of politics."

4.3 The theoretical model

4.3.1 The Size Principle

The fundamental principle of the theory of coalitions concerns their size. In Riker's view, Game Theory allowed him to restate this principle in a compact form:

> *"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)

From this claim, Riker derived a descriptive proposition (or "sociological law") meant to be empirically testable: *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 offered only fragments of evidence for this proposition, chiefly by drawing on U.S. political history in the third chapter. Before doing so, however, he devoted several pages to clarifying the features of von Neumann and Morgenstern's theory of games that he intended to use.[8]

Riker's largely positivistic attitude toward the social sciences included the possibility of cumulative knowledge. In that spirit, he presented his theory of political coalitions as a partial improvement upon the rational approach that Downs had developed for the relationship between rational voters and rational parties. The parallel, as Riker framed it, runs through the analogy between parties and coalitions. Downs assumed that parties

[7] Even though Friedman's argument was widely debated among economists and social scientists, I have not found direct proof that Riker knew it when writing on political coalitions. Still, it is unlikely he was unaware of it, since Downs also refers to Friedman.

[8] 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).

aim to maximize votes. Similarly, coalitions might be said to maximize their membership. Riker, however, shifted the focus: coalitions expand only up to their minimum winning size. Furthermore, his analysis does not incorporate the discussion of voters' and politicians' spatial preferences and the probabilistic distribution of the former, which instead is the pivotal contribution of Downs' and will be at the core of the subsequent developments of formal political theories from the late Sixties onward, as well as the introduction of non-cooperative game theory.

The idea of a "minimum winning size" had appeared earlier, notably in Shapley and Shubik's Game Theory analysis of power. Riker used the concept in a rather different way. He did not attempt to compute each player's value in a game, nor did he focus on measuring the power of each member of an assembly. Instead, his aim was to use the notion to constrain the coalition structure of a game, explore equilibrium and stability within that constrained space, and ultimately suggest a way to test the resulting claims.

To get there, Riker began by sketching the model's foundations through an overview of the theory of games, drawing chiefly on Luce and Raiffa's textbook and on John Charles C. McKinsey, alongside the more verbal exposition in the first chapter of von Neumann and Morgenstern(Luce and Raiffa 1957; McKinsey 1952). He reviewed basic features of games (the number of players, moves, sets of alternatives, information, the scope for collaboration, and payoffs), but he concentrated in particular on the number of players. He then turned to n-person games, in which agreements are possible, and utility can be transferred among players.

At the same time, as Riker emphasized, such games do not yield a unique solution. In his view, this difficulty followed from game theorists' tendency to emphasize the properties of coalitions (existence, reasonableness, fairness) rather than the possibility of delimiting coalition structures more directly.[9] Since, among the solution concepts proposed after von Neumann and Morgenstern, none seemed to him clearly preferable to the original framework, whether in generality or in empirical testability, Riker chose to build directly on the concepts introduced in *Theory of Games*.

The key idea, as seen earlier, is that when the number of players exceeds 2 (calling N the set of all players) every coalition in the game, from the singleton coalition to the grand coalition, has a value given by its "Characteristic Function," $v(S)$ (with $S \subseteq N$).[10] This value is defined by the solution to a 2PZSG between the coalition and its opposite, i.e., $v(S) = -v(S)$. Each coalition divides its value among members under two constraints: no member receives less than he would obtain as a one-player coalition, and the coalition can distribute no more and no less than its total

[9] To briefly sum up this point: 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

[10] 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.

value. The resulting vector of payoffs is the "imputation." One imputation dominates another when all its components are greater. The solution of an n-person game is then the set V of all imputations that do not dominate one another (internal stability) and that dominate all imputations outside the set (external stability).

Riker introduced further restrictions on the Characteristic Function. Specifically, he focused on coalitions that are "winning." A winning coalition is defined as one whose size exceeds an exogenously specified threshold (here, m, with $m \geq 1/2$). If a winning coalition exists, its complement is losing. If no winning coalition exists, then all coalitions count as "blocking."

Von Neumann and Morgenstern normalized the Characteristic Function to make it operational, thereby establishing the "range" of possible values for a coalition: $-p\gamma \leq \bar{v}(S) \leq (n-p)\gamma$, where n is the total number of players and p the coalition size (Neumann and Morgenstern 1944, pp. 248–53). [11]

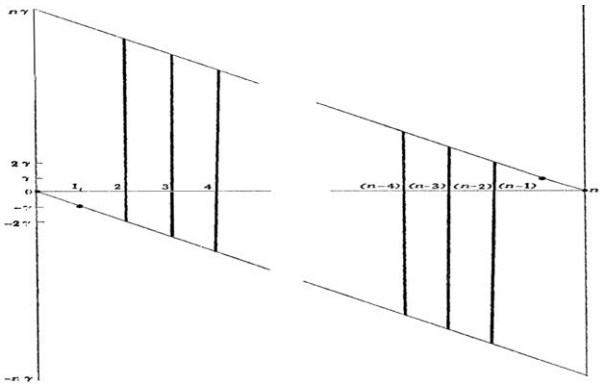

Figure 2. The range of possible values, $v(S)$ for each coalitions in the game. *Source*: Von Neumann & Morgenstern, *The Theory of Games*, p. 252

Figure 2 provides a graphical representation of this range.[12] The maximum loss for a coalition is $-n\gamma$, while the maximum gain is $(n-p)\gamma$. On a Cartesian diagram, the y-axis records the coalition value (from $-n\gamma$ to $n\gamma$), and the x-axis records coalition size (from 0 to n). The boundary lines connecting $(0, n\gamma)$ to $(n, 0)$ and $(0, 0)$ to $(n, -n\gamma)$ delimit the feasible region. As the diagram makes clear, the range contains infinitely many values. For Riker, narrowing the domain therefore required focusing only on those Characteristic Functions that correspond to winning coalitions. To do so, he distinguished winning, losing, and blocking coalitions through what he acknowledged to be a "rather imprecise" notion of majority (Riker

11 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, $v(S) = -v(S)$.

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

1962b, p. 256). Intuitively, no coalition is winning unless it has over $\frac{1}{2}$ the total membership or votes in the decision-making system. Accordingly, $S_p \in W$ if and only if $m \leq p$ (with m the majority threshold, p coalition size, and W the set of winning coalitions). If n is even, m is greater than $n/2$; if n is odd, m lies between $n/2$ and $(n+1)/2$.[13]

In this way, Riker restated von Neumann and Morgenstern's framework in a more straightforward and less mathematically sophisticated language to provide a static description of the coalitional structure of a political situation represented as an n-person game. Yet he also translated that structure into a vocabulary of equilibrium and disequilibrium. For any $v(S)$, the admissible values depend on coalition size and on the size of the opposing "blocking" or "losing" coalition. His notion of equilibrium mirrors the strategic reasoning and rational choices of the players within 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)

He then restricted attention to winning coalitions, that is, to the "winning region" of Figure 2 (above the abscissa, and for $n > \frac{n}{2}$), represented in Figure 3 (Riker 1962b, p. 41). This area contains the characteristic functions associated with winning coalitions.

Within it, he distinguished three types of winning coalitions:

1. Those for which the value of the Characteristic Function, 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 type corresponds to coalitions whose value falls as their size increases. The second corresponds to coalitions that benefit from adding members up to a point, after which the value declines. The third type includes coalitions whose value does not depend on size. The central problem, then, is straightforward: if a coalition has already reached

[13] 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.

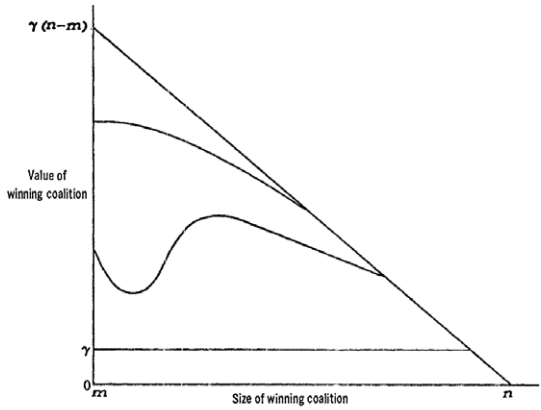

Figure 3. Different types of Characteristic Function
Source: Riker, *The Theory of Political Coalitions*, p. 41

a majority but regards its size as disadvantageous and prefers to exclude some members, the larger coalition cannot be an equilibrium outcome.

Riker summarized his conclusions in general form:

1. For functions with a negative slope throughout the whole range for winning coalitions, only the 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.

Yet the argument was not presented as a fully rigorous game-theoretical theorem. Rather, these results emerge from a difficult and sometimes opaque verbal discussion of coalition behavior, the permissible ranges of values, and the incentives to add or exclude members.[14]

Still, Riker was pursuing a theory designed to generate predictions and to be confronted with political facts through the formal structure of its

[14] 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 it will be shown in the next chapter, the main limitation of Riker's analysis is that, albeit using von Neumann and 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 (Shepsle 1974). Riker attempted to provide a new and simpler proof of the size principle in 1966 (Riker 1966).

assumptions. As he put it: "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: in social situations similar to n-person zero-sum games, only coalitions no larger than the minimum size tend to occur.

Among the model's assumptions, perfect information was probably the one most readily perceived as problematic.[15] A seemingly more realistic formulation would be that coalition-makers form coalitions they *estimate* to be minimum winning. Yet this modification quickly becomes tautological: if an Minimum Winning Coalition is not formed, one can always invoke imperfect knowledge or uncertainty; conversely, even when these coalitions appear, they might result from "irrational motives" rather than from the principle itself.

In a strict positivistic sense, the problem is that verification becomes elusive. One possible escape is experimental work, in which games are simulated in a laboratory setting to collect evidence about the size principle and coalition formation more broadly.[16] Riker devoted several pages to such efforts (Riker 1962b, pp. 49–53). But the results were necessarily partial: he did not lay out a precise methodology, nor did he specify adequate experimental rules. He therefore 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 that setting, even though it can be difficult to separate outcomes produced by rational action from those that are not, the size principle can be assessed through a particular observational 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

[15] 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 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

[16] Only in recent times the experimental testability of some assumptions regarding the rational behavior of economic agents, or players in Game Theory, 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. (Smith 1992) However, yet in the Fifties, some attempts to develop such tests were carried forward.

> 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 address the empirical challenges in testing the size principle, Riker turned to historical reconstruction, assembling evidence from selected episodes in American and European political history. He focused in particular on situations in which near-grand coalitions (whose value is assumed to be zero) contracted in size. One example is the collapse of the Federalist Party and the emergence of the Jacksonian Party (or Democratic Party) as a response to the de facto grand coalition represented by the Democratic-Republican Party between 1816 and 1828 (Riker, 1962, pp. 54 et ss.).[17]

In Riker's reconstruction, the Federalist party virtually disappeared after the War of 1812, leaving the Democratic-Republican Party as a near grand coalition. The coalition then dissolved into what he described as a "disorganized melangè of blocking coalitions," under the assumption that a whole coalition gains nothing. As he narrated the episode: "when Jackson took office in 1829, he had command of a relatively valueless coalition to which nearly everybody claimed to belong," and he attempted to reshape it into a minimal winning coalition by expelling some members and adopting a confrontational posture toward those who opposed him. Seen this way, "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 historical reasoning, as well as Riker's own justification of it, also reveals an "instrumentalist" stance, as the following passage suggests:

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

[17] The other two examples 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 (Riker 1962b, pp. 59–65).

In the fourth chapter, Riker returned to the model's restrictions, with particular emphasis on information. The central point was that incomplete and imperfect information can lead coalition-makers to form coalitions larger than the minimum winning size. To explore this "information effect," he introduced a numerical example: a zero-sum game with 101 players, in which the minimal winning coalition requires 51 players. The players are distributed 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, both S and T must persuade at least three of the singleton sets to join them. Then, the solution must be found within the set of all 5-tuples which pay a positive amount to at least three players. Additional assumptions are that members of S and T are loyal (and thus cannot desert), and that information about the moves of the singleton sets is imperfect, namely it is not known the move of each set. Because recruitment requires offers, the setting is explicitly a bargaining one.[18] He represented this by assuming that each 48-player group has "operating funds," with $F_S > F_T$ (so S has greater funds than T). The question becomes how S and T should behave strategically. Each coalition has two strategies:

1. To make an offer only to three players
2. To make an offer to four or five players

Given imperfect information, Riker argued, the first strategy is not preferable to the second. Neither coalition can ensure that offers will be accepted unconditionally, and in addition S must decide how to use its financial advantage to increase the probability 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)[19] From the example, Riker concluded that imperfect information pushes "not-yet-winning coalitions to seek more additions than they need to win" (Riker 1962b, p. 88). On that basis, he proposed the following hypothesis:

> *"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,*

[18] A further simplifying assumption is that the uncommitted players receive their promised sum only if the coalition they join becomes a winning coalition effectively

[19] 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.

> *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)

Riker suggested that this information effect might illuminate "critical elections" in U.S. electoral history. V.O. Key had introduced the notion of "critical elections" to denote periods of heightened voter involvement and the formation of new electoral alignments. In Riker's reading, such periods may be interpreted as phases in which information declines and uncertainty increases about the size of the winning coalition.

In this context, Riker also engaged with Downs's economic analysis of democracy. Downs argued that in a two-party model, where voters maximize utility and parties maximize vote shares, both parties may find it advantageous to remain as ambiguous as possible. But this seems to contradict the assumption that voters maximize utility. In Downs's formulation, such ambiguity makes it harder for citizens to vote rationally. When party positions are unclear, voters are pushed to base their choices on factors unrelated to policy, such as the personal characteristics of the candidates. Yet only parties' decisions on issues affect voters' utility derived from government action. Consequently, voting on any other basis must be regarded as irrational. This tension was a challenge for any model that attempted to treat voters and parties as simultaneously rational in the same framework. Riker suggested that his approach could offer a way out: if parties maximize votes only up to the minimum winning size, then they have no general incentive to obscure their positions, but only to do so on issues involving voters about whom they possess imperfect information Riker 1962b, 98 et ss.).

4.3.2 Strategy and coalition building

Until now Riker's analysis has been largely static. The size principle functions as an ideal benchmark: a standard to which rational coalition-building should tend. Even though it is grounded in von Neumann and Morgenstern's framework for n-person games, it does not itself constitute a solution to those games, but rather a "sociological principle." Riker used elements of the structure of such games to clarify the rational logic behind coalition politics, in parliamentary bargaining, in wartime alliances, and, more generally, in international political alignments.

In the second part of *Political Coalitions*, Riker shifted to the question of how political leaders conduct coalition-building in order to reach a stable institutional arrangement, if such stability is achievable. The goal was to exploit what he later recalled as the main promise of Game Theory: the analysis of strategic choice (Riker 1992).

Yet this section is also the weakest part of the book. Here Riker's analytical limits become most visible: the intuitions that supported the discussion of the size principle and the information effect are no longer sufficient to sustain the argument. The analysis now hinges on dynamic

and sequential reasoning—topics that were developed systematically only after the Seventies, in the work of scholars such as David Kreps, Robert Wilson, Ariel Rubinstein, and Ken Binmore, among others. Were these developments that fueled the decisive boom of non-cooperative Game Theory in economics.

Game theory has always been, of course, about strategy, but in von Neumann and Morgenstern's treatment of n-person games strategic behavior is largely subsumed by the general idea of a solution. The value of each Characteristic Function is derived via a Minimax solution in a 2PZSG between a coalition and its complement; and the strategic considerations governing why a player joins one coalition rather than another are embedded in the concept of imputation. The analysis is thus primarily static.[20] To address this limitation, they suggested that the set of imputations "correspond to the 'standard of behavior' connected with a social organization" (Neumann and Morgenstern 1944, p. 41), so that a solution can be read as an accepted standard of behavior.[21]

A different line of thought, initially formulated for two-person cooperative bargaining, was introduced by Nash through the idea of non-cooperative foundations for cooperative outcomes (Nash 2002c). Nash asked how an apparently "algorithmic" bargaining solution could be understood as the outcome of a non-cooperative game that determines what is to be bargained. This move involved a dynamic, multi-stage structure. As Luce and Raiffa (as seen, Riker's main source on Game Theory) summarized it: "Roughly, his [Nash] 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 [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)

[20] 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)

[21] 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 their work on Game Theory as pure theory and the general worldview lying behind it (see, especially for von Neumann Leonard 2010). Instead, it must be specified that Morgenstern's criticisms of the state of economics in the Thirties 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 *Theory of Games*, Morgenstern accepted the purely static nature of such a theory because it represented, in his words, the first step for further elaborations

After Rubinstein's contribution (Rubinstein 1982), it became standard to analyze bargaining in non-cooperative, sequential terms, using Nash Equilibrium in extensive-form games and, in particular, Subgame-Perfect Equilibrium.[22]

This line of reasoning aligns closely with the kind of problem Riker was trying to address. In broad terms, he explored how coalitions form, hold together, or expand across multiple stages, where the final stage delivers the observable outcome. For example, consider a voting body with weighted majorities, in which different coalitional groupings (or, in Riker's vocabulary, "proto-coalitions") exist at the outset. Leaders of these proto-coalitions attempt to recruit new members, when necessary, through side payments. If, by the final stage, the process yields a minimum winning coalition, that outcome can be read as an equilibrium.

When Riker was writing, however, this kind of Game Theory remained underdeveloped. The first major contributions that put such ideas on firmer ground, such as those by Harsanyi and Selten, date only to the mid-Sixties.[23] Riker likely knew Nash's bargaining result, even though he did not cite it in *Political Coalitions*. As noted, Luce and Raiffa devoted a chapter to bargaining and discussed Nash's solution (Luce and Raiffa 1957, pp. 124–34, 140–4). Yet they were skeptical about its practical relevance, regarding it as an artificial mathematical construction. It is plausible that both their verdict and the abstract nature of Nash's work contributed to Riker's limited engagement with it. In any case, when he addressed coalition strategy, he seems to have sensed the theoretical issues at stake, even if his own attempt did not crystallize into a fully coherent theory.

[22] This was a stronger idea of Nash Equilibrium, developed by German economist Reinhardt Selten, which applies 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 Game Theory and paved the way for the game-theoretical revolution in the Eighties. 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 Nash Equilibrium in every sub-game (Gibbons 1992).

[23] 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 the Business School at UCLA-Berkeley, 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 Game Theory in extensive form date back to the second half of the Sixties (see also: Giocoli 2009b; for biographical information about Harsanyi and Selten: https://www.nobelprize.org/prizes/economic-sciences/1994/summary/).

Riker's model can be described in the following way. Begin with a decision-making body I composed of n members (i.e., an n-person zero-sum game with side payments). Members can assume different roles, and each member's power (weight) may vary. The decision rule states that any coalition reaching weight m (with m exceeding half the sum of all weights) can act for the whole body. The zero-sum condition imposes a boundary: no decision can be such that the losers would rather resign from the body than acquiesce. Coalition-building starts when a leader within I undertakes to form a coalition around a particular issue, and to do so he must attract followers among the other members.

Because he was focused on process, Riker distinguished coalitions from "proto-coalitions." Coalitions are the end products of coalition-building and may be "winning," "losing," or "blocking." Proto-coalitions are intermediate groupings: a subset of I in a setting where at least three subsets exist and none yet reaches weight m. Proto-coalitions change in size through moves made by the members of I, and each move alters the internal structure of the body. In the first stage of the process, there are n one-member coalitions; in the second, there are $n-1$ one-member proto-coalitions and one two-member proto-coalition; and so on, up to the final stage, where either a winning coalition emerges or the outcome is a set of blocking coalitions. Since coalition-building triggers counter-moves, the leader's first step toward constructing a proto-coalition invites others to do the same. Proto-coalitions grow insofar as their leaders can recruit followers by offering side payments (Riker 1962b, 122 et ss.).

Side payments, for Riker, take different forms. They include promises concerning policies or future decisions, and they can extend as far as the threat of reprisal. Such payments also have costs borne by the coalition leader, and those costs must be incorporated in the analysis (Riker 1962b, pp. 109–20). Most importantly, he assumed that side payments are scarce and finite, and thus subject to considerations of economic value.

Because dynamic coalition-building raises questions of strategy, equilibrium, and internal stability, Riker introduced a notion that he regarded as "in some respect stronger, and in some weaker" than von Neumann and Morgenstern's set-valued solution, because the latter did not specify if some coalition in the V-set was winning. He proposed instead the idea of a "uniquely preferable winning coalition," which specifies a particular winning coalition: one with higher value than any other feasible winning coalition and in which all participants can meet their initial expectations. An "initial expectation," for a proto-coalition, is defined as the amount it can secure by joining alternative non-minimal winning coalitions.[24] Because proto-

[24] 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 winning coalition. Finally, a "strategically weak proto-coalition" is one that cannot become part of a winning coalition (Riker 1962b, 127 et ss.). For

coalitions face shifting advantages and disadvantages across stages, Riker defined equilibrium as the situation in which, once a "uniquely preferable winning coalition" forms, no rival proto-coalition can join it or construct a new winning coalition. Since, under the size principle, equilibrium corresponds to the formation of an Minimum Winning Coalition, this also yields a tidy link between equilibrium and minimum-winning outcomes. Yet the core difficulty is that such equilibrium cannot generally be maintained: it appears unstable, depending on the size and relative strength of the minimum winning coalition (Riker 1962b, 147 et ss.).

For political analysis, the implications of this lack of equilibrium could be troubling. 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) The last three chapters of *Political Coalitions* therefore turn into a largely verbal exploration of the components of this disequilibrium and of its consequences.

However intertwined these theoretical reflections with another attempt at historical testing. The case was a famous—and controversial—episode in presidential history: the election of 1825, the "corrupt bargain," in which Andrew Jackson won a plurality in the electoral college but lost in the House of Representatives to John Quincy Adams.

Interpreted as a dynamic coalition-building process, the episode can be recast as involving four presidential candidates, each the leader of a proto-coalition: Jackson, Adams, William H. Crawford, and Henry Clay. Clay entered the election, but since the House vote could include only three candidates, he had to transfer his support. Riker assigned the proto-coalitions the following values: $w(P) = 11$ (Jackson), $w(Q) = 7$ (Adams), $w(R) = 3$ (Crawford) and $w(S) = 3$ (Clay) In this example, 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. Since Clay, Adams, and Crawford were hostile to Jackson, Jackson's proto-coalition became "strategically weak," while Q, R, S formed a "uniquely preferred winning coalition." Jackson's coalition shrank, whereas Adams's became the preferred one. The new values were: Adams with 9 states, Jackson with 7, and Crawford and Clay with 4 each. To reach a majority, Adams could ally with Crawford or with Clay, but not with both if he wished to preserve an Minimum Winning Coalition. Ultimately, an alliance between Clay and Adams formed, and Riker interpreted it as Adams's dominant strategy. If Clay allied with Jackson (despite ideological differences), Crawford would ally

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 Minimum Winning Coalition, 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 Minimum Winning Coalition and that immediately next to it). In other words, the winning coalitions containing R are worth more than those which does not.

with Adams, creating an Minimum Winning Coalition. If Clay joined Crawford, their combined value would match Jackson's; together, Jackson and Clay-Crawford could win, but the value would be divided three ways over two. Given these strategic options, Clay joined Adams, a majority was secured, and Adams became the sixth President of the United States. Clay then became Secretary of State.

Once again, the historical case allowed Riker to display the explanatory ambition of his model and to suggest a new perspective on political facts, past and present. And once again he emphasized that the example does not show that Adams, Clay, or Jackson consciously employed game theory or rational choice, but rather that their behavior can be understood *as if* they had.[25]

In conclusion, the description above highlights two central features of Riker's analysis: it is dynamic, unfolding through time, and it is a bargaining process, structured around exchanges between a leader and followers. Yet he did not develop the argument as a non-cooperative multi-stage game, even in a tentative way. What such a move would have required was beyond his technical means at the time. Too much still had to be invented, and what existed, such as the extensive form of games or the notion of "information set," both present in *Theory of Games*, was not sufficient in itself. Even so, Riker showed a notable intuition for what was missing in von Neumann and Morgenstern's static approach, in which the strategic choices preceding coalition formation were absorbed into the general concept of a solution. As he put it:

> "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)

This limitation mattered, especially if one wanted empirical testing or a more explicitly positive characterization of strategic behavior. One might argue, straightforwardly, that the emergence of non-cooperative Game Theory, where binding agreements are not allowed and communication is restricted,

[25] 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)

offered a natural solution. In that setting, strategy is foregrounded, whereas cooperative theory tends to emphasize fairness, enforcement, or other normative aspects (Serrano 2005). Yet a different, more explicitly theoretical path, originating with Nash and revived from the Eighties onward, was the non-cooperative foundation of cooperative games, later known as the "Nash Program" (Nash 2002c; Binmore and Dasgupta 1987). In a sense, Riker was gesturing in the same direction when he tried to expose the strategic logic behind coalition-building. He anticipated where Game Theory could move, but he could not fully pursue that path.

4.3.3 The reviews of Riker's work

To conclude, it is worth examining the impact of Riker's work, focusing on its immediate reception among the scholarly community it primarily addressed. That audience consisted above all of political scientists, although, given the book's engagement with formal analysis, it might also have attracted other social scientists and perhaps game theorists. In practice, however, the second and third groups paid little attention. Political scientists did review it, but typically focused on selected features, without engaging deeply with the work's formal aspects or its methodological claims.

Published in 1962, *Political Coalitions* received less attention than Riker expected. Even so, reviewers generally praised the effort and stressed the originality of the approach. Yet no "real" game theorists reviewed the book, and none addressed in detail the inaccuracies or excessive simplifications in its use of Game Theory.

Among the most enthusiastic reviewers was Alfred J. Hotz, writing in the *Midwest Journal of Political Science* (Hotz 1963). For 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) Yet he did not examine the analysis in depth, and his review remained close to a summary of Riker's argument.

Donald Matthews and Morton Kaplan were more critical. Matthews, a leading scholar of the U.S. Senate, wrote in *The Journal of Politics* that 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) He raised the question of whether political scientists possessed enough substantive knowledge about politics to construct such models. He did not reject formal model-building on epistemological grounds, but suggested that the discipline's empirical knowledge might still be too limited, and that additional empirical theory was needed before highly abstract formalization could become fully useful.

Kaplan, as discussed earlier, had explored game-theoretical implications in international politics during the Fifties (Kaplan 1957). His work relied mostly on non-cooperative bimatrix games and did not aim for high formal sophistication. He deployed Game Theory in a "functionalist" way, without explicitly grounding it in rational choice or a broad methodological defense

of modeling. Yet Kaplan's work was respected by Morgenstern, and they were in touch. Kaplan was not a game theorist in any stronger sense than Riker. Rather, he was an international relations scholar. In his review for *The Annals of the American Academy of Political and Social Science*, he criticized two aspects of Riker's work: its claims to generality and its reliance on rational choice. He argued that alternative models of coalition-building were possible and that political decision-making could be organized around many different values (Kaplan 1963). Despite his earlier use of Game Theory, his review remained at a highly general level.

Richard Fagen's review, published in the *American Political Science Review*, was among the most supportive (Fagen 1963). Fagen was not a specialist in game theory or voting models, but he had previously reviewed recent work on power, including Shapley and Shubik's 1954 paper, and thus had some familiarity with game-theoretical approaches. (Fagen 1961) He emphasized that Riker's work belonged to an emerging political science literature that employed mathematical reasoning, while also noting—correctly—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 distinguished three purposes of the book: first, to construct a theory of coalitions; second, "[...] to nudge –or shove–his fellow professionals toward the theoretical sophistication which (he feels) characterizes the neighboring disciplines of economics and psychology;" and third, a "policy purpose," mainly oriented toward international politics. Yet this last aim seemed less persuasive to him, since the model's constraints, especially the zero-sum condition, fit legislative settings better than international politics. He concluded:

> "[. . . .] 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)

The most comprehensive review appeared in *The Journal of Conflict Resolution*, authored by James A. Robinson, an international politics scholar (Robinson 1963). He described Riker's work as "an original and unusually well-written work." (Robinson 1963, p. 763) Robinson offered a brief but detailed summary and then raised two main points. The first concerned the mathematical model: he sensed the imprecision of Riker's argument, although he admitted he lacked the competence to evaluate it fully. The second concerned empirical confirmation. Robinson did not share Riker's skepticism about experimental methods in the social sciences, and he

suggested, by way of example, laboratory experiments on international relations conducted by Harold Guetzow and his research team. Yet Riker's position toward experiments was less hostile than Robinson implied, since Riker himself pursued experimental tests of Game Theory during the Sixties.

As Riker later complained in his interview with Shepsle, few readers grasped the book's theoretical stakes (Riker and Shepsle 1979, p. 24). Political scientists tended to focus on selected elements, especially the last three, largely verbal chapters. Moreover, many reviews framed the book primarily through the lens of international politics: Riker's final chapter, with remarks on the U.S.–U.S.S.R. rivalry, attracted attention more than the model's formal structure. At the same time, his adoption of von Neumann and Morgenstern's solution concept passed largely unnoticed among game theorists.

So where, then, does the importance of *Political Coalitions* lie? The size principle generated a wave of debate and pushed the discussion toward more explicitly formal assessments of ideas that were still only partially articulated in Riker. His arguments on coalition-building itself proved less influential than his claim about coalition size. Yet the broader problem of legislative coalition formation unsurprisingly became a major topic in formal political science, and from the Eighties onward much of that literature moved away from Riker's original framework and toward bargaining models developed in economics.

The importance of Riker's work on political coalitions rests on its being the first sustained attempt to model political behavior explicitly through a game-theoretical lens, even if it remained far from the level of contemporary formal analysis in economics. This book did not contribute new results to game theory as such, but, in light of what formal political theory became from the Sixties onward, it marked the beginning of one of the most successful translations of theoretical tools across disciplinary boundaries. That later and highly formal literature realized Riker's vision and was often closely tied to his work at the University of Rochester.

Chapter 5

The Development of Positive Political Theory

The University of Rochester played a pivotal role in the story of how formal political theory spread into American political science, not merely as a host institution but as an intellectual community in which formal political theory could consolidate and spread. The emergence of Positive Political Theory as a recognizable subfield of American political science cannot be understood without considering Rochester's institutional strategy and its unusually strong commitment to theory-driven social science. As Sonja Amadae has emphasized, one of the distinctive features of Positive Political Theory, as opposed to Public Choice, was its close association with the intellectual leadership of a single individual. While Public Choice developed as a strongly institutionalized enterprise, first at the University of Virginia and later at Virginia Polytechnic Institute and George Mason University, Positive Political Theory was, from the outset, deeply identified with Riker's scholarly agenda and personal influence (Amadae 2003).

Thus, the development of social sciences at Rochester and Riker's intellectual trajectory are inseparable. Any attempt to assess Riker's contribution must therefore take seriously his role as an "entrepreneur of ideas," a figure who combined theoretical ambition with the ability to shape departments, curricula, and research agendas. In her historical reconstruction, Amadae also devoted considerable attention to the parallel transformation of the Department of Economics, which, under the academic and charismatic leadership of Lionel W. McKenzie, moved decisively toward a strong theoretical orientation. According to Amadae, this convergence was crucial. 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 of 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 several ways to approach this topic. One option would be to rely on the now well-established notion of "intellectual community," which emphasizes the collective and social dimensions of scientific activity. The

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history and sociology of science literature offers a rich set of taxonomies to describe such communities, often stressing the role of leadership, institutional resources, and shared research agendas (Forget and Goodwin 2011; Medema 2011). In its earliest formulation, this perspective can be traced back to the concept of the "research school" developed by J. B. Morrell, which highlights the combined importance of charismatic leadership and adequate financial support (Morrell 1972). Subsequent debates have broadened this notion considerably, stretching the idea of "charismatic leadership" from the ability to mobilize resources or attract the attention of disciplinary gatekeepers to the capacity to draw colleagues and students into a shared research program (Forget and Goodwin 2011, p. 20). In some cases, the same individual may fulfill both roles, and Riker plausibly belongs to this category.

Despite the usefulness of these conceptual tools, the discussion that follows remains deliberately historical in orientation.[1] During the Sixties and Seventies, Riker's institutional activities at Rochester were accompanied by a broader effort to construct a network of scholars who shared his vision of formal political analysis. This period also witnessed the publication of Buchanan and Tullock's main work and the emergence of Public Choice, as well as a series of formal innovations in political theory, including the spatial theory of voting developed by Davis and Hinich, and later contributions by Rochester-trained scholars such as Ordeshook and Shepsle.

This chapter is divided into two main parts. The first reconstructs Riker's role in the transformation of the Department of Political Science at the University of Rochester, as well as McKenzie's efforts in building the Department of Economics, and the diffusion of Positive Political Theory and Rational Choice during the Sixties and Seventies.

The second part of the chapter then turns to a selection of advanced theoretical developments in formal political analysis. Rather than attempting an exhaustive survey of the field, the discussion focuses on a series of debates surrounding the size principle and coalition theory. As will become clear, Riker's analysis of political coalitions provided a set of powerful intuitions, but it was also affected by mathematical and game-theoretical limitations. By examining Robert Butterworth's critique of the size principle, Shepsle's response, and McKelvey's subsequent (and largely unpublished) formal proof, the chapter highlights both the significance and the shortcomings of coalition theory as originally formulated by Riker.

[1] A general historical account of the establishment of the Department of Political Science is provided by Amadae and Mesquita 1999, partially reproduced in Amadae 2003. For the case of Economics, see Düppe and Weintraub 2014b and McKenzie 2012. Alongside secondary sources, the narrative draws on archival materials from McKenzie's papers, housed at the Rubenstein Library at Duke University, and from Riker's papers preserved at Rochester

5.1 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, the city emerged as a significant industrial center, where major companies such as Kodak and Xerox were founded. In addition to this industrial development, Rochester also possessed a long-standing tradition of intellectual activity. For instance, the city was home to the prominent activist and abolitionist Frederick Douglass, and it functioned as one of the forums of antislavery debate before the Civil War. Later on, Rochester was also the hometown of the social reformer and women's suffrage activist Susan B. Anthony, which provided further intellectual stimuli.

Despite this historical background, in the mid-Fifties the University of Rochester was still a relatively small private liberal arts college, located in a geographical position that was certainly not favorable. Situated in the north-western part of New York State, on the shores of Lake Ontario and close to Niagara Falls and the Canadian border, it was far removed from the intellectually vibrant environments of the Ivy League universities, the University of Chicago, or the major Californian institutions. At that time, its academic reputation rested primarily on the nationally renowned Eastman School of Music, founded in 1921 thanks to the endowment of George Eastman, the founder of Kodak.

Another philanthropist who contributed significantly to the University's academic rise was Joseph Wilson, the owner of Xerox. In the Fifties, Rochester received a substantial endowment from him, which placed it among the wealthiest research universities in the United States (Amadae and Mesquita 1999). With these resources, the University's administrators launched an ambitious program aimed at enhancing the academic status of the institution, beginning with the recruitment of senior and junior faculty and the establishment of new advanced programs of study.

One of the major consequences of these well-financed development plans was the strengthening of the social sciences. The outcome was the creation of nationally leading, theory-driven graduate programs in Economics and Political Science. In the case of Political Science in particular, Rochester eventually became the institutional stronghold of what came to be known as "Positive Political Theory." Among the first scholars hired to implement this transformation was the relatively young, yet already renowned, mathematical economist Lionel W. McKenzie.

Although McKenzie's impact on Economics was perhaps less revolutionary than Riker's on Political Science, the presence of an advanced and theory-oriented Economics Department played a crucial role in shaping the intellectual environment in which Riker could pursue his disciplinary agenda. The next pages will therefore examine McKenzie's transformation of the Economics Department, Riker's activity as chairman of the Political Science Department, and, finally, some highlights of Riker's efforts to build a broader scholarly network. Particular attention will be devoted to two aspects: his relationship with the Public Choice group, and the dissemination

of mathematical political theory.

5.1.1 The activities in the Economics Department

McKenzie (1919–2010) was undoubtedly among the most important mathematical economists of his generation. In the Fifties, his achievements were remarkable, especially given his relatively peripheral position within the community of mathematical economics. A native of Georgia and a graduate of Duke University (at a time when Duke was still a relatively prestigious regional university located in a segregated state) he later moved to Princeton, where he earned his Ph.D.[2] Prior to this, he had been a Rhodes Scholar at Oxford and had served as an instructor at MIT (McKenzie 1999).

McKenzie largely trained himself in mathematical economics and, in 1950, spent an entire year at the Cowles Commission at the University of Chicago, where he encountered many of the central figures of the emerging formal revolution in economics, such as Gerard Debreu, Tjalling Koopmans, and others (Weintraub 2002). Moreover, during his time in Chicago, he received systematic training in mathematics, particularly in algebra, topology, and measure theory, thereby expanding his knowledge far beyond the calculus-based preparation that still constituted, if anything, the standard requirement for students and graduates in economics (McKenzie 1999, p. 4).

After this period, McKenzie returned to Duke, where he was appointed Professor of Economics, despite not having yet received his Ph.D. His theoretical work during these years focused primarily on General Economic Equilibrium. Most notably, he produced the first mathematically rigorous, and economically meaningful proof of the existence of a General Economic Equilibrium, anticipating by several months the better-known Arrow–Debreu theorem.[3]

These accomplishments place McKenzie in a relatively central position within the emerging community of mathematical economists. Many colleagues recognized the originality and depth of his mathematical contributions, and he maintained close personal relationships with prominent economists, including Paul Samuelson. By contrast, while Riker was largely isolated, both within the community of political scientists, to which he formally belonged, and among game theorists, to which he aspired to belong, McKenzie enjoyed substantial recognition and esteem among his peers.[4]

Nevertheless, both his correspondence and later recollections indicate that McKenzie was only partially satisfied with his academic situation. This

[2] Although his dissertation had been completed earlier, personal circumstances delayed the formal awarding of the degree until 1956, after the publication of his first major theoretical contributions.

[3] In addition to General Economic Equilibrium, McKenzie's contributions to economic theory also included foundational work on demand theory (McKenzie 1957) and on optimal economic growth, particularly the first of the so-called turnpike theorems. A comprehensive overview of McKenzie's theoretical contributions can be found in Mitra and Nishimura 2009 and McKenzie 2009.

[4] For example, Samuelson wrote in a personal letter that McKenzie was among the very few mathematical economists who truly understood the economics underlying mathematical economics (cited in Düppe and Weintraub 2014b, p. 195).

dissatisfaction proved decisive in leading him to accept the offer made by the administrators of the University of Rochester in 1956. The sources of this discontent were twofold. First, on a personal level, Duke was geographically and intellectually distant from the most prestigious departments where the rapid advances in mathematical economics were taking place. McKenzie pursued a theoretical agenda that found little institutional support at Duke.[5] More generally, the Duke faculty was not particularly receptive to the new developments in mathematical economics and, like most economics departments at the time, remained committed to traditional approaches grounded in verbal reasoning and descriptive generalizations rather than econometrics and formal mathematics. McKenzie, by contrast, strongly favored the rigorous mathematical foundations of economic theory and made major contributions in this direction. At the same time, unlike what could be described as the dominant strand of axiomatic theory in economics (most notably associated with Debreu at Berkeley, which produced some of the most abstract general equilibrium results of the Sixties and Seventies), McKenzie consistently emphasized the economic relevance of his theoretical work. For this reason, he did not regard himself simply as a mathematical economist, or still less as a mathematician working on economic problems, but rather, in his own words, as an "economic theorist." Mathematics and rigorous analysis were essential tools for establishing logical consistency in economic theory, but they were not reducible to a mere sequence of formal theorems (Düppe and Weintraub 2014b, p. 190).

The offer from Rochester arrived in 1956, at a moment of growing dissatisfaction with the environment at Duke. As noted earlier, the University of Rochester was actively seeking promising senior scholars with original ideas to build advanced graduate programs in the social sciences, and McKenzie fit this profile particularly well. His correspondence sheds light on the recruitment process. He was offered not only a full professorship but also the chairmanship of the Department of Economics, which had recently been separated from the Business School. This proposal, together with a salary that was exceptionally high for the time, strongly appealed to him, even though he hesitated for several months, primarily because Rochester lacked the academic prestige of more established universities. Seeking advice, he consulted several colleagues, including Samuelson, who encouraged him to continue negotiations with the Rochester administration (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 in the following terms:

> "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

[5] Additional personal considerations also played a role, including the fact that McKenzie had married a Jewish woman in North Carolina, a state that—although more moderate than the Deep South, where he had been born, was still marked by anti-semitism, in addition, of course, to racial segregation affecting African Americans.

> 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, McKenzie n.d., Box 6)[6]

In the same letter, McKenzie outlined both the advantages and disadvantages of a potential move. On the positive side, he emphasized the substantial increase in salary, far exceeding anything he could expect at Duke. On the negative side, he mentioned the northern climate, the administrative responsibilities associated with the chairmanship, and the challenges of building a viable graduate program from scratch at an institution lacking an established tradition in economics. In a subsequent letter written one month later, after he had accepted the offer, he stated:

> "[...] 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, McKenzie n.d., Box 6)

As noted above, McKenzie was recruited with the explicit mandate of developing a first-rate graduate program in economics. One of the initial steps in this process was the formal separation of the Department of Economics from the Business School, a decision that had been made prior to his arrival but only implemented after his appointment but was fully consistent with his views on economics education. Indeed, At Duke, he had advocated precisely such a separation between business and economics programs (Düppe and Weintraub 2014b). Even more important, however,

[6] McKenzie is referring to the historian Cornelis de Kiewet (1902-1986), previously president of Cornell University

was the hiring strategy he pursued. At the time of his arrival, the department consisted of three full professors, one associate professor, one assistant professor, and one lecturer, none of whom were closely engaged with the most advanced developments in economic theory.[7] Their research fields reflected the standard undergraduate curriculum in U.S. economics at the time, including money and banking, public finance, international economics, area studies, labor economics, government, and economic history (Albert W. Noyes to Lionel W. McKenzie, November 20th, 1956, McKenzie n.d., Box 6). Immediately after accepting the position, McKenzie began searching for young scholars to recruit. Even before formally accepting the offer, he had outlined his hiring standards in a letter to Noyes, the Dean of the College of Arts and Sciences. In that letter, 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." (Lionel W. McKenzie to Albert W. Noyes, October 29th, 1956, McKenzie n.d., Box 6).

Between late 1956 and early 1957, McKenzie wrote to numerous economists with whom he was well acquainted, including Samuelson, Robert Dorfman, Arrow, William Baumol, Milton Friedman, Jacob Viner, Franco Modigliani, and Albert Hirschman. He sought advice on recruiting graduate students from their departments and requested assessments of the existing Rochester faculty. Among these efforts was an unsuccessful attempt to recruit Gary Becker, who ultimately chose 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, McKenzie n.d., Box 6).

The first new faculty appointment was Ronald Jones, who had earned his Ph.D. at MIT under Robert Solow and would later become one of the leading figures in international trade theory (Düppe and Weintraub 2014b). He was soon followed by Richard Rosett, Edward Zabel, Nanda Choudry, Michio Hatanaka, S.C. Tsiang, Rudolph Penner, Alexander Eckstein, Sherwin Rosen, and Robert Fogel. Fogel later moved to Chicago and was awarded the Nobel Prize in Economics in 1994 for his contributions to quantitative economic history. One year later, the graduate program was launched, with a strong emphasis on recruiting students from abroad, particularly from Japan, Europe, and other parts of Asia. Among the earliest cohort of students, Akira Takayama would later emerge as a prominent mathematical economist.

Further insight into the department's scientific activity in the early 1960s is provided by reports prepared for the Committee on Social Science (CSS) at the University of Rochester. These documents offer a broader overview of the state of social science research at Rochester and of the plans devised to promote its development. The Committee was established in the fall of 1961 and included representatives from all social science departments in the College of Arts and Sciences: Anthropology, Political Science, Psychology,

[7] These faculty members were William E. Dunkman, Donald W. Gilbert, Warren Hunsberger, Robert R. France, Jack Taylor, and Alice Wynd.

Sociology, History, and Economics. Drafts from the first meetings, held between October and November, reveal that the central concerns included interdisciplinarity and the alignment of Rochester's social science programs with national research trends. The reports also describe the activities, objectives, and scope of research pursued by each department.

The CSS reports outline how research in the Economics Department was structured under McKenzie's chairmanship. Scholarly activity was divided into empirical and theoretical work. Empirical research included, for example, Fogel's investigations into American economic history, with particular attention to the role of railroads. Methodologically, Fogel framed his work "within the categories of growth economics," employing advanced mathematical and statistical techniques to collect and analyze data. The same report emphasized the expansion of funding and infrastructure necessary for such empirical research, especially computing facilities and library resources ("Empirical Research currently under way in the Department of Economics," McKenzie n.d., Box 32). Notably, even though economic history constituted only one component of empirical research, particular emphasis was placed on the "continued development of relations between the economics and history departments" ("Empirical Research currently under way in the Department of Economics" 1960). A similar call for interdisciplinarity was made with respect to political science, especially in areas related to taxation and fiscal policy. Research in theoretical economics, McKenzie's primary field, covered topics ranging from the mathematical analysis of competitive markets, with special emphasis on the key issues surrounding general equilibrium (existence, uniqueness, and stability), to international trade and macroeconomics. Among the broader objectives identified was also the development of mathematical and statistical methods in the social sciences, with particular attention to choice behavior, psychology, and social psychology ("Research in Theoretical Economics" 1960, McKenzie n.d., Box 32).

Although these reports are largely descriptive and remain at a general level, they are invaluable for understanding the kind of commitment to social science research and interdisciplinary collaboration that characterized Rochester in the early Sixties. Even without extensive detail, they convey the image of a highly ambitious and intellectually stimulating academic environment, an impression corroborated by interviews, personal recollections, and oral histories of scholars who spent part of their careers there. As Düppe and Weintraub observed: "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 Weintraub 2014b, p. 193)

5.1.2 Setting the "Positive Political Theory"

In the late Fifties, the Department of Economics was undergoing significant transformations, and the Department of Political Science was on the eve of similar changes. It is important to note that the Committee on Social Science mentioned above was established several months before Riker officially assumed his position at the University of Rochester. Even so, the discussions concerning Political Science were clearly shaped by his impending arrival. Indeed, the meeting drafts make it evident that Riker was hired primarily for his ideas about how the discipline ought to develop. Moreover, although he was not yet formally on the faculty, many references, albeit very general ones, were made to the activities he was expected to undertake in the Department.

So then, in the first draft describing the situation in the Political Science department, it is stated: "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." (University of Rochester 1961, October 2nd, 1961) Then, even though the Department was represented on the Committee by Richard Fenno, a scholar committed to traditional analysis (though not hostile to a more formal approach), it nonetheless appears that some of Riker's ideas found their way into the discussions. In particular, emphasis was placed on the new perspectives Riker was expected to introduce, namely the "growth toward a doctoral program," beginning 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, McKenzie n.d., Box 32). Again, at the third meeting, when Fogel and McKenzie discussed empirical research and applied mathematics, Fenno remarked on Riker's active interest in mathematical methods for the study of political behavior. (University of Rochester n.d., November 6, 1961, McKenzie n.d., Box 32).

As noted above, each department was also asked to prepare a report for the Committee. Unfortunately, the report on Political Science is unsigned, so it is impossible to determine whether it was written by Riker or instead by Fenno. Nonetheless, even a cursory reading suggests that Riker's theoretical agenda is visible once again. With regard to teaching, the Department followed a traditional and fairly standard curriculum: American Politics, Comparative Political Systems, International Politics, and Political Theory. Yet the report also anticipates 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) This reorientation of teaching was presented both as a prerequisite for creating a graduate program and as a means of reshaping departmental activity more generally. This was particularly urgent because,

in the closing years of the Fifties, the Department "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 Shepsle 1979, p. 62)

Among the research projects the Department intended to pursue were the analysis of decision-making in small groups, the study of federalism, the analysis of congressional committees, and also more explicitly theoretical topics such as "methods and hypotheses which may be used to develop a general theory of politics" or "the relationship between leisure behavior and political behavior." The report also acknowledged the intrinsically interdisciplinary character of these projects, for instance, the connections among decision-making theory, mathematics, and economics ("Report on Political Science for Committee on Social Science" 1960).

As for hiring policies, the stated goal was explicitly to recruit "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 implied an interdisciplinary approach, with a broader research horizon that could be sustained only through strong collaboration with other fields. However, as the report stated, "[...] 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." At the same time, the emphasis was not exclusively on formal analysis and mathematical techniques. For example, social psychology was also considered part of this broader renovation effort.

The report concluded with a general statement about 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 under conditions very similar to those offered to McKenzie, and he began his work in the fall of 1962. Like McKenzie before him, Riker was seeking new professional opportunities and was partially dissatisfied with his previous academic environment. Moreover, McKenzie almost certainly played a role in persuading Riker to join Rochester. As McKenzie 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 formally assumed his appointment as full professor of Political Science and chairman of the Department in the fall of 1962. As soon as he arrived, he began a program of radical renovation, closely following the lines discussed above. At Rochester, he found precisely the environment

he needed to advance his agenda, and he was supported even by scholars, such as Richard Fenno, who remained committed to traditional approaches. Riker recalled that, beyond personal reasons, what attracted him were the efforts made by the Rochester administration to move "from a small liberal arts college to a university and systematically developing departments" and to build an advanced graduate education program (Riker and Shepsle 1979, p. 62).

No one resisted his plan to improve the Department's standing by advancing his own vision of political science, one that, plainly, required teaching courses in decision theory and game theory, as well as methodology. Shepsle, who entered the newly established graduate program in 1967, recalled 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." (Shepsle 2021) What Riker aimed to accomplish can be succinctly captured in the brief memorandum on Political Science curriculum that he drafted as a Fellow at Stanford's "Center for Advanced Studies in the Behavioral Sciences" (1961).[8] The same vision that can be found throughout his work in the late Fifties, culminating in *The Theory of Political Coalitions*.

To understand Riker's perspective, and how it reshaped the teaching of political science at Rochester, some attention must be paid to the Department's curriculum and broader institutional condition. Rather than relying solely on the Committee on Social Science drafts, a more complete picture can be drawn from the University of Rochester Undergraduate Bulletins. These bulletins provide, alongside a general description of the University's history and activities, a detailed list of departments, faculty members, and courses. For the academic year 1961–62, the Political Science curriculum reflected a traditional structure, divided into "International Relations," "American Politics and Institutions," and "Political Theory." The latter also included an 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 his first hires were Arthur Goldberg and, most importantly, Gerald H. Kramer as an instructor, who joined the original six members of the faculty (in addition to Riker himself).[9] Kramer, an MIT Ph.D., had a strong mathematical background, and his role would prove essential in the following years for advancing both training and research in mathematical political science within the Department. After Riker's arrival, the number of undergraduate courses in scope and methodology doubled, and by the academic year 1963–64 a course

[8] 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 earlier

[9] These were Richard Fenno, Theodor Bluhm, Glen Wiltsey, William Diez, Dale Neuman and Peter Regenstreif (Riker and Shepsle 1979, p. 63)

titled "Positive Political Theory" had been introduced within the "Political Theory" section ("University of Rochester Undergraduate Bulletin 1963-4" 1963). Amadae and Bueno de Mesquita described the transformation 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). Within this approach, the notions of rational choice in political decision-making clearly occupied a central place.

Still, as in the case of the Department of Economics, a crucial prerequisite for establishing a successful graduate program was the recruitment of both faculty and students. As Riker later recalled:

> "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 that that was one good thing that this Department did for the world was to make Jerry into a political theorist." (Riker and Shepsle 1979, p. 65)

Kramer, according to Shepsle, was "probably the leading mathematical political scientist at the time" (Shepsle 2021) and played a pivotal role in supervising the dissertations of analytically oriented graduate students such as Peter Ordeshook, Shepsle himself, and especially Richard McKelvey. After his period at Rochester, Kramer joined the faculty at Yale and spent substantial time at the Cowles Foundation there.

Beginning in 1967–68, Richard Niemi joined the faculty. He earned his Ph.D. at Michigan State but had earlier been an undergraduate at Lawrence, where in 1961 he had likely attended one of the earliest courses taught by Riker on "Positive Political Theory," during the same period in which Riker was also a Fellow at Stanford (Niemi 2021). Although he lacked formal training in mathematical modeling, unlike Kramer or several of the graduate students who began arriving once the graduate program was underway, Niemi had strong statistical training and fit well within the theoretical agenda Riker was pursuing. Accordingly, by the academic year 1967–68, the number of undergraduate courses addressing topics such as game theory, decision theory, and analytical methodology had risen to five ("University of Rochester Undergraduate Bulletin 1967-8" 1967).

Nevertheless, the most important development in the second half of the Sixties was the definitive establishment of the graduate program, which

was precisely the task for which Riker had originally been recruited. The first student enrolled in 1962, but the program officially began in 1963. Ordeshook, who entered with the class of 1964, recalled that although he chose Rochester over Columbia largely by chance, he was deeply struck by Riker's treatment of formal political theory (Ordeshook 2021). The curriculum covered virtually all major topics available at the time, ranging from Arrow's Impossibility Theorem to spatial models of voting, and including Buchanan and Tullock's *The Calculus of Consent* as well as Olson's newly published *The Logic of Collective Action* (1965). It also addressed 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."* (Ordeshook 2021. Italics in the text)

Shepsle, who arrived in Rochester in 1967, similarly recalled that Riker was deeply involved in graduate training. He taught two seminars required of every graduate student: an introductory seminar on game theory and another called "Positive Political Theory." (Shepsle 2021) A more advanced game theory course was likely taught by Gerald Kramer, whose mathematical abilities exceeded those of virtually anyone on the Rochester faculty at the time. However, students, according to Ordeshook and Niemi, were also required to take traditional courses, including those taught by Fenno and Bluhm. In any case, by the late Sixties, the Rochester graduate program was widely recognized for its strong emphasis on mathematical political science and formal modeling. One consequence was that the undergraduate background of applicants differed substantially from that of students in other political science programs. As Shepsle put it: "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 Shepsle 1979, p. 70) Shepsle had majored in mathematics, while, as noted, Ordeshook was trained as an engineer. The 1968 cohort included Richard D. McKelvey, who had a similar background (Palfrey 2005), and this pattern continued throughout the Seventies.

Mathematical reasoning occupied a central place in "Positive Political Theory." Indeed, as Riker conceived it, this was not merely an "economic approach," in the sense of the many parallels historically drawn between economic and political action.[10] Rather, at its core lay the importance of formally modeling political phenomena, in which concepts such as preferences, choice, and equilibrium acquired meaning precisely through the internal consistency of the model. For this reason, mathematical training played a pivotal role in the education of political scientists at Rochester. It should be noted, however, that Riker was not really a mathematician.

[10] For a brief discussion of this intellectual history see: Barry 1970

As mentioned earlier, he was largely self-trained, and his technical skills were well below those required to pursue the ambitious theoretical agenda he had set for himself. As 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) With respect to mathematical training for graduate students, several options were initially available. On the one hand, scholars such as Kramer taught them; on the other, students were encouraged to take econometrics and statistics courses in the Department of Economics or in Mathematics (Ordeshook 2021; Shepsle 2021). For example, after he joined the Economics Department, James W. Friedman, one of the most important contributors to game theory in the Seventies, helped political science students acquire more solid game-theoretical training, as Ordeshook recalled (Ordeshook 2021). Moreover, because the graduate program attracted students with strong mathematical backgrounds, extensive remedial training was often unnecessary, at least in the earliest years. As Bueno de Mesquita recalled: "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, however, that no formal training in economics seems to have been required, let alone in mathematical economics. What mattered was mastering the basic techniques, namely mathematics and statistics, rather than falling prey to any supposed "imperialism" of economic science.

5.1.3 The Spreading of Formal Political Theory in the Sixties and early Seventies

In 1962, the same year of Riker's appointment, the University of Rochester hired the renowned statistician and economist Wilson 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 also becoming chancellor between 1970 and 1976. Under Wallis' mandate, the University completed its transition from a small regional university to a leading U.S. research institution. Moreover, Wallis was not merely a gifted administrator. Rather, he was embedded in an extended network of scholars and public intellectuals. At the University of Chicago, he had established strong ties with figures such as 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 the Second World War by F.A. von Hayek.

Teaching at Rochester was pivotal for establishing Positive Political Theory during the Sixties. In the same period, Riker was also working to build a network of scholars who shared a similar theoretical agenda. In

Virginia, Buchanan and Tullock adopted Price Theory and Cost Analysis to address issues ranging 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, sponsored both by academic institutions and by organizations such as the National Science Foundation and the Social Science Research Council, were organized. Indeed, in the Sixties, after the publication of the "founding text" of Public Choice, *The Calculus of Consent* (Buchanan and Tullock 1962), Buchanan and Tullock began a series of conferences devoted to "non-market decision making." Similarly, at the beginning of the decade, 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 early Seventies. There, the emphasis was placed on theoretical contributions, such as voting models, as well as on statistical analyses. Eventually, these conferences moved to the Virginia Polytechnic Institute in 1966, since their organizer, Joseph L. Bernd, had relocated there. Still, Virginia Polytechnic was the hometown of Public Choice, thanks to the presence of Buchanan (Levy and Peart 2020). In addition, 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 the Social Science Research Council.

In the summer of 1963, when he was still an Associate Professor of Political Science at Southern Methodist University, Bernd invited Riker to participate in the planned conference on Mathematical Applications in Political Science, to be held the following year. In his invitation letter, Bernd presented the conference as an opportunity for post-doctoral researchers to expand their training in formal techniques in political science and to consult on research projects. Accordingly, Riker was asked to present a paper on Game Theory and legislative policy (Bernd to Riker, July 18th, 1963, Riker n.d., Box 11, Folder 2). At that conference, Riker met Downs, who was among the lecturers. However, only the lectures by Riker and Downs addressed theoretical aspects. Riker's lecture focused on cyclical majorities, revisiting themes from his 1961 paper. Downs, by contrast, offered 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 was later published (in revised form) in the first conference proceedings (although Downs's did not). In the published version, Riker reassessed Arrow's paradox, focusing on two episodes in which cyclical majorities had occurred: one was the case of the "Powell amendment" in 1956; the second, already discussed in earlier work (Riker 1961), was the failure to pass the 17th amendment to the U.S. constitution in 1911 (direct elections of the U.S. Senate).[11]

[11] The Powell Amendment case became an example to 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

The other lectures at the conference dealt with statistical topics used to analyze political behavior, both in domestic politics and in international relations, and were delivered by renowned quantitative scholars such as Harold Guetzow, Donald Stokes, and Sidney Ulmer. Overall, thirty-five scholars attended the first conference between July 19th and 29th, 1964 (the average age was 39: Bernd to Riker, March 16th, 1964,Riker n.d., Box 11, Folder 2). With few exceptions, the applicants lacked advanced mathematical and statistical skills. This was also because, as the organizer noted, priority was given to scholars without advanced training in these techniques, for the obvious reason that this group was far more numerous within the political science profession.

Riker was invited to participate in the second conference as well. In the remainder of the Sixties, these scholarly meetings and the ensuing proceedings catalyzed the exchange of ideas and the development of new, increasingly formal political theories. Moreover, they fostered the "meeting of minds" that made the diffusion of formal political theory possible (Bernd, Claunch, and Herndon 1964-73; Medema 2009). Furthermore, from the second conference onward (1965), statistical analysis also opened the way to a growing number of theoretical and even methodological contributions. Here, Riker's influence is readily visible, as his correspondence with Bernd makes clear. Indeed, Riker conveyed to him his concerns about the state of political science, which he regarded as "far behind Economics in quantification," with the discipline "hav[ing] had no Alfred Marshall." (Bernd to Riker, July 31st, 1964, Riker n.d., Box 11, Folder 2) Ultimately, the second conference (July 18th–August 7th, 1965) included a decision-theoretic analysis applied to political campaigning (specifically, investigating whether door-to-door canvassing maximized each candidate's expected plurality; Kramer 1966); a reassessment of the size principle by Riker; and, finally, a landmark paper by Otto Davis and Melvin Hinich (Carnegie Mellon), in which they addressed policy formation within an advanced theoretical framework (Davis and Hinich 1966).

Given the importance of the latter contribution, it is worth spending a few words on it. Davis and Hinich's work offered the first mathematical exposition of the spatial preferences model. As noted, the idea of spatial preferences in politics dates back to the work of Black, Downs, and, earlier still, Hotelling. Yet their contributions had not explored the problem in depth from a mathematical standpoint. Black restricted his discussion to single-peaked preference functions, whereas Downs largely overlooked the issue, focusing instead on the probability distribution of voters' preferences over a single-issue space.

Both Davis and Hinich had not been trained as political scientists. Davis earned a Ph.D. in economics at the University of Virginia, where he studied under James Buchanan. Hinich obtained his Ph.D. in statistics from Carnegie Mellon (and he also held a BS and MS in mathematics) (Hinich

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).

and Munger 2006; Jones et al. 2011). Before the two met at Carnegie-Mellon, Hinich had spent many years working as an applied mathematician for research institutions such as Bell. At Carnegie, Davis was among the promoters of the School of Public Policy and Management, established in 1968. Hinich also worked at Virginia Polytechnic and, later, at the University of Texas in Austin.

The central achievement of the 1966 paper was the formalization of the spatial theory of voting and elections. In doing so, the authors specified Downs' theory more precisely, showing why it is a dominant strategy for each candidate to adopt the median voter's position in the case of a single policy issue. They also extended this result to the case of n-issues, showing that when the density of preferred points is normal, the dominant strategy consists of choosing a policy vector that coincides with the vector of the means of voters' preferred positions. Another result of the paper was the finding that, given two parties, if the variance of the density of preferred points for the first party is much smaller than for the second, the first party may win a plurality even though it is the smaller party (Davis and Hinich 1966, pp. 189–95).

Beyond these formal results, the importance of their analysis lay in the formalization of Rational Political Choice and in a basic generalization of the idea of "Political Space." The problem 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." (Davis and Hinich 1966, p. 176)

Assuming, first, that policies can be measured by certain indices and, second, that these indices can be normalized across voters, Davis and Hinich represent each voter i's preferred position as a vector, $\vec{x_i} = [x_1, x_2, \ldots, x_n]'$. Similarly, each candidate j's preferred policy can be represented by a vector. Each voter can then construct a loss function measuring the distance between his preferred point and a candidate's preferred position:

$$L_i(\theta) = (\vec{x_i} - \vec{\theta})' A (\vec{x_i} - \vec{\theta})$$

This is a quadratic utility function, where A is a symmetric positive definite matrix. The idea of Rational Political Choice can therefore be stated precisely by observing 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 foundational

notions of utility analysis. Each voter then chooses the candidate who minimizes her loss function (Davis and Hinich 1966, p. 178). Though fairly straightforward, these assumptions provide an important point of departure for formally characterizing individual political choices. With two candidates, 1 and 2, each voter i casts his vote for candidate 1 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 when $n = 1$, the dominant strategy for each candidate is to adopt the median voter preference: $\theta_j = \theta^*$. [12]

The idea of an individual loss function was borrowed from statistics. However, it can easily be generalized within a mathematical model: for any n alternatives, a choice function exists that maps preference into a Euclidean space $\mathbb{R}^n$. Accordingly, 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 the formal developments of Spatial Voting Theory from the Seventies onward are based, including two of the most important results in highly formal political science: the extension of the median voter theorem to multidimensional issues (Davis, DeGroot, and Hinich 1972); and the proof of the persistence of intransitivities in multidimensional voting models (McKelvey 1975; see also Austen-Smith 2006)

That a foundational contribution such as Davis and Hinich's 1966 paper was presented at one of these conferences on Mathematical Political Science is evidence that the efforts of Riker and his affiliates were beginning to bear fruit. Returning to the conferences themselves, it is also important to stress that, beyond strictly theoretical contributions, the foundations of the models were themselves a topic of discussion. For instance, at the fourth conference, organized at the University of Virginia, where Bernd had moved, Otto Davis explored methodological issues that complemented those addressed by Riker in his early work (Davis 1968).

After Bernd moved to Virginia Tech, it became the host of the subsequent six conferences on mathematical political science, which continued until 1973. Eventually, both Davis and Hinich joined the "Public Choice Society" (first established as the "Committee on non-market decision making" and later renamed the "Public Choice Society" in 1968) and served as its president (Davis between 1970 and 1972. Hinich between 1992 and 1994). Before them, Riker himself held the same position: his appointment in 1966, immediately after the mandates of Buchanan (1964) and Tullock (1965), shows how close the Rochester group was to the activities of the Virginia group.

[12] 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:$P(x \leq \theta^{)} \leq \frac{1}{2}$ and $P(x \geq \theta^{)} \geq \frac{1}{2}$. 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 (Davis and Hinich 1966, pp. 181–2).

While there is no doubt that the impact of Public Choice on postwar social sciences was 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 subfields ran in parallel and grew together.[13]

Four years after the first meetings of the Committee on Non-market Decision-Making," together with the name change 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 and Tullock and favorably reviewed *The Calculus of Consent* in 1962 (Riker 1962a). Moreover, in their work, Buchanan and Tullock also analyzed simple majority voting through game theory in characteristic-function form, albeit in an elementary model. In doing so, it is apparent that they benefited from Riker's comments (Buchanan and Tullock 1962, p. 150).

The principal difference between the two approaches lies in Buchanan and Tullock's normative stance (even if their analysis also included a positive dimension) and in its connection to the analysis of public finance from a theoretical perspective. Moreover, Buchanan and Tullock ultimately favored Chicago-style price theory over game theory and mathematical modeling (even if in their 1962 work they briefly explored some game-theoretical notions regarding n-person games).

In 1966, Mancur Olson, who the year before had published his pivotal analysis of collective choices (Olson 1965), submitted to the Social Science Research Committee a request to create an interdisciplinary committee to organize a series of conferences on non-market decision-making (Medema 2000). This proposal paralleled a similar initiative advanced by Riker in August 1966 (Bryce Wood to Riker, September 20th, 1966, Riker n.d., Box 29). Unfortunately, both requests were rejected, but the Research Committee opted to sponsor a new conference and, in particular, a summer training program in mathematical political science.

Riker served as chairman of the fourth Conference on Non-Market Decision-making, held in Chicago on 8–9 December 1967. During the two-day conference, participants discussed an econometric model of fluctuations in U.S. voting behavior developed by Gerald Kramer, as well as a debate concerning the frequency of voting paradoxes. Finally, sociologist James Coleman presented a paper on the marginal utility of voting (Coleman 1968). The list of invited participants gives a sense of the network Riker had assembled by the late Sixties. Among others, it included familiar names such as Davis, Downs, Olson, Kramer, and Ordeshook; cutting-edge political and social scientists such as James Coleman, Vincent Ostrom, Hayward Alker, and Philip Converse; and economists and philosophers such

[13] For a history of the genesis and development of Public Choice, see Medema 2009; a highly critical (and criticized) perspective is offered by: MacLean 2017; see also Boettke 2019; Levy 2017.

as Oliver Williamson, John Rawls, and the game theorist John Harsanyi (McKenzie n.d., Box 29).[14]

A summer course in Mathematical Political Analysis was held in 1968, jointly sponsored by the Social Science Research Council and the Inter-University Consortium for Political Research (ICPR), at the University of Michigan, Ann Arbor. The course was divided into two parts. The first, directed by Riker, emphasized the main issues of the emerging "Positive Political Theory," such as Spatial Voting Theory and Game Theory. The second, directed by Kramer, examined advanced econometric techniques for political model building ("Report to MSSB on Summer Course in Mathematical Political Analysis," Riker 1968), McKenzie n.d., Box 29). This course is interesting for two main reasons: first, as Riker wrote in the report submitted to the "Mathematical Social Sciences Board" within the Social Science Research Committee[15]: "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 can 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) approach to political science associated with the behavioral revolution.

Taken together, these initiatives portray a vibrant intellectual community centered on Rochester, but with additional layers in Virginia and, especially, Carnegie Mellon, where several Rochester Ph.D.s were recruited (notably Ordeshook and McKelvey). According to Ordeshook, Carnegie Mellon was as crucial to the development of formal political science in Riker's mold during the 1970s as Rochester itself. It was also the institution where Herbert Simon spent most of his career and where he was working when he received the Nobel Prize in Economics in 1978. However, Simon had virtually no influence on political science there. A decisive role was instead played, beyond Davis and Hinich, by the economist Allen Meltzer, Dean of the Business School and himself interested in the causes of government growth, and by William Cooper, who succeeded Davis as Dean of the School of Urban and Public Affairs and recruited McKelvey once he completed his Ph.D. with Riker in 1974 (Palfrey 2005; Ordeshook 2021).

At Carnegie, the group composed of Ordeshook, McKelvey, Davis, Hinich, and Howard Rosenthal (a political scientist and another MIT Ph.D.), James Lange (a sociologist), and Thomas Schwartz (a social choice theorist) produced some of the most important contributions of the second generation of work in "Positive Political Theory," ultimately completing its transformation from the pioneering, but not adequately formal, early contributions of Riker into a fully fledged axiomatic and mathematical research field.[16] In 1978, McKelvey moved to Caltech, establishing the last,

[14] Both Harsanyi and Williamson future Nobelist in Economics, respectively in 1994 and 2009.

[15] To the board belonged, among the others: Noam Chomsky, James Coleman, Robert Dahl, Lionel McKenzie, Frederick Moesteller, and Patrick Suppes.

[16] Among these works, for instance, the first attempt to map congressional preferences

and perhaps the most advanced, from the mathematical point of view, layer of Positive Political Theory in the Eighties and Nineties (Palfrey 2005). By then, one could safely claim that Riker had ultimately concluded his battle to establish American Political Science on formal grounds.

5.2 Some developments of Positive Political Theory

In a review on the "future of a science of politics" (Riker 1977), Riker outlined three ideas that, in his view, were amenable to analytical treatment in a way closely modeled on economic theory. These were: the somewhat intuitive claim that simple-majority elections in single-member districts favor two-party systems; the Median Voter Theorem, and, more broadly, the spatial analysis of voting; and, finally, the "size principle" and coalitional politics. In the previous section, it has been showed what the development of spatial analysis owed to Davis and Hinich (but also Ordeshook and McKelvey) in providing a precise characterization of utility functions for political actors and Euclidean preference space. With respect to the first point—simple-majority elections, also known as "Duverger's law" (named after the French political scientist Maurice Duverger), the topic was examined in a highly formal fashion from the Eighties 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 Riker's most ambitious theoretical contribution. At the same time, it was criticized on both theoretical and empirical grounds, prompting attempts by some of Riker's associates, such as Shepsle and McKelvey, to reformulate it more precisely, still within cooperative Game Theory but not necessarily through the von Neumann and Morgenstern solution. Coalitional politics still occupies a pivotal role in the formal analysis of politics. Therefore, by briefly addressing one of the cornerstone results in this field, namely David P. Baron and John Ferejohn's's 1989 contribution on bargaining in legislatures (Baron and Ferejohn 1989), the following pages try to identify some lines of development that moved away from Riker's original result.

5.2.1 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 situations where coalitions exceed the minimum winning size, it is rational to expel some members in order to attain the maximum payoff. It was allegedly easy to assess Minimum Winning Coalitions empirically by observing real-world politics or political history. This fulfilled Riker's aspiration for a "positive theory" of political

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 Carnegie Mellon between 1974 and 1978.

behavior rather than an exclusively mathematical analysis. The importance of this principle in Riker's theoretical work was further reinforced by the apparent weakness of the second part of his analysis, namely the dynamic model of coalition-building. However, the result was also criticized on both theoretical and empirical grounds.

On the "theoretical side," most critiques focused on Riker's treatment of "equilibrium," which he interpreted in terms of the "realizability" of a Minimum Winning Coalition, as well as on the role of side payments and, finally, on the role of transferable utility. Of these critiques, the first two broadened the perspective of Riker's analysis, producing a shift in the toolbox of game-theoretical coalition analysis from the "stable set" solution concept to the "Bargaining Set." Game theorists Robert Aumann and Michael Maschler first introduced this idea in order to integrate existing cooperative solutions (the "stable set" and the "core") with behavioral considerations (Aumann and Maschler 1964). The intuitive idea behind this notion is that the solution of a game is given by a pair consisting of a stable payoff vector and a coalition structure. For a payoff to be "stable," the members of each coalition must be able to defend their payoff against possible objections by their partners.

Political scientist Robert Butterworth attacked the Size Principle, arguing that a more precise perspective should focus on the efforts made by members of a non-minimum winning coalition to avoid being expelled (Butterworth 1971). Although flawed from a game-theoretical standpoint, this intuition led Kenneth Shepsle to investigate the plausibility of the Size Principle through a more mathematically grounded analysis (Shepsle 1974). He therefore introduced a simplified version of the "Bargaining Set." Yet 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 Smith 1975).

However, in this way, it lost any "positive feature," becoming a purely mathematical result.[17] To overcome this difficulty, McKelvey addressed the problem by dropping the transferable-utility assumption, elaborating, and experimentally testing, together with Ordeshook and Mark D. Winer, a new solution concept for n-person games, the "Competitive Solution"(McKelvey, Ordeshook, and Winer 1978).

In a paper published in the *American Political Science Review*, Butterworth disputed Riker's Size Principle by 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 reframed 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 exchange or "bribery" among players. Sup-

[17] This aspect, together with the apparent simplicity of the mathematical result, one may speculate, also explains why this proof was never published but remained in a mimeographed form as a Carnegie Mellon working paper. Nevertheless, McKelvey, in other words, referenced it.

pose a coalition member's loss from being expelled is greater than what it would cost him to remain in the coalition, even with a lower—or even negative—payoff. In that case, the member could "bribe" his companions in order not to be expelled. This point becomes clear through a simple numerical example.[18]

Take a five-player zero-sum game.[19] There are three possible partitions (not the total number of winning coalition) of winning coalitions: ([1, 2, 3, 4, 5]), ([1, 2, 3, 4]; [5]) and ([1, 2, 3]; [4, 5]). According to Riker's Size Principle, the expected winning coalition must be the minimum winning one, namely a coalition composed of three members. Assume the following characteristic functions for each partition:

1. $v(1, 2, 3, 4, 5) = 0$
2. $v(1, 2, 3, 4) = 20$ & $v(5) = -20$
3. $v(1, 2, 3) = 30$ & $v(4, 5) = -30$

These yield 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. $v(1, 2, 3) = (10, 10, 10)$ & $v(4, 5) = (-15, -15)$

When side payments are permitted, one can ask if it is possible to identify an incentive for three players within a four-player coalition to expel the fourth and reach a minimum winning coalition. From the values above, a three-player coalition yields a total value of 30, meaning that each player could gain an additional 5. For the fourth player, however, the change is a total loss of 20, since he goes from receiving 5 as a member of the winning coalition to -15 as a member of a losing coalition. He may therefore prefer to avert this loss by offering, for example, 6 to each of the other coalition members in order to stay. The four-player coalition is still valued at 20, but the imputation becomes: $(11, 11, 11, -13)$. This clearly dominates the imputation $(10, 10, 10, -15)$, which corresponds to what each of the three players would receive in the winning coalition $v(1, 2, 3)$ and what the fourth player would lose as part of the losing coalition $v(4, 5)$. Therefore, the winning coalition's size will be 4 rather than 3, contrary to the Size Principle.

As Butterworth summarized: "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

[18] Note that Shepsle, as well as McKelvey and Smith, adopted the same numerical example in building upon Butterworth's idea.

[19] 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)

coalition with bribing in operation dominate those of the three-person coalition without bribing." (Butterworth 1971, p. 742) He also showed that this conclusion holds for at least two of the three situations identified by Riker: when the value of the winning coalition decreases as its size grows (the example above: in Riker's terms, when the winning coalition's characteristic function is negatively sloped) and when the slope is zero.

Notice that Butterworth did not reject Riker's analysis completely, but rather generalized it by proposing a new principle, the "maximum number of positive gainers principle". This principle says nothing about the size of winning coalitions. Instead, it asserts that there is a range within which the number of positive gainers must fall (and that 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 in order to reach the minimum winning size, it is not necessary to expel members in order to gain b. Butterworth claimed that each example explained by the "Size Principle" is equally explained by his principle, which, in addition, also clarifies why some winning coalitions may assign negative payoffs to their members.

Riker replied in the same issue of the *American Political Science Review*, focusing on an apparent inconsistency in Butterworth's argument. To claim that a four-player coalition can yield payoffs $(11, 11, 11, -13)$ is equivalent to claiming that a three-player coalition worth 33 exists, which violates the rules of the game, since a three-person coalition is worth 30 (Riker 1971).

Nevertheless, Butterworth had raised several important theoretical issues that needed to be incorporated into a more mathematically rigorous analysis of the principle. This was the aim of Shepsle's paper, published in 1974, in the same Journal of the previous contributions.

Shepsle noted that both Riker's original argument and Butterworth's critique suffered from two major problems. First, the inherent instability of zero-sum essential games; second, the way both authors had used 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."(Shepsle 1974, p. 508, italics in the text.)

Neither Riker's nor Butterworth's reasoning was grounded in a formal solution concept. Riker's reliance on the stable set (von Neumann) and Morgenstern's original solution concept for n-person games) was justified mainly by his dissatisfaction with many subsequent solution concepts (beginning with the "core" and the "Shapley Value") (Riker 1962b, pp. 38–9). Yet this choice weakened the general mathematical consistency of his result. Shepsle's inquiry therefore addressed whether a coalition size could be

identified that, by itself, sustained an imputation in the stable set. Using Butterworth's numerical example, the stable set of the game is:

$$V = \big\{(10, 10, 10, x, -30 - x)| - 20 \leq x \leq -10\big\}$$

This set is infinite, because the losses are determined only up to an interval. Yet there is no explicit mention of the coalition structure that generates particular elements as outcomes of the game. Any coalition whose size is greater than or equal to the Minimum Winning Coalition can sustain an imputation in V.[20] Since coalitions of three, four, or five members can sustain imputations in the stable set, coalition size cannot be determined solely on this basis. This result exposes the flaw in Riker's analysis.

To address this problem, Shepsle first introduced two behavioral constraints. First, he defined the *acceptable coalition structure* for each outcome in V.[21] Second, each player is constrained only by the characteristic function. In doing so, Shepsle accepted one of Butterworth's central points, namely the possibility of bribery by coalition members. He argued that, given a subset V^* of V and the set:

$$V - V^* = \big\{(10, 10, 10, x, -30 - x)| - 20 < x < -10\big\}$$

any coalition C with $|C| = 4$ fails to satisfy the first constraint. Therefore, if the outcome of the game lies in $V - V^*$, only coalition structures that include a Minimum Winning Coalition are acceptable. But this is not the case for $V^* = \{(10, 10, 10, -10, -20)\}$ (Shepsle 1974, p. 513). Hence: "the size principle will be strongly sustained in this game if there is some reason to exclude the $y \in V^*$ as likely outcomes. This can be done through a careful specification of the second constraint such 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 a Minimum Winning Coalition." (ibidem) In other words, the Size Principle depends heavily on "sociological" considerations about the structure of the game.

To reinforce his argument, Shepsle adopted an idea closely related to another cooperative solution concept, namely the "Bargaining Set." Unlike the Stable Set, this concept 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 payoff vector 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)

[20] To better see this point, take $|C| = 3$ (which is a Minimum Winning Coalition). 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.

[21] Formally, it means that each coalition that is formed 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 element in the stable set, together with a coalition structure K_1 that contains only the three positive gainers, is both an IRPC and an APC.[22]

Since, in Shepsle's view, Butterworth's most valuable point was precisely the internal activity of coalition members (and Shepsle's second behavioral constraint addressed this issue), the bargaining-set framework was well suited to operationalize it.

Formally, consider a IRPC $(x; K)$, and $s, t \in I$. Player s has an *objection* against t (denoted $[y^s(t); B]$) if there exists a coalition B, with $s \in B$ and $t \notin B$, and a payoff vector y satisfying the following three conditions:

$$\sum_{i \in B} y_i = v(B)$$

$$y_s > x_S$$

$$y_i \geq x_i \quad \forall i \in B$$

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:

$$\sum_{i \in C} z_i = v(C)$$

$$z_i \geq x_i \quad \forall i \in C$$

$$z_i \geq y_i \quad \forall i \in B \cup C$$

If every objection to an IRPC $(x; K)$ has a counterobjection, then $(x; K)$ is *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 in which "*both gains and losses are symmetrically shared by a minimum winning coalition and a maximum losing coalition.*" (Shepsle 1974, p. 515)

Although he restricted his analysis to the five-player game and did not generalize it, he 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

[22] To see this point: IRPC is satisfied when: $x_1 = 10 > -20$; $x_2 = 10 > -20$; $x_3 = 10 > -20$; APC is satisfied when: $10 + 10 + 10 \geq 30$ and $x - 30 - x = -30$

> 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."(Shepsle 1974, p. 515, italics in the text)

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 had treated the bargaining set as a subset of the stable set, which is valid only under certain conditions. Further, in his example, objectors and counterobjectors did not belong to the same coalitions, contrary to what the theory requires (McKelvey and 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 [maximum number of positive gainers] principle are not based on any formal solution concept is correct." In particular, they showed that in a large class of games the size principle follows directly from the assumption of "internal stability" of coalitions, which bargaining-set theories implement through the requirement of "coalitionally rational payoff configurations." (McKelvey and Smith 1975, pp. 3–4)

Formally, a payoff configuration $(\mathbf{x}, B) = (x_1, \ldots, x_n; B_1, \ldots B_m)$ is a pair with $x \in \mathbb{R}^n$ and B a partition of N such that $\sum_{i \in B_k} x_i = v(B)$ for $k = 1, \ldots, m$. Here, x_i represents the payoff to player i. That is, coalitions that form must receive their value. For internal stability of payoff configurations, two stronger conditions are required:

1. Individual rationality if and only if $x_i \geq v(i) \ \ \forall i \in N$. This condition requires that every individual receive his value.

2. Coalitional rationality if and only if $\sum_{i \in T} x_i \geq v(T) \quad for \ \ T \subseteq B_k \in B$. This condition requires that every subset of an extant coalition receive its value.[23]

McKelvey and Smith showed that the internal-stability conditions implied by coalitional rationality generate the size principle. The result can be understood by returning to the standard numerical example introduced by Butterworth. In that example, only three-person coalitions within the coalition structure receive their value (i.e., are coalitionally rational payoff configurations). Indeed, any three-member subset of a larger four-person coalition receives less than the value it could obtain by forming a coalition on its own (since within a four-person coalition any three members jointly receive 15, whereas a three-person coalition receives 30). Hence, a winning coalition maximizes its value by reducing its size to the minimum winning size. Moreover, the same logic applies to four-member winning coalitions (namely, those in which the fourth player "bribes" his way into the coalition).

[23] 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.

While the result held for the example above, the two authors generalized it to a broad class of games, especially simple and symmetric games.[24] They added two further formal conditions. The first concerns how a coalition's value changes when an additional player joins. Suppose a player is added to a minimum winning coalition and the marginal value added is less than the proportional share each player is receiving. In that case, the game exhibits "decreasing returns to scale."[25] The second condition concerns the "inessentiality" of a subset of players within a coalition. A set of players is inessential in a coalition if they can do just as well by not joining. They then proved two main theorems: in a simple game, if a payoff configuration is coalitionally rational for some $B_k \in B$ with $B_k \in W$, then either $B_k \in M$, or some individuals in B_k are inessential for the coalition (that is, a winning coalition is either minimal, or some individuals can obtain the same payoff by not joining it). The second theorem concerns symmetric games with decreasing returns to scale (again under coalitionally rational payoff configurations) and states that, in this case, if a coalition is winning, then it is also minimal winning.

These results illustrate how fruitful Riker's insights were. Moreover, in the late Sixties and early Seventies, other young scholars, such as the future Nobelist Robert Wilson, then a graduate student at the Stanford Business School, began to analyze social choice theory and voting through cooperative game theory (Wilson 1971; Wilson 1972).[26] However, it also became apparent that, once fully axiomatized, the size principle and coalition theory lost much of their simplicity and their alleged predictive power.

5.2.2 The game-theory revolution and its impact on "Positive Political Theory."

Coalition theory occupied a central role in formal political theory. The attempts to solve the difficulties discussed above, then, followed two main paths. The first remained within cooperative Game Theory but abandoned the crucial assumption of transferable utility. The second adopted bargaining theory in extensive-form games, namely, embraced the most recent developments in non-cooperative Game Theory. The first line of research includes the new solution concept for n-person games without transferable utility, namely McKelvey'sand Ordeshook's "Competitive Solution" (McKelvey, Ordeshook, and Winer 1978; McKelvey and Ordeshook 1978). The second path is exemplified by one of the most influential models in

[24] 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).

[25] 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."

[26] That the issues of coalitions and voting are tied can be 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 example relates the lack of a Condorcet Winner (the "Condorcet Paradox") to the emptiness of the core.

formal political science: David P. Baron's and John Ferejohn'sanalysis of bargaining in legislatures (Baron and Ferejohn 1989).

A passage from Ordeshook is worth quoting 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." (Ordeshook 2007, p. 183)

In cooperative games, the assumption of transferable utility is contained directly in the definition of the characteristic function. This is, as seen, a real-valued function that assigns to each coalition a numerical value. This value represents the quantity of utility that coalition members can secure and distribute among themselves. If utility is assumed to be linear in money and side payments are allowed, then the coalition's value is linear in the exchanges among players.[27] McKelvey and others showed that this assumption becomes untenable when preferences are Euclidean. In such cases, it can be shown that for some groups of voters the value of their coalition cannot be summarized by a single number (McKelvey, Ordeshook, and Winer 1978, pp. 194–5). This observation calls for redefining the characteristic function as a set-valued function rather than a real-valued one, that is, $v(S) \subseteq \mathbb{R}^n$.

Once the assumption of transferable utility is abandoned, however, many results in cooperative coalition theory no longer hold. To address this issue, McKelvey and Ordeshook developed a new solution concept for n-person games without transferable utility, namely the "Competitive Solution." They explored this idea analytically in a paper presented at a conference on Game Theory and Political Science sponsored by the Mathematical Social Science Board of the Social Science Research Council (McKelvey and Ordeshook 1978). In addition, they published a paper with a third political scientist, Mark Winer, in which the concept was also tested experimentally (McKelvey, Ordeshook, and Winer 1978).

[27] Note, however, that transferable utility is a technical hypothesis and does not preclude that players exchange resources among themselves.

The rationale behind this new solution was the idea that "potential coalitions must bid for their members in a competitive environment via the proposals they offer." Since several coalitions attempt to form simultaneously, an efficient bid rewards the pivotal members of each coalition (McKelvey, Ordeshook, and Winer 1978, p. 200). A proposal by a coalition $C \subseteq N$ is defined as an ordered pair (u, C) such that $u \in v(C)$ and $u \in v(N)$. A proposal (u_1, C_1) is considered viable if, for any other proposal (u_2, C_2), it is not the case that $u_1 <_i u_2$ for all $i \in C_1 \cap C_2$, that is, for the set of pivotal players common to both coalitions. In other words, a proposal is viable if the pivotal players do not strictly prefer another proposal over the one in which they are pivotal. Let K be a set of proposals. A proposal is viable in K if it is viable against all proposals in K.

To define the "Competitive Solution," two additional conditions are required. The first concerns the set K. If each coalition in K has exactly one proposal, and if all proposals in K are viable against one another, then K is said to be balanced. Since multiple balanced sets may exist, the proponents of the competitive solution focused on those balanced sets that are not upset by any alternative proposal—that is, proposals that are "as attractive as possible" to their pivotal members. A "Competitive Solution" is therefore defined as a set K that is balanced and not upset by any proposal (u, C).

Beyond its formal properties, the competitive solution was also intended to improve the predictive power of coalition theory. As McKelvey observed, "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, Ordeshook, and Winer 1978, p. 189) The main advantage of the competitive solution, therefore, lay in its potential for empirical testing.

These claims may appear surprising given McKelvey's strong commitment to highly formal analysis. However, they closely echo Riker's emphasis on positive political analysis and on the need for analytically precise and empirically grounded theories. Another point of continuity with Riker's approach was the continued reliance on cooperative game theory. Yet, as Riker himself later remarked, "unfortunately, this enterprise comes just at the time that, in economics, theorists are abandoning cooperative theory for non-cooperative theory." (Riker 1992, p. 219) He was referring, of course, to the game-theoretic revolution that reshaped economics from the late Seventies and early Eighties onward.

One of the forces driving that revolution was precisely the perceived lack of strong behavioral foundations in cooperative Game Theory. An issue only partially addressed by solution concepts such as the bargaining set.[28]

Perhaps nothing illustrates how marginal non-cooperative Game Theory remained for political scientists as late as the Seventies better than a look

[28] Another factor was the development of the Nash equilibrium concept in the Sixties and Seventies, including its extension to extensive-form games.

at Riker's and Ordeshook's textbook *An Introduction to Positive Political Theory* (Riker and Ordeshook 1973).[29] Of the twelve chapters in the book, four are devoted to game theory, amounting to 124 pages. More striking, however, is the way the material is organized. One chapter deals with n-person games, one with power indices, one with the size principle, and one with two-person games. The classification does not revolve around the key distinction between cooperative and non-cooperative games, that is, whether communication is unrestricted and binding agreements are possible, but rather around the number of players. Under the heading of n-person games, the authors discuss only coalitional games, without even mentioning non-cooperative n-person games. Non-cooperative analysis and the Nash equilibrium appear only in the context of two-player games, alongside the bargaining problem (a cooperative solution). In the authors' taxonomy, two-person games are divided into zero-sum and non-zero-sum, finite and infinite, cooperative and non-cooperative. Yet very little attention is devoted to the Nash Equilibrium itself, partly because in the types of games emphasized, namely zero-sum, finite, non-cooperative games, the Nash Equilibrium coincides with von Neumann's minimax solution. Notably, no attention at all is given to the cutting-edge work of Harsanyi and Selten on extensive-form games and incomplete information.

Neglecting Harsanyi's and Selten's contributions greatly diminishes the relevance of game theory for theoretical analysis in both economics and political science, as David Kreps's words from 1991 clearly indicate:

> "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, italics in the text)

Why, then, did Riker and Ordeshook overlook these developments? Several explanations are possible. One is that these contributions were not easily accessible to non-specialists. Selten's early work, for instance, was written in German and remained untranslated, even though he later expanded his ideas in a major 1975 paper published in the *International Journal of Game Theory*. Harsanyi, although part of the American economics community and a Ph.D. student of Kenneth Arrowat Stanford, was based at Berkeley's Business School and thus somewhat peripheral to the core of economic theory. Nonetheless, the community of game theorists was small, and both Selten and Harsanyi maintained close ties with figures such as Aumann, Maschler, and Shapley. Another explanation concerns Riker's and Ordeshook's primary sources for game theory. Apart from the canonical but increasingly outdated work of Luce and Raiffa, their

[29] A similar argument can be raised for another comprehensive review of game theory and politics, that of Steven J. Brams (Brams 1975)

main references were the introductory and non-technical texts by Anatol Rapoport and Morton Davis, published in 1966 and 1970.

These explanations, however, are not fully satisfactory. As shown earlier, Harsanyi had some contact with the public choice community, and Riker had acquaintances among game theorists in the late Sixties. More importantly, while Riker's command of advanced mathematics was limited, Ordeshook's was not. His knowledge of game theory exceeded what the joint textbook suggests. Indeed, although game theory did not occupy a central place in the Rochester economics department, a common feature of most economics departments in the Sixties and Seventies, the department did include the young economist and game theorist James Friedman. While less famous than Harsanyi or Selten, Friedman is well known for his critical applications of game theory to market theory.[30] Friedman helped Ordeshook to expand his understanding of game theory well beyond the introductory level of Luce and Raiffa (Ordeshook 2021). Thus, when Ordeshook worked on the textbook, his knowledge of game theory was probably unmatched among political scientists, with the exception of McKelvey.

The most convincing explanation for Riker's and Ordeshook's relative indifference to non-cooperative Game Theory lies perhaps elsewhere. Scientific innovations are rarely adopted immediately, and Harsanyi's and Selten's contributions did not settle the problem of how to model non-cooperative extensive-form games.[31] It is therefore not surprising that Riker remained attached to the form of Game Theory that had originally inspired him, namely coalitional games. Politics, in Riker's view, involves both enforcement and the aggregation of interests.[32] One of the most appealing features of non-cooperative Game Theory is that Nash equilibria are self-enforcing, a feature closely resembling the functioning of competitive markets. In politics, however, choices and actions are enforced by institutions, and institutions themselves are the product of deliberate and binding agreements. Despite its reliance on somewhat loose behavioral assumptions, n-person cooperative theory allows one to focus on the properties of institutional arrangements and provides a natural framework for combining heterogeneous interests through imputations and set-valued solutions.

Subsequent developments in non-cooperative Game Theory showed that similar insights could be achieved using point-valued solutions such as Nash Equilibrium. Not surprisingly, the most successful results also incorporated cooperative elements, as in bargaining theory. The development of such models was one of the major achievements of game theory in the Eighties. One of the most influential of these models is Baron's and Ferejohn's analysis

30 He also contributed to the first formal proof of the so-called "Folk Theorem," a foundational class of results relating subgame-perfect Nash equilibria to the length of games.

31 Consider, for example, the notion of Subgame Perfect Equilibrium, Perfect Bayesian Equilibrium and Sequential Equilibrium, developed in the Eighties.

32 In the introduction to their textbook, the authors identify three core political processes: "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 Ordeshook 1973, pp. 2–7)

of legislative bargaining.

They published their work in the *American Political Science Review* in 1989 (Baron and Ferejohn 1989). The central issue they addressed was endogenous agenda formation in a unicameral legislature operating under majority rule, which they modeled using non-cooperative Game Theory. Social choice theory has shown that voting equilibria are often unattainable in multidimensional settings, making it possible, following McKelvey's most famous result, for an agenda setter to engineer virtually any outcome by sequencing votes appropriately. Earlier, in 1979, Shepsle had shown how equilibrium could be achieved in multidimensional spaces by structuring decisions across multiple issues, thereby guaranteeing a Condorcet winner for each single-issue vote, an idea he termed "structure-induced equilibrium" (Shepsle 1979). Shepsle thus recast a social choice problem as an institutional one, focusing on committee systems in legislatures. His equilibrium, however, remained "based on a concept of stability drawn from the field of social choice." (Baron and Ferejohn 1989, p. 1181)

Like Shepsle, Baron and Ferejohn adopted an institution-based approach, emphasizing the sequential processes of proposal making, amendment, and voting. Unlike him, however, they modeled these processes as a non-cooperative, multi-session game and demonstrated the existence of a Nash Equilibrium. Moreover, the equilibrium outcome varies with institutional rules, particularly whether the legislature operates under a "closed rule" or an "open rule." Under a closed rule, amendments are not allowed, and proposals are voted directly against the status quo, yielding only win-or-lose outcomes. Under an open rule, amendments may be proposed, and the identity of the member who proposes or calls for a vote, the "recognition rule", becomes crucial. At the beginning of a session, each member i has a probability p_i of being recognized. Given a feasible set of proposals X, a proposal is a vector $x^i = (x_1^i, \ldots, x_n^i)$ such that $\sum_{j=1}^{n} x_j^i$. In a closed-rule system, only one member is recognized; in an open-rule system, another member j may be recognized with probability $\frac{p_j}{\sum_{k \neq i} p_k}$ and propose an amendment. The resulting model is therefore substantially more complex.

Baron and Ferejohn represent each legislator's preferences using the utility function $u^j(x^k, t) = \delta^t x_j^k$, where t denotes the session in which the allocation x^k is adopted. Each player's pure strategy s_τ^i is a set of functions mapping the history H_τ (the sequence of events up to time τ) into the set of proposals X if player i is recognized, or into the set {yes, no} otherwise. Mixed strategies are probability distributions over pure strategies. Each player also has a continuation value $\delta v_i(t, g)$, representing the value to player i if the legislature moves to subgame g.

The model describes a bargaining process in which proposals are made and either accepted or rejected. As discussed earlier, Nash provided the first systematic analysis of bilateral bargaining (Nash 2002b). Rubinstein later developed an axiomatic model of bilateral bargaining, in which two players bargain over splitting a dollar (Rubinstein 1982). In Rubinstein's model, the first player proposes a division $[p, 1 - p]$. If accepted, the game ends; otherwise, the second player makes a counterproposal, and

the process continues. Each player is impatient, with a discount factor δ reducing payoffs over time. The game is non-cooperative and features complete information, with strategies defined as sequences of proposal and response functions. Because the game is multistage, equilibrium must be subgame perfect. Rubinstein showed that the unique equilibrium outcome involves immediate acceptance of the first proposal. This result generated an extensive literature.

Baron and Ferejohn's model belongs to this tradition but differs in important respects. First, Rubinstein's model is bilateral, whereas Baron and Ferejohn's is multilateral. Second, impatience plays a different role: in Rubinstein's model, equilibrium allocations converge to equality as impatience diminishes, whereas no analogous result emerges in Baron and Ferejohn's framework. Third, equilibrium outcomes depend explicitly on institutional rules. Under a closed rule and a finite horizon, the first proposal is accepted, resembling Rubinstein's result. Under an open rule, however, outcomes depend on factors such as impatience and the number of legislators.

One of the most influential findings of Baron and Ferejohn's model is that amendment rules affect the equity of outcomes and the distribution of resources. As they state:

> "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 stress that this result does not directly contradict Riker's Size Principle.[33] Minimal majorities may still arise. However, the nature of the result is fundamentally different from Riker's. Earlier, we argued that Riker's coalition analysis was flawed in two respects. First, the Size Principle, although intuitively appealing, lacked a solid game-theoretic solution concept and behavioral foundation. Second, Riker's "dynamical model" failed to articulate the underlying bargaining process clearly. Through the bargaining set and the competitive solution, scholars such as Ordeshook and McKelvey addressed the first issue. But, as Baron and Ferejohn observed, "Cooperative models of politics abstract from the

[33] A table in Baron and Ferejohn's paper reports equilibrium outcomes for different discount factors and legislature sizes (Baron and Ferejohn 1989, p. 1197)

process by which alternatives arise and assume that coalitions will freely form to defeat alternatives when a majority of members prefers another available alternative." (Baron and Ferejohn 1989, p. 1200) Recasting the problem as a non-cooperative bargaining game allowed Baron and Ferejohn to highlight the sequential nature of legislative decision-making and to accomplish what Riker had attempted nearly thirty years earlier. The result, like much else in this story, underscores the ambivalent legacy of Riker's work: on the one hand, its limitations, perhaps rooted in excessive optimism about cooperative Game Theory; on the other, the profound insights it generated for the analysis of political phenomena in a new, more scientific outlook.

Chapter 6

Conclusions

These conclusive pages advance a set of reflections on the current status of Rational Choice Theory and Positive Political Theory within contemporary debates in Political Science. The heyday of Rational Choice in the discipline occurred during the Eighties. Since the beginning of the new millennium, however, enthusiasm for this theoretical approach has substantially declined. One explanation lies in the methodological debates of the Nineties, when Rational Choice theorists were called upon to respond to the apparent lack of empirical validation for many of their analyses. For several political scientists, the outcome of this debate crystallized the definitive failure of the expansionary ambitions of Positive Political Theory within Political Science (Green and Shapiro 1994; Friedman 1995). As a consequence, the theoretical and formal sophistication achieved during the Eighties and Nineties has increasingly been absorbed into economic theory, often under the broader label of "political economy."

These pages also outline how reconstructing the entry of Game Theory into Political Science complements the broader historiography of the theory of games.

Formal Political Theory and Mathematical Economics. Riker's "Dilemma"

Behavioralism did not provide political science with either a unified method or a single theoretical framework, in the way that the postwar mathematical approach did for neoclassical economics. Nevertheless, Behavioralism represented the mainstream of the discipline throughout the Fifties and Sixties. After that period, however, no comparable unifying paradigm emerged in political science.

The decline of behavioralism resulted from multiple causes, most notably the dramatic transformations affecting American social sciences and society in the late Sixties. The Vietnam War and the student movement demanded new approaches to social inquiry, and no discipline was immune.[1]

[1] A notable parallel can be found in economics. In 1968, to protest the decision to hold the American Economic Association meeting in Chicago, where police had violently repressed largely peaceful demonstrators, several prominent economists, including Lawrence Klein (later Nobel laureate in 1980), organized an alternative meeting in Philadelphia, an unprecedented and never-repeated event.

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Many younger scholars began to question political science's role in interpreting, and potentially legitimizing, these events. Within the heterogeneous anti–Vietnam War movement, the "Caucus for a New Political Science" was established in 1967 (Dryzek 2006).

Despite resistance from behavioralists, David Easton's 1969 American Political Science Association presidential address attempted a conciliatory response to these emerging critiques. Easton explicitly referred to a "new revolution in political science" and articulated seven core tenets of what he called a "Credo of Relevance" (Easton 1969, p. 1052). These included claims such as the priority of substance over technique and the contention that behavioral science concealed an ideology of empirical conservatism.

More generally, the rallying cry of the post-behavioral revolution involved a politicization of the profession. In particular, it rejected the pretense of value-free social science, one of the defining features of Behavioralism. This rejection was motivated, in part, by the observation, explicitly made by Easton, that political science as a discipline had failed to anticipate the social and racial crises that engulfed the United States in the latter half of the Sixties.[2]

Finally, Easton also issued a call for what he described as "creative speculation." By this expression, he meant, following "the great political theorists of the past", the formulation of "new and often radically different conceptions of future possible kinds of political relationships" (Easton 1969, p. 1058). Beyond what might appear as a mere appeal to utopian thinking, Easton advocated "boldly speculative theorizing" that would build upon, rather than reject, the findings of contemporary behavioral science, and that would reflect on the implications of these findings for political life within alternative and explicitly articulated value frameworks (ibidem).

As discussed earlier, Riker's attitude toward Behavioralism exhibited little, if any, hostility. On the contrary, his theoretical agenda ran parallel to and, at least in his own assessment, complemented the reformist aspirations advanced by Behavioralists. At the same time, however, he identified what he regarded as the major shortcomings of Behavioralism, most notably its weak theoretical foundations and its lack of a clear conception of individual action. As Riker put it, "the behavioralists [...] were totally atheoretical. They had no picture of human beings." (Riker and Shepsle 1979, p. 21)

Moreover, Riker understood the development of political science as a cumulative enterprise, in which superior theories gradually supplanted or complemented earlier ones. For this reason, he did not align himself with the post-behavioral revolution, primarily because the call for politicizing the discipline appeared to him conceptually misguided. He even seemed to reject the very label "Post-Behavioral." In his view, the call for an orientation of political science beyond mere empirical data collection could be adequately captured by the simple notion of "science" itself. By contrast, he argued that "the post-behavioral revolution, as other people use the word, seems

[2] As evidence, Easton pointed to the negligible number of articles published in the American Political Science Review between 1958 and 1968 addressing issues such as racial conflict, urban crises, poverty, violence, and civil disobedience.

to refer to the success [that] Straussians and others of that sort have had in persuading people that science is biased" (Riker and Shepsle 1979, p. 127). While Riker did not oppose the practical or reformist motivations behind the project of a science of politics, he insisted that even such an enterprise required rigorous scientific and methodological foundations that, in his view, Behavioralists had acknowledged, but which Post-Behavioralists were threatening to undermine.

Riker never abandoned his methodological commitment to what he called a "genuine science of politics." At the core of this commitment lay a precise expectation: the possibility of generating genuine explanations and predictions in the social sciences, analogous to those seemingly produced by economics. Yet his conception of political theory evolved in a markedly different direction. This shift was closely related to the feasibility of producing reliable predictions in political science and to the role of what he termed "political craftsmanship," as distinct from straightforward models of rational action. It also illuminates how Riker assessed the achievements of Positive Political Theory and the relationship between economics and political science.

A key text for reconstructing Riker's evolving stance toward both economics and formal political theory is a 1977 review essay, aptly titled *The Future of a Science of Politics* (Riker 1977). In this piece, Riker once again criticized particularistic explanations of political events, emphasizing instead the importance of general analysis. This position rested on what he explicitly described as a "positivistic view of science," whose core requirement was the existence of scientific laws, understood as "well-verified generalizations." Importantly, Riker defined scientific laws not only as empirical regularities discovered through observation, but also as theorems derived from axiomatic systems. He appeared to attribute equal scientific status and explanatory force to both.[3]

For Riker, the paradigmatic model of such a science was Price Theory, which, he argued, "satisfies, in structure and outcome, [his] notion of what a science is just as well as, perhaps, physics." (Riker 1977, p. 22) Price Theory holds that prices are determined through the equalization of supply and demand in a competitive market. The law of demand, for instance, can be derived by empirically validating how quantities demanded respond to changes in prices. This empirical evaluation can then be extended through axiomatic reasoning, yielding a theory of consumer choice. Although Riker regarded the supply side of Price Theory as less empirically intuitive than the demand side, economists ultimately generalized both into a theory of competitive equilibrium. As he summarized:

> "[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

[3] "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).

> 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, 21–22)

As noted earlier, Riker identified three core political problems that, in his view, were amenable to theoretical treatment analogous to Price Theory: spatial models of electoral competition, the influence of voting rules on electoral outcomes, and theories of political coalitions, particularly the size principle. He also outlined several conditions favorable to the development of a rational choice approach in political science. First, these theories possess what he termed the "essential structure of science," insofar as they rest on empirical laws that can be axiomatized to generate theorems about strategic interaction. Second, they incorporate the concept of equilibrium, shifting attention away from subjective motives and toward the outcomes generated by purposive interaction. Third, they typically concern small-scale and frequently repeated events, a point already discussed in the philosophical literature of the late Fifties. Finally, rational choice theory addresses the problem of intentionality without excluding judgments about motives or regularities in human behavior. On this last point, Riker went so far as to claim that "the assumption of rationality serves just about the same function in social science that the principle of mechanics once served in physical science." (Riker 1977, p. 32)

However, Riker eventually became deeply skeptical about the possibility of deriving useful predictions in political science, even through game-theoretic or formal modeling. As a result, his focus shifted from equilibrium to disequilibrium (Riker 1980). He interpreted equilibrium as a stable configuration of preferences, and disequilibrium as the failure to reach such stability. In the textbook he co-authored with Peter Ordeshook in 1973, Riker proposed a threefold typology of "social equilibria." First, a "strong, unique equilibrium," arising from interactions so precisely specified and goals so narrowly defined that society will certainly converge upon it. If disturbed, the system will return to equilibrium as quickly as possible. Competitive price formation served as the canonical example, while in political science, this type of equilibrium was associated with certain results in Social Choice Theory. Second, they identified a "weak, unique equilibrium," produced by more complex interactions oriented toward more complex goals—monetary macroeconomics being their primary example. Finally, they described "non-unique (unstable) equilibria," in which outcomes belong to a set of possible results, with interaction leading to some unspecified point within that set rather than to any particular outcome (Riker and Ordeshook 1973, 150–151). Riker classified the size principle as an instance of a weak, unique equilibrium (Riker and Ordeshook 1973, p. 177). Notably, this taxonomy says little about the mathematical existence of equilibrium and instead reaffirms the emphasis on prediction.

In his 1980 article, Riker drew an even more explicit connection between equilibrium and prediction. He defined economics as a discipline that "admits predictions of an equilibrium" and attributed its prestige among the

social sciences to the "actual occurrences of numerous predicted equilibria." (Riker 1980, p. 434) Because he equated equilibrium with the ability to predict concrete outcomes, Riker viewed political science as particularly vulnerable to impossibility theorems such as those developed by Richard McKelvey and Norman Schofield in the Seventies. These results demonstrated that, in multidimensional policy spaces, collective choice almost invariably generates cycles, thereby undermining the global transitivity required for consistent social choice.[4] Riker interpreted these findings as evidence that something analogous to economic equilibrium might be unattainable in political contexts. Consequently, political science necessarily involved the study of persistent disequilibrium. He famously concluded that:

> [Not economics but] "politics is the *dismal* science because we have learned from it that there are no fundamental equilibria to predict. In the absence of such equilibria we cannot know much about the future at all, whether it is likely to be palatable or unpalatable, and in that sense our future is subject to the tricks and accidents of the way in which questions are posed and alternatives are offered and eliminated." (Riker 1980, p. 443)

Note that Riker's earlier analysis of political coalitions already contained a closely related intuition. Indeed, he devoted several pages to a largely verbal discussion of the implications of persistent disequilibrium for his model. At that stage, however, Riker still appeared convinced that a sufficiently refined theory might ultimately resolve this difficulty. Later, he came to believe that overcoming this obstacle required a more fundamental step, one that could not be achieved through purely mathematical refinement. To address this decisive problem, Riker argued for a renewed focus on institutions, particularly on their role in shaping political outcomes through the capacity of rational actors to deploy manipulative rhetorical strategies in order to secure preferred results. This emerging research program did not abandon rational choice theory or game-theoretic tools as such. Rather, it rejected the notion that political outcomes could be rendered predictable. As Riker himself put it: "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

[4] 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 (McKelvey 1975; Riker 1980).

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 argument is particularly significant for political theory, insofar as he used the persistence of disequilibrium to articulate a conception of liberal democracy opposed to a populist one: that is, a Madisonian view contrasted with a Rousseauian one. The crucial distinction between these two conceptions lies precisely in the fact that the former does not incorporate anything analogous to a "general will," understood as an equilibrium representing a stable configuration of preferences and values. According to Riker, democracy is better defined by its rules than by its outcomes, since outcomes are always susceptible to manipulation through strategic voting and rhetoric. It was in this context that Riker coined the term "heresthetics" to describe these manipulative practices.[5]

This intellectual trajectory appears to imply a substantial retreat from an "economic-style" conception of formal political theory. Once the significance of equilibrium is called into question, the prospect of developing a discipline grounded in formal analysis, such as utility theory or Game Theory, rather than merely mimicking economic ideas becomes deeply problematic. For this reason, Riker's paper elicited a direct response from other formal political theorists. Peter Ordeshook, in particular, replied in the same issue of the *American Political Science Review*, defending a position much closer to that of mathematical economics, especially with respect to the centrality of equilibrium (Ordeshook 1980).[6] Ordeshook addressed only briefly the issues of equilibrium reliability, why Nash Equilibria are played, or whether coalitions are stable. Instead, he focused on the meaning of equilibrium in economics and, by extension, in formal political theory. He summarized Riker's position 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" (Ordeshook 1980, p. 447). Ordeshook, however, remained unconvinced by what he saw as the "new" Riker.

First, Ordeshook argued that Riker attributed excessive importance to

[5] From the late Seventies until his death in 1993, Riker devoted the final phase of his scholarly career to the further development of these ideas. Among his later works, *Liberalism against Populism* (Riker 1982) is arguably the most influential. In this book, Riker revisited and extended the arguments of his 1980 article, while also offering a historical narrative designed 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 examined how slavery emerged as a national political issue in the decades preceding the American Civil War. In subsequent works, beginning with his presidential address to the *American Political Science Association*, Riker pursued similar analyses of other episodes in American political history, starting with the adoption of the U.S. Constitution (Riker 1984; Riker 1986; Riker 1996). Riker's historical reconstructions have, however, been contested on empirical grounds (for discussions and assessments of these criticisms, see McLean 2002; McLean 2009).

[6] In this reply, Ordeshook's position differed from that advanced in the earlier textbook, suggesting that the relevant sections may have reflected primarily Riker's authorship.

equilibrium understood as stability, and therefore assumed, incorrectly, that price theory itself rested on stable markets. In reality, he contended, "the presumed stability of markets is an abstract fiction that most economists recognize as a theoretical impossibility" (Ordeshook 1980, p. 447, italics in the original). Stability exists within the formal model, but not necessarily in the real world.

The same reasoning applies to predictive power. Abstract market models may predict how changes in the parameters of a model affect prices and consumption, but they cannot forecast the specific values these variables will take. Consequently, "market stability is also a fiction of the mathematical abstractions used to represent it" (Ordeshook 1980, p. 448). For example, consider a market with one seller and two buyers, in which the value of the buyers' coalition is zero. The core, that is, the set of undominated imputations, includes only allocations that assign all value to the seller and none to the buyers. In this scenario, the seller is effectively a monopolist who extracts the entire surplus from exchange. However, it is equally plausible to assume that buyers might form a cartel and negotiate collectively, giving rise to a new equilibrium. The situation then becomes a bargaining problem in which the monopolist seeks to secure the most favorable distribution along the Pareto frontier. In this case, the core consists of all points on the Pareto frontier, demonstrating that the existence of equilibria alone is insufficient to generate predictions (Ordeshook 1980, p. 448).

Ordeshook's central claim, therefore, is that equilibria are features of formal models and may exhibit properties such as existence, uniqueness, or stability, but they do not necessarily confer predictive power. This argument does not resolve the problem of social choice impossibility but rather, it points toward new analytical directions. As he concluded, "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" (Ordeshook 1980, p. 450).

Examining the contrast between Riker's argument and Ordeshook's allows for a more precise appraisal of how Riker employed Game Theory and economic theory, as well as of how the early development of Positive Political Theory diverged from postwar neoclassical economics and from other attempts to extend economic reasoning beyond its original disciplinary domain.

As previously discussed, following the influential work of E. Roy Weintraub, historians of economics have interpreted the transformation of economics into a mathematical discipline as closely connected to the parallel development of mathematics as a formalist program (Weintraub 2002). Giocoli summarized the profound transformations in economics between the Thirties and the Fifties as a shift between two distinct visions of the discipline (Giocoli 2003b). The first conceives economics as a "system of forces," according to which the discipline studies processes generated by market and non-market forces, including, but not limited to, those that drive a system toward equilibrium. After more than a century operating within

this framework, economists increasingly replaced it with an alternative conception of economics as a "system of relations." This latter view defines economics as "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)

Within this distinction, the concept of equilibrium occupies a pivotal position. If economics is conceived as a "system of forces," equilibrium represents a state of an economic process, the outcome of interacting forces that generate it in an empirically meaningful way, while related issues such as perfect foresight, stability, and perfect information also require explicit consideration. By contrast, under the formalist "system of relations" approach, equilibrium is simply the necessary solution to a formally specified economic problem. In this framework, equilibrium either exists or does not; it never "emerges" or "arises" through a process.

Riker's formal analysis aligns more closely with the "system of forces" vision. For him, equilibrium does not function as an abstract analytical framework within which formal analysis unfolds, nor as the solution concept of a game-theoretic model. Rather, it denotes a relationship among forces, akin to equilibrium in partial-equilibrium economic analysis. This is evident, for example, in Riker's own definition from his 1962 work: "The notion of equilibrium is that of a relationship of forces arranged so that the deviation from some point of balance results in a (possibly automatic) correction back to balance" (Riker 1962b, p. 147). The previously discussed threefold classification of equilibria further illustrates this point.

This perspective also helps explain Riker's preoccupation with disequilibrium. Within a "system of forces" framework, equilibrium and disequilibrium are analytically symmetrical, and disequilibrium may even appear more empirically salient, given that social reality rarely exhibits anything resembling physical equilibrium. By contrast, in axiomatic models, and in the concept of equilibrium commonly employed in game theory, namely Nash Equilibrium, equilibrium is simply the solution to a formally defined problem. Consequently, the primary concern becomes the existence, non-existence, or multiplicity of equilibria.

The crucial point is that, in economics, Game Theory developed within the "system of relations" framework. This historical fact highlights how the introduction of Game Theory into political science differed fundamentally from its adoption within economics. It also clarifies the difficulty of the task Riker attempted to undertake, particularly in his theory of political coalitions. This difficulty stemmed not only from Riker's limited access to advanced game-theoretic techniques, which might have rendered his analysis more mathematically robust, but also from the fact that he approached game theory from a conceptual standpoint fundamentally at odds with its mathematical and economic development.

This conclusion can be further reinforced by recalling how Riker addressed the issue of rationality in his work on political coalitions (Riker 1962b, 17 et ss.). As noted earlier, Riker criticized the notion of rationality

adopted by economists since at least the late Thirties, namely the tautological idea that rationality could be divorced from substantive content through mathematical formalization.[7] Instead, Riker advanced a conception of rationality grounded in the elementary preference for winning over losing. His rejection of preference-ordering arguments can be interpreted as an effort to render political rationality intelligible to an audience uncomfortable with mathematical sophistication, but capable of grasping intuitive formulations of key concepts. Nevertheless, Riker's argument remains weak and poorly integrated into his broader discussion of modeling in the social sciences. He defended the rational choice assumption through what he called a "summation argument": even if not all actors are rational, the most important ones are. This move overlooked a crucial aspect of economic rationality, namely its function as a constraint on the beliefs and desires agents may hold, thereby rendering their actions theoretically intelligible (Reiss 2013, p. 31). In this sense, even when economic models appear descriptively plausible, the goal of modeling rational behavior still requires strong assumptions about preferences, beliefs, and their formal structure.

Riker's position can thus be interpreted as resting on an older conception of economic analysis as a "system of forces", a conception that, while appealing, was increasingly displaced within the very domain Riker had chosen as his theoretical reference, namely Game Theory, by an axiomatic "system of relations" approach. Notably, this was an area of economics that, until quite recently, remained particularly distant from empirical validation. This tension, in essence, constituted Riker's unsolvable dilemma.[8]

Positive Political Theory and "Economic Imperialism"

Above, it has been shown how Riker's use of economic theory differed from the trajectory postwar Economics was following. Nevertheless, it has also been argued that the case of "Positive Political Theory" was different, insofar as, from the Seventies onward, it clearly embraced an approach much closer to mathematical economics. A further step is to assess whether the development of formal political theory should be interpreted as an "act of conquest" by economics or rather as an independent intellectual event.

The extension of economic theory and analytical tools beyond the traditional domains of economics (namely markets, labor, and money) has conventionally been labeled "economic imperialism" (or "economics

[7] This is what game theorist and philosopher Ken Binmore later termed the "consistency view" of rational action (Binmore 2015).

[8] In a later methodological essay published in 1990, Riker revisited several of these issues. Most notably, he then linked rational choice and equilibrium to explanation rather than prediction. He argued that "the difference between prediction and explanation is that explanation requires much more convincing support" (Riker 1990, p. 167). While sociological laws may generate predictions, they cannot explain phenomena unless they are "placed inside a theory of equilibrium" (Riker 1990, p. 176). Nevertheless, Riker did not engage directly with broader philosophical debates concerning the nature of explanation in the social sciences or with the question of whether mathematical models are genuinely suited to this task.

imperialism"). The term "economic imperialism" is commonly attributed to the economist Kenneth Boulding, who allegedly used it to describe "an attempt on the part of economics to take over all the other social sciences" (quoted in Tullock 2004, p. 3). Gordon Tullock popularized the expression through a short essay published in the early Seventies. In that piece, he praised the economic approach while simultaneously advocating the blurring of disciplinary boundaries within the social sciences (Tullock 2004).

Sonja Amadae and Bruce Bueno de Mesquita challenged the "economic imperialism" thesis in the specific case of Positive Political Theory along three distinct lines (Amadae and Mesquita 1999). First, the thesis presupposes that Rational Choice Theory was fully articulated within economics and subsequently "colonized" other disciplines, including political science. This historical reconstruction, as previously shown, is inaccurate, particularly with respect to Game Theory.[9] Second, the assumption of "economic imperialism" shifts credit for innovation away from political scientists and toward economists. Once again, this does not constitute an accurate factual account. As discussed earlier, political scientists made significant contributions to formal theoretical developments, for example, in the literature on voting or in cooperative game-theoretic solution concepts such as the "Competitive Solution." Finally, Amadae and Bueno de Mesquita argued 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, 289–290).

It is important to note that Ordeshook articulated a view closely aligned with this latter argument. He contended that the development of "positive political theory," or "political economy," represented 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" (Ordeshook 1990, p. 10). Accordingly, the re-emergence of political economy amounted to the "reintegration into a refined paradigm of those features of reality that economists discarded in order to facilitate theorizing" (ibidem).

The arguments outlined above address the issue of "economic imperialism" primarily from a historical perspective. From this standpoint, they effectively demonstrate that the intellectual projects associated with Riker and with Positive Political Theory cannot simply be dismissed as manifestations of economics' imperialistic ambitions. Historically speaking, therefore, the situation is considerably more complex than is often assumed.

However, "Economic imperialism" has also been examined from a philo-

[9] As Amadae and Bueno de Mesquita rightly noted, "[...] 'rational choice,' denoting conscious decision making in a strategic environment with rational competitors, as originally articulated by von Neumann and Morgenstern (1944), became the status quo within political science before economists fully grasped its merits for their field" (Amadae and Mesquita 1999, p. 290).

sophical perspective. Approaching this issue from the standpoint of Philosophy of Science is equally interesting. While it is true that Riker and early political game theorists could not have been strongly influenced by economics, given that Game Theory still occupied a marginal position within the discipline during the Fifties and Sixties, it is equally true that Positive Political Theory and economics became increasingly intertwined, a closeness that persists to this day. From this perspective, "economic imperialism" appears to be at least twofold. First, it refers to economists applying their analytical tools to non-traditional domains such as politics, law, sociology, or crime. Second, it denotes the adoption of an economic-style formal approach in the construction of entirely new theories. The distinction becomes clear when one contrasts, for example, the spatial analysis of voting in political science with Gary Becker's well-known economic analysis of discrimination.

Becker was deeply influenced by Milton Friedman's positive methodology, which dominated the so-called Chicago School until the Seventies.[10] Becker employed Price Theory to assume that individuals possess a "taste for discrimination," such that proximity to members of other ethnic groups generates disutility. This preference for discrimination could thus be represented by a "discrimination coefficient" added to conventional capital and labor costs (Fleury 2012, 11 et ss. Lazear 2000). In doing so, Becker linked individual discrimination to market discrimination and proposed a market-based solution to the problem. If discrimination constitutes a cost for firms and individuals, market competition will tend to eliminate it by favoring the survival of the most efficient firms, those with lower costs. Crucially, Becker did not develop an entirely new theory of discrimination. Rather, he applied what he regarded as a powerful and general analytical tool, Price Theory, to generate new insights. The same logic applies to much of the Public Choice literature, as well as to Mancur Olson's classic analysis of collective action (Olson 1965; Ordeshook 1990).

Consider the Spatial Theory of Voting instead. While its development would indeed have been impossible without adopting the mathematical approach characteristic of economic theory, it required far more than a passive borrowing of analytical tools. Although Downs's most famous result appears relatively intuitive, despite its intellectual debt to the less obvious findings of Hotelling, Duncan Black's analysis, though not exceptionally sophisticated mathematically, demanded a precise formalization of distinctly political concepts, such as the assumption of single-peaked preferences. Matters became considerably more complex when these results were extended to n-dimensional spaces, as in the highly mathematical works of McKelvey, Schofield, Ordeshook, Davis, Hinich, and others briefly addressed earlier. These scholars did not merely apply existing tools to

[10] On Friedman's methodology, see Friedman 1954 and Caldwell 1994. Becker addressed discrimination in his Ph.D. dissertation, later published in 1957 as *The Economics of Discrimination*. He was among the most prominent price theorists of the Chicago School, alongside Friedman and George Stigler, and was awarded the Nobel Prize in Economics in 1992 (Spencer and Macpherson 2014, 189–209). For a historical and critical assessment of the Chicago School, see Horn, Mirowski, and Stapleford 2011.

clarify a phenomenon. Rather, they constructed entirely new theories that would have been impossible without adopting the same highly mathematical reasoning prevalent in economics, while simultaneously inventing formal counterparts to inherently political concepts related to voting.

To fully appreciate this distinction, it is useful to consider how preferences are represented in microeconomics. First, a commodity space, typically a Euclidean coordinate system, $\mathbb{R}^n_{++}$, is assumed.[11] Each point in this space represents a bundle of goods, and a utility function encapsulates individual choice. Indifference curves and preference sets represent agents' rankings over these bundles. Each economic agent faces a maximization problem, which mathematically consists in identifying the quantities and prices that maximize her utility function. Graphically, this corresponds to the indifference curve tangent to the budget constraint. This so-called "consumer problem" is an optimization problem whose extension to large numbers of goods and agents yields the framework of General Equilibrium. Economic theory subsequently investigates the conditions under which such equilibria exist and how they change when properties of utility functions, budget constraints, or traded goods are altered.

Standard consumer choice theory typically assumes that both budget sets and indifference curves are convex. These assumptions capture two intuitive ideas: that a mixture of goods is preferable to consuming only one good, and that, at constant prices, larger bundles are always preferred to smaller ones (for example, four apples and four pears are preferred to two apples and two pears). A particular case involves satiable preferences, for which the latter condition holds only up to a certain point. This feature is also central to formal political analysis, since in spatial voting models each rational voter's utility is defined as the distance between her ideal point and the proposed alternative.

Microeconomics thus analyzes decentralized decisions concerning the production and consumption of goods and services. Similarly, Spatial Voting Theory concerns the collective selection of a point within a feasible space under a given decision rule, typically majority voting. This parallel, however, does not imply that the latter is merely an offshoot of the former. The following assessment by Ordeshook is therefore worth quoting at length:

> "The use of single-peaked preferences, or the more general conceptualization of convex preference sets with internal satiation 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*

[11] $\mathbb{R}^n_{++}$ denotes the strictly positive orthant of Euclidean space. This reflects the intuitive assumption that traded commodities exist and therefore have positive quantities, although this assumption is not universally applicable.

> *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." (Ordeshook 1990, p. 20, italics added).

The concept of disciplinary imperialism has, of course, been examined more systematically by philosophers of science. While Ordeshook's account may already appear sufficiently explanatory, it is nonetheless useful to relate the foregoing distinction to the taxonomy proposed by the Finnish philosopher of science Uskali Mäki (Mäki 2009). Mäki distinguishes between "expansionism" and "imperialism." The former refers to the expansion of a theory's explanatory scope, that is, the range of phenomena it can explain. "Imperialism," by contrast, is a subset of expansionism. "Economic imperialism" is thus defined as "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). According to Mäki, the key difference rests on "*historical and social contingency*" (Mäki 2009, p. 361, italics in the original). In some cases, established disciplines already address the phenomena later subsumed by the expanding discipline. In others, they do not. The very notion of imperialism presupposes boundaries, and economic imperialism consists precisely in crossing disciplinary boundaries. From this perspective, the distinction is pragmatic, defined in terms of (the existence or absence of) established disciplinary practices and of the relations between the practices of the "conquering" and "conquered" fields.

The primary motivation behind scientific "imperialism" lies in the aspiration to provide unified explanations of phenomena. While Mäki accepts unification as a "virtuous achievement of scientific theorizing," he nevertheless proposes several constraints on its legitimacy. For present purposes, the most relevant is the "ontological constraint," which he further divides into "derivational unification" and "ontological unification." In brief, derivational unification concerns the ability to explain diverse phenomena using a parsimonious set of theories, whereas ontological unification "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)

These notions are not easily operationalized, as few theories fit neatly into one category or the other, and the boundary between them remains somewhat blurred. Nevertheless, one may interpret derivational unification as the attempt to construct an "explain-everything" theory, as in simplified versions of rational choice theory or even price theory. Under the assumption that individuals always behave as maximizers, theorists can generate wide-ranging explanations of diverse social phenomena. Such explanations do not necessarily refute alternative theories, but they may appear especially appealing due to their reliance on clear and circumscribed

assumptions. Becker's analysis of discrimination provides a paradigmatic example. By contrast, extending Game Theory and related microeconomic approaches to domains such as voting or rational-choice models of political institutions entails a stronger ontological commitment: it assumes that unity across social phenomena is possible and that the task of theory is precisely to render this unity formally explicit (see Mäki 2009, 364 et ss.). Mäki associates this position with a realist philosophy of science. From this standpoint, "economic imperialism" is objectionable only in cases where derivational unification occurs without corresponding ontological unification. Accordingly, Mäki does not reject economic imperialism on principle, provided 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).[12]

The distinction between Price Theory and microeconomics further helps to clarify this issue. These two approaches are often treated as interchangeable, since both rely on rational choice assumptions and supply-and-demand analysis. Nevertheless, important differences exist, as clearly articulated in the most recent edition of the now-classic *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 places greater emphasis on rationality and optimizing agents. Both price theory and game theory model behavior in terms of equilibrium, but the latter typically focuses on interactions among small numbers of agents and seeks to generate separate predictions for each of them, treating the rest of the market as given. [...] With its emphasis on competitive market equilibrium, basic price theory is concerned not with bid prices but with final transaction prices, aggregate quantities produced and sold, and their relationship to costs of various kinds, as well as with the position of goods within the consumer demand system." (Jaffe et al. 2019, 2–3)

If this reconstruction is correct, then Positive Political Theory, while distinct from Price Theory, yet closer to and partly subsumed within microeconomics, would not constitute, in Mäki's terminology, an unjustified instance of economic imperialism. This interpretation is further supported

[12] Mäki refers to this situation as "Economics Imperialism*."

by the fact that scholars such as Ordeshook or McKelvey, by applying Game Theory and other formal tools to voting, political coalitions, and institutions, did not invade an established disciplinary territory. Rather, they produced an entirely new class of analyses and results that would have been unattainable otherwise. At the same time, however, as argued in the previous section, Riker's strong faith in the predictive power of an "economics-like" approach, particularly his identification of equilibrium with meaningful prediction, can plausibly be interpreted as a form of unjustified economic imperialism, at least within Mäki's analytical framework.

A Ideological Conservative Bias?

A final point deserves further exploration: the possible existence of a "conservative and free-market bias" in Positive Political Theory. In several works (see for example Mirowski 2002; Amadae 2003; Erickson et al. 2015), the postwar development of mathematical Economics, Game Theory, and Rational Choice Theory is interpreted as deeply intertwined with Cold War political imperatives and, in particular, with the defense of a free-market economy against the perceived threats of socialism and Marxism. This line of argument parallels, and at times overlaps with, the so-called "economic imperialism" thesis.

Above, it has been argued that it is historically misleading to interpret the entry of Game Theory into Political Science as a form of economic imperialism. From the standpoint of philosophy of science, however, the issue is more complex. From that perspective, one might still argue that Riker's intellectual enterprise exemplified an unjustified mode of economic imperialism, while the same conclusion does not necessarily follow in the case of "positive political theory."

Yet this observation does not exhaust the question of what sustained Riker's long-term commitment to formal analysis. Despite the differences emphasized above, Riker's commitment to formal theory and to the Theory of Games followed, in certain respects, a trajectory similar to that of mathematical economists in the Fifties. Indeed, practical ambitions underpinned the development of General Economic Equilibrium models, Econometrics, and Linear Programming: advanced formal analysis could serve applied objectives (objectives that were not necessarily identical with, or reducible to, a radical free-market program).[13] Similarly, Riker's main goal was to advance the scientific understanding of politics, namely to articulate true propositions about political phenomena. He regarded game theory and economic theory as rigorous and valuable analytical instruments and therefore employed them. In the Fifties, his agenda was not primarily political, but methodological.

[13] Consider, for example, the Cowles Commission at Chicago in the Fifties. Its members were largely European and often held left-wing political preferences. This ideological orientation frequently placed Cowles researchers in tension with the Department of Economics that hosted the Commission, where enthusiasm for mathematical economics was, if anything, weaker than among Cowles affiliates. Hibbard n.d.

In the Seventies, however, Riker's political ideology evolved toward the political and philosophical outlook associated with figures such as Milton Friedman, George Stigler, and Gary Becker. He thus adopted a firm free-market orientation.[14] Moreover, as discussed above, from the late Seventies onward Riker explicitly argued that the inevitability of majority cycles and the vacuity of concepts such as the people's will supported the superiority of liberal democracy (understood through social choice) over populist democracy (Riker 1982). These conclusions, grounded as they were in the mathematical analysis of Social Choice, were already implicit in foundational works such as those of Arrow in the early Fifties. It remains open to debate, however, whether the primary force sustaining this intellectual development was the defense of liberal democracy against more radical alternatives (or, analogously, the protection of market systems against collectivist planning).

A potentially pivotal factor in Riker's turn toward libertarian positions may have been the presence at Rochester, although for a relatively brief period, of the prominent legal scholar Henry G. Manne. Trained at the University of Chicago Law School, Manne was a central figure in the institutionalization of Law and Economics as a recognized field of research. He was also a committed libertarian, shaped by his Chicago experience and by his engagement with the Austrian economist Ludwig von Mises and the UCLA economist Armen Alchian (Gindis 2020; Manne and Stocker 2012).[15] Although he was not an economist, Manne took economic theory with great seriousness, convinced that legal phenomena could not be understood without analyzing the market forces shaping actors' incentives. Moreover, his interest was not confined to theory: he was actively engaged in the economic training of lawyers, judges, and law professors.

Manne arrived at Rochester in 1968, hired by W. Allen Wallis, himself a former University of Chicago faculty member, closely associated with Friedman and Stigler, and a member of the Mont Pèlerin Society. was tasked with establishing a law school and, in the interim, temporarily joined the Political Science department. His involvement in departmental activities, however, appears to have been minimal (Bueno de Mesquita 2021). The plan to create a law school eventually dissolved, and Manne moved to Miami. Yet it may not be coincidental that in the Seventies Riker began collaborating more intensively with the Liberty Fund and increasingly

[14] In his interview with Shepsle, he stated that "I have gone 180 degrees in what I think the appropriate reformist position is" (Riker and Shepsle 1979, p. 145).

[15] Aaron Director played a crucial role in establishing "Law and Economics" at Chicago, especially through the introductory Price Theory course he taught at Chicago Law School in the late Forties. From that point, the transition from having an economist teaching in a law school to building an entire research field proceeded relatively quickly. During the late Forties and Fifties, Director became increasingly involved in joint teaching and research with legal scholars, applying price theory to traditional legal and public policy issues such as antitrust (Medema 2009). Armen Alchian was particularly well known for his economic theory of property rights (Alchian 1965; Alchian and Allen 2018). Ludwig von Mises was a committed classical liberal whose conception of economics, named "praxeology", challenged both socialism and neoclassical economics (Caplan 1999).

participated in conferences and initiatives often organized by the "Law and Economics Center," founded by Manne at the University of Miami.[16]

In his interview with Shepsle, Riker returned explicitly to the issue of an "inherent conservative bias" in Positive Political Theory (Riker and Shepsle 1979, 143 et ss.). He then offered an ideal-typical account of why individuals are drawn to social science and why, among the various areas of political science, formal political theory might be more inclined to exhibit conservative dispositions. "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 Shepsle 1979, p. 144) These remarks fit closely with Riker's epistemological position as reconstructed above, namely, his progressive view of science and the theoretical posture he sought when he first adopted Game Theory. 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 Shepsle 1979, p. 145) He thus forcefully rejected the claim of an inherent conservative bias, bracketing his own political commitments.

One of the main reasons this allegation is so frequently directed at formal approaches in political science may be the association of Public Choice, especially Buchanan's version of it, with a broadly classical liberal normative orientation. Above, it has been shown how Riker's efforts to institutionalize Positive Political Theory in the Sixties and thereafter often overlapped with Public Choice. Yet there were also meaningful differences between the two approaches. One of the most significant is precisely that Public Choice was more explicitly politically committed than Positive Political Theory. As it is evident in Buchanan's and Tullock's early work, prior to their joint 1962 book (and as reconstructed masterfully in historical studies such as Levy and Peart 2020), they, together with figures such as Warren Nutter and Rutledge Vining, were motivated by mounting concerns in the Fifties about the appropriate role of social science and government. Consequently, in the eyes of many observers, Public Choice came to represent a libertarian posture.[17]

It is true that rational-choice approaches to political institutions may, at least in part, support analogous aspirations. Yet, as the broader history of economics itself demonstrates, this is insufficient to sustain the claim that a discipline grounded in the mathematical representation of individual

[16] A detailed list of these activities is provided in Gindis 2020. The "Liberty Fund" is an American think tank founded in 1960 by businessman Pierre F. Goodrich. It focuses on disseminating conservative and libertarian ideas, particularly through educational initiatives.

[17] It is nevertheless simplistic and imprecise to suggest that Public Choice is straightforwardly ideologically biased. More generally, it can be understood as a "set of theories" concerning governmental failure (Buchanan 2003), but this does not imply that every Public Choice scholar aims to minimize the size of the state as much as possible.

preferences and choices is inherently politically conservative.

Concluding remarks

Three years before his death in 1993, Riker participated in a conference organized by E. Roy Weintraub at Duke University, the first meeting explicitly devoted to the history of Game Theory. There, Riker presented a paper on the entry of the theory of games into Political Science, an intervention that was also partly autobiographical, given his own role in that process (Weintraub 1992; Riker 1992). Despite his later turn toward the study of institutions and "heresthetics," the research program he had built from the Sixties onward had flourished, becoming one of the most distinctive and intellectually vibrant subfields of political science in the Eighties and Nineties.

When Riker began his theoretical enterprise, American Political Science was entering a period of ferment and disciplinary transformation. In his view, economics offered a viable model for creating a "genuine science of politics." Ultimately, however, political science did not achieve methodological unity in the way Mathematical Economics did. On the contrary, by the late Eighties, Stanford professor Gabriel Almond famously spoke of "separate tables" in Political Science, and Cornell professor Theodor J. Lowi offered a similar diagnosis in 1992 (Almond 1988; Lowi 1992). Introducing a double cleavage to conceptualize the state of the discipline, one methodological (soft/hard) and one ideological (left/right), Almond painted the following, rather bleak, portrait of the 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." (Almond 1988, 827–828)

Many things have changed again in political science over the three decades since those words were written. Yet Almond's assessment, together with subsequent developments, suggests that Riker's expectation that his approach might become the "main hope for a genuine science of politics" has not been realized. More importantly, this outcome was not determined primarily by theoretical refutation, but rather by what might be called "empirical" developments, at least insofar as the number of articles employing game theory in the leading political science journal appears to have peaked

in the Nineties, after which enthusiasm for formal political theory noticeably waned (Lohmann 2008).

In a widely debated book published in 1994, two political scientists, Donald P. Green and Ian Shapiro, questioned how rational choice theories conformed to the empirical phenomena they purported to explain (Green and Shapiro 1994). Famously, they argued that one of the most troubling features of rational choice scholarship was its being "method-driven" rather than "theory-driven." In other words, the main advances in the approach resulted more from the progressive refinement of technical, namely, mathematical, tools than from the emergence of substantive real-world puzzles. Their critique rested on the premise that what is mathematically interesting is not necessarily what is most consequential for explaining actual political phenomena.

Nevertheless, even before Green and Shapiro's intervention, the challenge of incorporating imperfect knowledge and error into game-theoretic models, so as to render them more amenable to empirical validation—had become, in some circles, a central concern among formal theorists. For McKelvey, the core problem was how to relate Nash Equilibria to experimental findings obtained in laboratory settings. The outcome was what has been described as a "statistical facelift to traditional non-cooperative game theory," known as "Quantal Response Equilibrium" (Palfrey 2005, p. 16; McKelvey and Palfrey 1995). McKelvey and his coauthors were not directly responding to Green and Shapiro. Rather, their efforts emerged from longstanding, and still unresolved, problems in Game Theory, such as whether Nash Equilibrium strategies are in fact played in simple experimental environments. Quantal Response Equilibrium represented a highly technical attempt to address these difficulties. It has since been employed across Political Science, Economics, and Econometrics, and it sits precisely at the intersection of these disciplines.

After Green and Shapiro's book, other formal theorists called for greater empirical relevance, though without necessarily embracing the full force of their critique of rational choice in political science. In many respects, these responses followed the trail Riker had already opened with his rational-choice institutionalist program in the Eighties. Yet important differences remained. For instance, Ordeshook emphasized an "engineering" approach to institutions, namely, the use of Game Theory and formal analysis not to predict outcomes, but to support institutional design (Ordeshook 1996). This move aimed to reframe the longstanding positive-versus-normative debate: normativism would be strengthened by game-theoretic tools while being constrained in scope. The question was no longer what the best polity or institution would be in the abstract, but how existing institutions might be improved. This orientation was not far from what Baron and Ferejohn achieved when they related "more universalistic" outcomes to "open rule committees." Their model implies that an open rule committee increases each member's bargaining power and may therefore reduce internal conflict. In a different vein, but with a comparable objective, Norman Schofield called for a "theory of rationality based on both preference and belief," which he labeled the "Condorcetian Research Program," explicitly intended

to overcome the limitations of a purely preference-based approach (Schofield 1996).

To conclude, it would be difficult to claim that, three decades after Riker's death and six decades after his work on political coalitions, Game Theory, and, more broadly, formal approaches, occupy a central place in contemporary political science. Nonetheless, Positive Political Theory remains a well-defined and established subfield, even if its boundaries with economic theory appear destined to blur further.[18]

Yet the purpose this research was not to adjudicate the current standing of formal political science within the discipline, but rather to reconstruct and discuss, both from an internalist and an externalist standpoint, how political scientists began to use Game Theory. This focused on three main dimensions: first, Riker's role in this process as a formal theorist; second, his role as an "intellectual entrepreneur," or "charismatic leader," of an intellectual community; and third, a neglected chapter in the history of Game Theory, namely the cross-fertilization between economics and politics.

Beginning with the latter point. Cross-fertilization is inherently two-sided. When economic analysis is applied beyond its traditional domains, the process is often framed as an "act of conquest." In such contexts, non-economist social scientists frequently adopt a twofold posture: they either resist the perceived invasion or become background figures, sometimes dismissing economic theory with irony. It has been argued instead that the distinctive feature of Riker's approach lies in the seriousness with which he treated Game Theory. He advanced an entire research program that, in his view, coincided with the scientific maturation of political science as a whole. Particularly in the Seventies, political Game Theory developed in parallel with, and at times with greater energy than, game-theoretic research in economics, especially regarding specific topics and approaches such as cooperative games. Eventually, Positive Political Theory joined the broader game-theoretic revolution in much the same way economics did: cooperative models were marginalized, and non-cooperative theory became dominant. Yet the path toward this outcome began earlier than in economics, and Riker's theory of political coalitions constituted a cornerstone of that trajectory.

It has also been argued that Riker lacked sufficient mastery of Game Theory to realize his extraordinarily ambitious theoretical goals. This concerns Riker as a formal theorist. His use of game theory was often defective and imprecise, and several explanations for this were advanced. One important explanation concerns biographical contingencies. Riker obtained an undergraduate major in Economics during the late Thirties and early Forties, when such training differed radically from today's curricula,

[18] As noted earlier, Ordeshook argued that the re-emergence of political economy represented the reintegration into economics of those features of reality that economists had previously discarded in order to facilitate theorizing (Ordeshook 1990, p. 10). Moreover, Riker wrote that "the main practical benefit 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). A principal advanced reference is Persson and Tabellini 2002.

requiring little if any mathematical or statistical preparation. The same is true of his graduate training in political science. As a result, his early analyses were frequently under-formalized and rarely attracted the attention of mathematically oriented game theorists.

This explanation, however, is still insufficient. A fuller account must complement it by appealing to epistemological assumptions, particularly the contrast between alternative conceptions of formalism in economics. As argued, Riker did not fully recognize the innovative character of mathematical economics and Game Theory relative to interwar economics. Moreover, he embraced a "hyper-positivist" philosophy of science, within which there appeared to be little room for the notion that a model could function as a "caricature" (Gibbard and Varian 1978) or an "idealization" (Reiss 2013) of reality, and within which rationality was treated as a substantive theory rather than as a purely formal one, as it increasingly became in postwar economics, especially in its most mathematically stringent versions.

Riker thus sought robust predictive power in Game Theory as the foundation for a genuinely empirical and fully scientific political science. Moreover, he looked for such predictive leverage in the segment of Game Theory, von Neumann and Morgenstern's cooperative games, that was least able, mathematically speaking, to generate predictions, insofar as it relied on solution concepts that often contain infinitely many elements or, in some cases, no solutions at all. Unfortunately, as a vast literature demonstrates, it remains difficult to specify what the predictive power of Game Theory actually is. Philosophers of science and economists have examined at length questions such as what a model is and what the explanatory power of a model consists in, yet no unified position has emerged. Nonetheless, a broadly shared view among economists and formal social theorists is that game-theoretic models, and formal approaches such as Rational Choice Theory more generally, do not produce genuine predictions, at least not in any straightforward sense of the term "prediction."

Moreover, like other formal approaches, game theory exhibits a distinctive feature. Martin Shubik, who worked extensively in the field, proposed a threefold division between "high church" Game Theory, "low-church" Game Theory, and "conversational Game Theory" (Shubik 2002). The first category includes highly mathematical work introducing genuinely new concepts or results (for example, contributions associated with figures such as Nash, Shapley, and Aumann). Much of economics' use of Game Theory belongs to the second category. Finally, as the label suggests, "conversational Game Theory" encompasses more informal and widespread reasoning, namely, adopting a game-theoretic mindset in practical problems without relying on formal modeling. The intriguing feature of this taxonomy is that, insofar as the notion of predictive power applies at all, it arguably fits "conversational" Game Theory better than the other two. Identifying situations that can be addressed with a game-theoretic mindset is often straightforward. Yet in such cases, proposing alternative explanations is just as easy, since the boundaries of what is being explained remain vague or poorly specified. By contrast, constructing a rigorous model in the first two senses allows for a more precise characterization of the problem, but it typically does so at

the cost of any realistic prospect of prediction.

It can be argued that the most convincing, though not the only, way to live with this puzzle (that is, to live with it rather than solve it) is provided by Ariel Rubinstein. For Rubinstein, Game Theory is primarily the study of the considerations that inform decision-making in interactive settings, with no necessary normative implications and limited empirical content (Rubinstein 2007). As he writes, "[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 this broad sense, the Theory of Games is, above all, a language, much like mathematics.

Integrating this language into the analytical repertoire of political scientists constitutes the true landmark contribution of William H. Riker, even though he did not succeed in his more ambitious project of using Game Theory to transform political science into a genuinely empirical science. Yet this conclusion should not be taken to delegitimize the theoretical efforts of Riker (and others like him) to adopt Game Theory within their respective disciplines. Indeed, the questions that initially puzzled him and later sustained his commitment to formal analysis lie at the heart of any attempt to search for a science of politics.

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This book contributes to the history of game theory by examining how the Theory of Games entered political science. This transition was largely driven by the work of the American political scientist William H. Riker, who, although trained in a traditional approach to the discipline, embraced game theory and formal political analysis in the late 1950s. His efforts were central to the emergence of Positive Political Theory. The book refines the conventional narrative on game theory's role in political science, showing its adoption as an autonomous intellectual trajectory rather than a mere case of economic imperialism. Methodologically, it offers a historical reconstruction of Riker's early works, especially *The Theory of Political Coalitions* (1962), combining critical analysis with extensive archival research and interviews to trace the intellectual and institutional forces shaping this transformation.

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