

# Boudon Reexamined

## Nuts and Bolts for Contemporary Sociological Science

Gianluca Manzo (Ed.)



*Boudon Reexamined* presents a selection of short essays by leading scholars from several generations who critically engage and enter into dialogue with the work of Raymond Boudon. Each chapter focuses on a specific topic from his extensive writings. Readers will follow this intellectual trajectory through analyses of early correspondence with Lazarsfeld and Merton, his typology of sociological styles, and his contributions to contemporary analytical sociology, including the notion of middle-range theory. In addition to already well-discussed aspects of Boudon's work, namely his understanding of methodological individualism and the theory of ordinary rationality, the book also explores less frequently discussed topics, including his early interest in formal modeling in sociology and his understanding of the link between interdependence structures and social change. Included in the following pages are new assessments of Boudon's well-known analyses of the inequality of educational opportunity and intergenerational social mobility, as well as his lesser-known substantive contributions to the study of relative deprivation and his early dialogue with game theory. The book also outlines Boudon's study of classical authors, especially Tocqueville, before two final chapters conclude by examining how Boudon's works can be used to teach sociology at the undergraduate and master's levels. Our hope is that *Boudon Reexamined* provides readers with a fresh assessment of his legacy – how his work can be applied to conduct theoretical and empirical research in contemporary sociology, as well as to promote high-quality scientific standards for new generations.

**Gianluca Manzo** is Professor of Sociology at Sorbonne University and a Fellow of the European Academy of Sociology. His research applies computational models and social network analysis to the study of social stratification and diffusion dynamics. He is the author of *La Spirale des inégalités* (PUPS, 2009) and of *Agent-based Models and Causal Inference* (Wiley, 2022). He also edited *Analytical Sociology: Actions and Networks* (Wiley, 2014) and the *Research Handbook on Analytical Sociology* (Edward Elgar, 2021). More information is available on his webpage: [www.gemass.fr/member/manzo-gianluca/](http://www.gemass.fr/member/manzo-gianluca/).



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# **Boudon Reexamined**

**Full book**

**Gianluca Manzo (Ed.)**

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L'intelligence  
du social

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The great books of the sociological tradition are either works of theory and epistemology or empirical studies structured by a profound theoretical or epistemological reflection. Émile Durkheim's first three books, *The Division of Labour in Society*, *The Rules of Sociological Method*, and *Suicide*, each fall into one of these three categories. This heritage represents an impressive growing legacy of authors and works that foster an understanding of social life through the formation of new concepts, models, and interpretations, thereby providing a pathway to deciphering the thickness and chaotic nature of human societies.

Gianluca Manzo (Ed.)

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## FOREWORD

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It seems fair to say that Raymond Boudon (1934-2013) is among the most influential French sociologists of the second half of the twentieth century.

Although imperfect, citations are one possible indicator of this fact. A study of the reception of 188 French sociologists between 1970 and 2012 in the United States has shown that Boudon is part of the very small minority of French authors whose work has been cited at least once in the most selective American sociological journals. He is even part of the top fraction of this minority that includes the nine French sociologists who have collected more than 100 citations in those journals (in particular, in a decreasing order of citations received, Boudon ranks below Durkheim, Bourdieu, Latour, Tocqueville, and Callon but above Crozier, Touraine and Mauss) (Ollion and Abbott 2016, Table 3, p. 342). A more extensive study considering citations received by 346 sociologists of any national affiliation between 1970 and 2010 in top-ranked American and European journals in sociology but also in a selection of sociological textbooks, handbooks, and encyclopedias found that Boudon was, in 2010, part of the worldwide sociology's "prestige elite" constituted of the 50 most cited authors. He was the fifth, behind Bourdieu, Durkheim, Foucault and Latour, if one considers only the French authors appearing within this "elite" (Korom 2020, Figure 1, p. 138). The fact that 83 sociologists, philosophers, economists, and political scientists from across the world agreed to contribute to *Raymond Boudon: A Life in Sociology* (see Cherkaoui and Hamilton 2009) can be seen as a more qualitative indicator of the academic visibility that Raymond Boudon was able to secure during his academic career.

Quantitative data and content analyses presented in these three pieces of work suggest that Raymond Boudon's worldwide visibility arose from the fact that his scientific production was multifaceted, being at the same time theoretical, substantive, and methodological. On the theoretical level, Boudon progressively elaborated a theory of rational action whose goal was

to go beyond the theory of expected utility inspired by microeconomics; on the substantive level, he applied this conception of the actor to propose clear explanations of macroscopic phenomena in a number of research fields, such as the sociology of education, the sociology of social mobility, the sociology of values and beliefs, and the sociology of social change. From a methodological point of view, he was one of the pioneers in sociology, from the 1970s onwards, of the perspective based on generative models and the use of mathematical methods and numerical simulations to study these models (see Hedström and Manzo 2017). Boudon's constant dialogue with the classics of the discipline – Tocqueville, Durkheim and Weber, in particular, but also Simmel, Pareto and Tarde – enabled him to contribute to the history of sociological thought, too.

8 But Boudon's impact can also be approached from the point of view of his activities as an academic entrepreneur. He indeed constantly operated on an institutional level with the goal of designing intellectual places where his understanding of sociology could have a chance to incubate and diffuse. In 1971, for instance, he created, and directed until 1998, the Groupe d'Étude des Méthodes de l'Analyse Sociologique (GEMAS; renamed "de la Sorbonne", thus GEMASS, in 2009), a research unit of the French National Center for Scientific Research (CNRS), associated with Sorbonne University, which still is one of the central research units in French sociology. At the international level, he contributed to founding the European Academy of Sociology in 2000, of which he was the first president (see Lindenberg 2002); since 2016, the Academy awards the Raymond Boudon Award for Early Career Achievement to recognize excellence in the work of young researchers affiliated to European academic institutions. His election as fellow of the American Academy of Arts and Sciences (1977), Academia Europea (1988), the French Academy of Moral and Political Sciences (1990), the British Academy (1997) or the Royal Society of Canada (2001) are also signs of the lasting traces that Boudon succeeded to leave in major institutions of the discipline in France and abroad. The creation in 2012 of the "Raymond Boudon Collection", composed of 81 boxes, occupying 27 linear meters, now stored in the French National Archives, a service of the French Ministry of Culture ensuring the conservation of documents regarded as of national relevance, can be seen as the latest tangible sign of the recognition Boudon received during his life (Zerilli 2020).

In line with this achievement, Boudon's passing on the 10 April 2013 was widely covered by *In memoriam* that quickly multiplied in academic journals of various countries (including the *Revue Française de Sociologie*, *L'Année Sociologique*, *Sociologie*, *Sociologie du Travail*, *Revue Européenne des Sciences Sociales*, *Revista Española de Sociología*, *Revista internacional de sociología*, *The*

*Tocqueville Review*), in major French national newspapers (like *Le Monde*, *Le Figaro* or *Libération*), media (like *France Culture*) or popular social magazines (for instance, *Sciences Humaines*) as well as in professional association's newsletters (including American Sociological Association).<sup>1</sup> A series of journal articles in French (see the issues 56-2 and 57-1 of *Revue Européenne des Sciences Sociales* between 2018 and 2019), a monograph in French (Leroux 2022) and an edited book in English (Robitaille and Leroux 2024) were also published by some of Boudon's closest and longstanding French-speaking interlocutors investigating a selection of Boudon's oeuvre's aspects.

At more than ten years after Boudon's decease, however, a systematic assessment of his scientific legacy is still missing. A first attempt was made by Joël Berger and Andreas Diekmann who organized on 29 and 30 May 2014 an international conference (meaningfully titled "Conference on the Legacy of Raymond Boudon") at ETH Zürich. Unfortunately, the event did not materialize into a publication. In the fall of 2022, GEMASS decided to pursue the German project by orchestrating a new International Symposium with the goal of providing a systematic review of all aspects of Boudon's oeuvre. Under the title of "Engaging with Boudon: Insights for Contemporary Sociological Science", the symposium finally brought to Sorbonne University in June 2024 scholars from different generations; each of them was invited to engage a conversation with a specific set of Boudon's pieces of writing in order to assess the importance of the chosen dimensions for theoretical and empirical research in contemporary sociology as well as for teaching sociology to the new generations. The book that the readers have in their hands results from a selection of papers initially drafted for that conference<sup>2</sup>.

These are organized into six parts. Part I ("Scientific Path and Style") begins with a chapter by Pierre-Michel Menger that provides an overview of how Boudon's scientific trajectory evolved from his early works on educational inequality and social mobility, through the elaboration of a general theory of rationality, until his late analyses of false beliefs and moral values. Then, Michel Dubois and Sylvie Mesure zoom in on various points of this trajectory by exploiting a portion of Boudon's correspondence, namely that which concerns his regular intellectual exchanges with Paul Lazarsfeld and

1 A selection of these pieces can be found on the GEMASS website: GEMASS, "Raymond Boudon – Member Profile", <https://www.gemass.fr/member/boudon-raymond>, accessed on June 29, 2025.

2 More details on the event can be found here: GEMASS, "27-29 June 2024, Engaging with Boudon: Insights for Contemporary Sociological Science", <https://www.gemass.fr/activity/engaging-with-boudon-insights-for-contemporary-sociological-science/?lang=en>, accessed on June 29, 2025.

Robert Merton, and show how this “intellectual friendship” was key in the construction of the distinctive “scientific ethos” characterizing Boudon’s entire work. Finally, Filippo Barbera focuses on Boudon’s articles where this “ethos” was programmatically elaborated and discusses the extent to which Boudon’s “style” is compatible with the project of a “public” sociology.

Part II (“Thinking by Social Mechanisms”) deepens the analysis of Boudon’s general perspective on how sociological inquiry should be framed. In particular, Peter Hedström focuses on Boudon’s pieces of work where he elaborated on the distinction between describing and explaining, and he forged the strategy of explaining by detailing the mechanisms behind the emergence of a given social regularity. Hartmut Esser reconstructs Boudon’s contribution to the development of the notion of “middle-range theory”, the distinctive type of theorizing within which mechanisms-based explanations are typically still designed within contemporary analytical sociology. Lucas Sage brings the discussion to the methodological level and show how Boudon actually implemented slightly different variants of formal models when, in three different early works, he had to study hypotheses about the mechanisms behind specific social phenomena, in particular how judges decide to drop a case (actually, an important but rarely considered piece of Boudon’s work), how family decides to invest in education, and how individuals decide to participate or not to social lotteries.

Part III (“Sociology of Social Stratification”) zooms in on the second of these substantive works, namely *L’Inégalité des chances*. Given the complexity of this book, three different specialists of the quantitative analysis of social inequalities dissect it, separately dealing with its first part on educational inequality (Richard Breen’s chapter VII), the second part on intergenerational social mobility (Gunn Birkelund’s chapter VIII), and the relationship between the two (Louis-André Vallet’s chapter IX). While they agree that *L’Inégalité des chances* is “a by now landmark book on the inequality of educational attainment and social status”, in Birkelund’s words, they also provide a balanced assessment of what it can now be regarded as wrong or outdated, given the most advanced methods and recent data in the field.

Part IV (“Relative Deprivation, Game Theory, and Social Interdependency”) shifts the focus to Boudon’s less well-known substantive analysis of relative deprivation (i.e. the third case analyzed from a methodological point of view in Sage’s chapter VI). In particular, Werner Raub explains how Boudon’s model illustrates the heuristic power of simple game theory to deal with the problem of the transition from the micro- to the macro-level of analysis, in particular when actors are embedded in complex structures of interdependencies that make it difficult to anticipate the consequences of one’s choices. Joël Berger,

Andreas Diekmann and Stefan Wehrli provide an overview of the studies that have elaborated Boudon's model of relative deprivation through various formal tools, and emphasize how the model gave rise to a lively field of experimental research systematically testing both the basic version of the model and more advanced versions of it. Inspired by Boudon's strategy of using simple game theory to design mechanism-based explanations, Jörg Stolz proposes to apply this strategy to formulate a possible explanation of ethnographic observations concerning the emergence of false beliefs ("ideologies", in Boudon's terminology) and rituals within small groups of individuals experiencing repeated interactions.

Part V ("Methodological Individualism and Rationality") goes back to Boudon's meta-theoretical perspective to deal with its most general backbone, i.e. methodological individualism, as well as with rationality, the topic to which Boudon devoted more and more singular attention during the last three decades or so of his career (Boudon 1989) being a possible starting point of this stage of his intellectual path). In particular, chapter XIII by Nathalie Bulle focuses on methodological individualism, systematically reconstructs how Boudon defined this notion over the years, and identifies (and explains the origin of) a turning point in this series of definitions around the year 2000. Pierre Demeulenaere addresses Boudon's theory of rationality; he shows how Boudon progressively conflated this notion with that of "reasons", and discusses the problems that Boudon had to face in his quest for a general understanding of rationality that would be able to capture not only how actors choose the means of their actions but also the genesis of the goals they want to pursue. By scrutinizing Boudon's reading of Tocqueville, Stephen Turner also formulates a warning against Boudon's ambition to explain every action through reasons, points out aspects of human experiences (like "tacit knowledge") that seems to resist to an interpretation in terms of actors' reasons, and emphasizes the centrality of interaction-based social learning as a mechanism of social life.

Part VI ("Training the New Generation") closes the book with the goal of delivering messages on how Boudon could be useful when teaching sociology. In particular, Emily Erikson re-reads Boudon's *Theories of Social Change* (1986), and advises the next generation to rely on existing computational tools to model systems of interdependency, and the unintended consequences of social actions rooted therein, that Boudon instead regarded as responsible for making it hard, if not impossible, to predict social changes at the macroscopic level. Fernando Sanantonio and Francisco Miguel consider a variety of Boudon's writings that Boudon himself regarded as resources for teaching sociology, and investigate the extent to which those pieces of work left a trace within recent books and handbooks in the field of sociological theory and quantitative methodology.

Finally, my own chapter offers a reasoned selection of Boudon's articles and book chapters that may be used to design a syllabus for a first-year, Master-level introductory course to sociology focusing on how to identify good research questions, on how to answer these questions through a set of well-defined research heuristics, and on how to understand sociology's diversity.

When considered as a whole, the eighteen chapters are clearly sympathetic toward Boudon. They show great intellectual respect to him; they are not condescending, however. This leads to an inedited assessment of Boudon's oeuvre where successes and failures are pondered in a non-apologetic manner. To the question of what Boudon's legacy is for today's sociology, the eighteen chapters seem to point to three main conclusions.

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First, the Boudon of the late sixties and seventies, that is, the Boudon working on specific empirical macroscopic phenomena, creatively contributed to devise a distinctive sociological research style focused on the construction of theoretical models clarifying how interdependent actions can lead to unexpected consequences; this research style also includes the use of formal models that are not limited to statistical ones. This is an important contribution that seems durable in that it is now part of well-established research programs in contemporary sociology.

Second, the Boudon from the mid-eighties onwards, that is, the Boudon seeking to develop a general theory of human behavior, convincingly contributed to identifying major limitations of a narrow understanding of rational action, and delivered an important research heuristic for those interested in micro-founding sociological analysis (i.e. "Always think of possible reasons behind actors' positive and normative beliefs, no matter how strange these beliefs may seem to an external observer"). He failed, however, to design sharp research designs documenting that reasons are the actual main drivers of every sort of choice and belief. This remains an important task for a future generation of scholars interested in theories of action.

Third, the progressive emphasis that the "mature" Boudon put on the "actor" should not lead to overlook that the "young" Boudon was a champion of the analysis of the interdependency among actors; at the same time, even this "young" Boudon tended to see others in these systems of interdependent actions as "abstract" others. The interdependency of actions nestled within concrete structures of dyadic as well as higher-order interactions, i.e., social networks, was not central to Boudon's ways of thinking. This is another research direction that Boudon left to the next generation for further exploration.<sup>3</sup>

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3 Interestingly enough, Boudon (2012, p. 18) lately seems to admit this himself: "Networks are today a popular topic of sociological research. But they are often



Needless to say, different readers are likely to read the book differently. Whether they agree or not with this assessment, the present book will reach its goal if, after picking this or that chapter, the reader feels motivated to go back to Boudon's original writings, to read more, and to develop their own view. After all, the present book was ultimately thought of as an invitation to read Boudon, and engage in an intellectual conversation with him.

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Gianluca Manzo is Professor of Sociology at Sorbonne University and a Fellow of the European Academy of Sociology. His research applies computational models and social

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treated in a mere descriptive or mechanical fashion, while a connection with the Theory of ordinary rationality would make network research more fruitful, as many classical and modern sociological works suggest."

network analysis to the study of social stratification and diffusion dynamics. He is the author of *La Spirale des inégalités* (PUPS, 2009) and of *Agent-based Models and Causal Inference* (Wiley, 2022). He also edited *Analytical Sociology: Actions and Networks* (Wiley, 2014) and the *Research Handbook on Analytical Sociology* (Edward Elgar, 2021). More information is available on his webpage: <http://www.gemass.fr/member/manzo-gianluca/>.



PART I

SCIENTIFIC PATH AND STYLE



## A SHORT JOURNEY THROUGH BOUDON'S WORK

*Pierre-Michel Menger**Collège de France**Académie des Sciences Morales et Politiques, France*

In 1990, Raymond Boudon was appointed to a Chair previously held by Jean Stoetzel at the Académie des Sciences Morales et Politiques. At the time, the Chair was located in the Philosophy section, as was the Chair held by Raymond Aron in 1963. In 1999, Boudon joined the Morale et Sociologie section of the Academy where Tocqueville was one of his most famous predecessors. I was pleased to accept the opportunity to join the Academy last year, although I unfortunately arrived far too late to continue the vibrant discussions I had had with Boudon over the years.

During his time at the Academy, Boudon organized the conference “*Durkheim aujourd’hui*” to commemorate Durkheim’s birth 150 years earlier, where he presented the lecture, “The nature of religion according to Durkheim”. He gave two further public lectures at the Academy. One was on the issue of representative democracy, entitled “What does it mean to give power to the people?”, a burning question for our current political situation in France. Finally, in 2011, he offered his “Reflections on sociology”, by opposing two major orientations, methodological singularism and the holistic conception of society. He acknowledged the influence of structuralism, but he rejected any of its ties with the “false consciousness” assumption – in his words, “the idea that the reasons individuals give themselves for their actions are in principle illusory, which justifies ignoring them and attributing social phenomena to the action of social structures alone”. He also contrasted a dominant sociology based on descriptive surveys with a scientifically robust, but minority sociology based on quantitative work, a contrast that may still hold today:

Today, French sociology has largely abandoned the global visions of structuralism. Above all, it is highly diverse. Indeed, what characterizes all contemporary French sociological production is mainly descriptive surveys. They are sometimes instructive, but their scale is usually modest, so that they

are hardly distinguishable from those spontaneously carried out by journalists in the field.

Finally, the most interesting – if not the most visible – dimension of contemporary French sociology, the one that represents a distinctive contribution to sociology and frankly distinguishes it from history or journalism, is, in my view, represented by the quantitative surveys on which I have focused. They are of descriptive interest, but above all, they are of critical interest. They make it possible to correct the clichés marketed by *holistic* sociology, which attracts media attention as soon as it demonstrates a certain talent for writing. These quantitative surveys provide the same kind of service to the analysis of societies as CT scans do to medicine: they enable us to see what we cannot see with the naked eye (Boudon 2011, my translation).

In the 1970s and 1980s, book after book, Boudon developed a well-argued alternative to the deterministic sociological theory that had become dominant in France.

One of his influential contributions was a theory of social processes, elaborated in his trilogy *The Logic of Social Action* (1981), *The Unintended Consequences of Social Action* (1982) and *Theories of Social Change* (1986b).

These three books offered a completely new toolbox in sociology, including:

- a sociology of action and interaction systems, with game theory playing an essential role
- a sociology of change and its mechanisms, in which the reproduction of social order is a special case rather than a general law, and in which uncertainty and chance have their part to play
- the modeling of micro/macro relationships based on composition effects and emerging social phenomena that arise from the aggregation of individual actions and decisions
- the extensive use of quantitative methods and simulations, based on his previous *Analyse mathématique des faits sociaux*
- the formulation of paradoxes as a favorite heuristic device
- and to conclude this short list, the opening up of sociology to economics, political theory and cognitive psychology.

I should add his ethics of scientific discussion. More than any theorist in French sociology in the 1970s, Boudon would review the various existing models and paradigms when trying to solve a new sociological enigma. To this end, Boudon constantly refers to the founding fathers as well as contemporary leading theorists in social sciences: Tocqueville, Marx, Weber,

Simmel, Lazarsfeld, Merton, Schumpeter, Stouffer, Davis and Moore, Parsons, Schelling, Hirschman, Olson, Coser, Coleman, and Elster, among others.

What a pedagogical feast it was for me, in my student years, to learn how Boudon modeled various social processes from a wide range of aptly summarized case studies. Rightly so, his book *The Logic of Social Action* (1981) has been hailed by Siegwart Lindenberg (2013) as a perfect introduction to sociology.

The issue of education and social mobility was the subject of his famous monograph, *L'Inégalité des chances (Education, Opportunity and Social Inequality)* (Boudon 1973), which is by far the most cited in his entire body of work and remains actively discussed today. At a time when Bourdieu's theory made school the instrument for legitimizing the reproduction of inequalities and the social structure, Boudon built a completely different actionist and interactionist framework. His sociology endows the actors (students and students' families) with rationality under constraints, with unequally distributed resources, and with the capacity to choose and compute the educational decisions and investments to be made. Students' educational careers are sequenced and punctuated by tests and points of bifurcation, leading to cumulative and exponential inequalities of opportunity. Above all, Boudon highlights the aggregation effects of individual family decisions when growing demand for schooling doesn't match the volume and structure of jobs created by the economy.

As a result, even if inequalities in schooling opportunity are slowly but steadily decreasing, and even if credentials are playing a growing role in status attainment, the impact on social mobility is weak, as Max Weber prophesied as early as 1920.

This enigma is presented with impressive vigor, and its resolution is as simple as the paradox is powerful. The quantity and quality of jobs increase far less rapidly than the number of graduates: hence the defensive nature of individual educational investment, as noted at the same time by Lester Thurow, to whom Boudon often refers. This impressive argument is still valid, as I have shown in my spring 2024 lecture on education at the Collège de France, where I cited Thurow:

From the job competition point of view, however, education may become a defensive necessity. As the supply of educated labor increases, individuals find that they must improve their educational level simply to defend their current income positions. If they don't, others will, and they will find their current job no longer open to them. Education becomes a good investment, not because it would raise people's incomes above what they would have been if no one had

increased his education, but rather because it raises their income above what it will be if others acquire an education and they do not. *In effect, education becomes a defensive expenditure necessary to protect one's "market share."* The larger the class of educated labor and the more rapidly it grows, the more such defensive expenditures become imperative (Menger 2024, citing Thurow 1972).

Boudon also refers to a paper by Anderson published earlier, in 1961, that might have provided the first impulse to his view, insisting on paradoxical effects of micro-decisions in the context of educational investments rising faster than labor market "openings". Anderson's argument is that:

[A]s schools enroll progressively larger proportions of children and retain them longer, the correlation between schooling and later occupations can diminish over time. The influence of schooling upon mobility depends partly upon changes in the number of "openings": here we focus upon openings at the top. If children born in upper strata are distinctively capable inherently or receive superior training, there will be fewer vacated openings into which children of lower origins can penetrate. In dynamic economies, multiplication of tertiary and shrinkage of primary occupations plays a major part in alteration of opportunities (Anderson 1961).

Let me also quote Boudon's own words, taken from his reply to a critical discussion of his book by Alain Darbel (a statistician who worked closely with Bourdieu):

... the increase in the demand of schooling has to be analyzed as the paradoxical result of the aggregation of defensive strategies creating a prisoner's dilemma structure. Each individual seeks to protect himself against the effects of other people's demand on his own social expectations; but in so doing, each individual contributes to increasing the amount of schooling that everyone must obtain in order to reach a given social status, whatever that status may be (Boudon 1975, my translation).

A far-reaching consequence emerges: according to the Anderson paradox, social mobility remains stable or evolves *far less* than what the substantial impact of education on acquired status would promise.

To sum up Boudon's explanation, there is a persistent *mismatch between the schooling investment structure and the job supply structure*.

One could object that such a mismatch isn't static. Boudon finds that inequality of educational opportunity is slowly but steadily declining – nominally decreasing, I would add. Indeed, we also know that grade

inflation in a credential society is accompanied by an increasing horizontal differentiation in the returns on college education by field of study and a strong stratification of universities based on their selective admission policies.

Boudon said he was mainly interested in a minimalist approach to theorizing social stratification. To him, the paradox of a credential-competition generating social immobility was, at least at that time, of greater interest than a fine-grained exploration of trends in education or the changes in the structure of jobs. Replying to Robert Hauser's criticism of *L'Inégalité des chances* (Boudon 1973), Boudon lists the series of paradoxes he has been able to resolve, justifying in passing why theoretical work took precedence over the empirical foundation and potential re-elaboration of his theory. Here is the excerpt:

... my aim was to answer a set of questions not of the how much type, but of the why type [...] Given my objective [...], I came to the idea of building a model roughly describing the basic mechanisms responsible for educational and social inequality, to see whether it generated the "paradoxical" outcomes some of which are listed above. At one point, I thought of building a model fitted to French data, but abandoned the idea – available was only the partial observation of a single cohort, some aggregate data on the composition of the student body at some levels for some points in time, and some other partial aggregate and survey data (Boudon 1976).

Hence, Boudon's primary interest lies in explanatory rather than descriptive models, and his main quest is for simplicity in demonstrations.

He finds himself in good company. As he notes:

[D]escriptive models have to be fitted to data. Explanatory models may ultimately fit no data at all! and nevertheless increase our understanding of the phenomena they consider. A good example of this extreme case can be found, for instance, in Thomas Schelling's *Dynamic Models of Segregation* (1971), which Schelling has never made any effort to fit to any particular context but which increases powerfully our understanding of segregation (Boudon 1976, pp. 1178-1179).

Here, I would also like to highlight another facet of Boudon's work: his editorial role. Several of the sociologists and social scientists on whom Boudon relied were translated and presented to the French public in the famous book series *Sociologies* (sociology being in the plural), which he and François Bourricaud founded and directed. Any student who was trained in the last quarter of the twentieth century will happily recall the magnificent diversity of research thus made available. In so doing, Boudon was faithful to the early

stages of his career, which had taken him to New York to work with Lazarsfeld and become acquainted with North American quantitative social science.

I am less familiar with the second part of Boudon's work, which starts in the mid-1980s with his book *L'Idéologie* (1986a) and culminates in *Le Juste et le Vrai* (1995) and *Le Sens des valeurs* (1999). Previously, Boudon considered the rationality of individual action to be embedded in complex systems of interaction. Now, in his quest for a cognitive microfoundation of individual behavior, Boudon assumes that people, regardless of their actions, have good reasons for wanting and doing something. Interaction structures, feedback loops, adaptation processes, and unintended macro-effects seem to play a lesser role.

Boudon's challenge is to turn reasonability into a general force, in order to oppose theories that invoke unconscious or irrational motivations, beliefs, and preferences. His work then becomes increasingly philosophical, epistemological, and confidently rationalist in a broader sense.

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Boudon's new ambition was certainly to extend his theoretical research to political, aesthetic, moral, or religious phenomena, using the simple, unifying framework of an axiological and analytical cognitive sociology. This approach leads him to address, for example, the social justice dilemmas in Rawls's theory, or the issue of relativism and objectivity in science.

I note that in Boudon's cognitive sociology program, beliefs and reasons are ordinary guides to action, yet subject to multiple effects that can tip them over into what the observer would consider irrational, erroneous, etc. A list of these effects is drawn up in his book on *Ideology*: effects of communication, of position, of perspective, of disposition, of situation, of authority, to name but a few.

While reasons for action are affected by individual and contextual parameters, they are not, for all that, subjective, Boudon (1993) asserts. Given the variability of factors influencing individual reasons, Boudon must also find a way to explain the formation of collective beliefs, and do this without invoking unconscious mechanisms. He then argues that the reasons held by so-called *ideal-typical* individuals help define collective beliefs within a group.<sup>1</sup> This rather enigmatic argument highlights the difficulty of applying

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1 The ideal-typical individual seems close to those abstract actors to whom economics refers to model the behavior of actors such as producers or consumers. The argument makes it possible to aggregate individuals and their reasons for acting to explain collective beliefs and behaviors. As Raymond Boudon writes in his book *Raison, bonnes raisons*: "Tocqueville always treats the collective beliefs he sets out to explain as the aggregate effect of individual beliefs, or more precisely: beliefs that the sociologist can legitimately attribute to ideal-typical individuals. Then, he



the paradigm of individualistic rationality, taken in a broader sense, to social groups – without sketching simple mechanisms for aggregating beliefs. This contrasts with his previous work on processes, which refers to composition effects. In short, good reasons for action are affected by contingencies that are difficult to parameterize, when their core rationality must be preserved.

At some point, Boudon (1993) admits that he does not in any way conclude that all beliefs must be explained by reasons. This may make his cognitive sociology more speculative than prone to the falsifiability test.

One constant remained in Boudon's work right up to the end of his highly productive career: his passion for what the best of social sciences, past and present, had to offer for his quest to solve enigmas. In his Pantheon, one sociologist stands out as his supreme hero, and even more so in the last part of his career: Max Weber. We have indeed many good reasons to place Raymond Boudon's inventiveness under Weber's aegis.

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postulates that restoring the meaning of these beliefs amounts to reconstructing the reasons that an ideal-typical actor has for adhering to them. In short, he places himself within the MRG [« Modèle Rationnel Général »] framework: his approach consists in showing that collective beliefs derive from an argumentation that none of the real actors may have ever literally developed, but which can in all likelihood be imputed to an ideal-typical actor" (1993, p. 67, my translation).

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THE TRANSATLANTIC CIRCULATION  
OF A SOCIOLOGICAL SCIENTIFIC ETHOS:  
THE CORRESPONDENCE OF RAYMOND BOUDON

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The international circulation of sociological ideas between France and the United States has long interested historians of the social sciences as well as sociologists themselves. There are multiple approaches to studying this phenomenon. Some focus on a detailed study of singular “trajectories” – for instance, Maurice Halbwachs’s (Topalov 2005), Jean Stoetzel’s (Stankiewicz 2008), and Michel Crozier’s trips to the United States (Chaubet 2013). Others take a broader view, discussing the social and political conditions surrounding this flow and its more or less structural effects (Pollak 1976, Heilbron 2005, Boncourt 2016).

Comparative studies of how individuals and their ideas have been received – whether Americans in France (Marcel 2004) or the French in the United States (Ollion and Abbott 2016) – often highlight two key points. First, sociologists differ greatly in terms of their ability to transcend international borders in the long run. A small minority manages to catch and hold their foreign peers’ attention, but most simply remain invisible and overlooked, and this invisibility is often not of the kind one might expect. Second, a small group of *mediators*, fixtures within their national academic landscapes, plays a crucial

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relational role by securing research visits, book translations and adaptations, and conference appearances.

This chapter contributes to our understanding of these international flows among sociologists by focusing on the case of Raymond Boudon. In terms of the relationships between the French and American sociological communities, his career in sociology is of considerable interest, for at least three reasons. First, as a recent study points out, Boudon features among the most-cited French sociologists in American sociology journals<sup>1</sup> (Ollion and Abbott 2016). Second, recently established archival collections<sup>2</sup> make it possible to examine his international trajectory via new material that highlights the importance of two classical figures in American sociology: Paul Lazarsfeld and Robert Merton. These two sociologists played a central role in Columbia University's Department of Sociology and, more broadly, in shaping a scientific conception of sociology during the postwar era. Finally, circulation between France and the United States involving Boudon have already given rise to various remarks, and these can now be assessed against archival evidence. One memorable and notable example is his strong criticism of the reconstruction of his time at Columbia that Henri Mendras provides in *Souvenirs d'un vieux mandarin* (Mendras 1995).<sup>3</sup>

This chapter is divided into three main sections. The first examines the nature of the documents recently deposited in the French National Archives, highlighting their diversity as well as detailing how, in the autumn of 2014 and for the purpose of this chapter, we began studying a specific portion: Boudon's general correspondence. In particular, this first section serves to highlight the distinctiveness of this correspondence and to broadly characterise the nature of the informal personal network around which Boudon's scientific activity was organised from the 1960s through to the early 2000s. The second section focuses on a subset of Boudon's general correspondence, namely his exchanges with Paul Lazarsfeld and Robert Merton. It sets out both the characteristics of

1 The citation analysis (Ollion and Abbott 2016) covering the period 1970–2012 puts Raymond Boudon in sixth position, behind Émile Durkheim, Pierre Bourdieu, Bruno Latour, Alexis de Tocqueville, and Michel Callon.

2 This case is of course not unprecedented. Recent deposits of researchers' archives have served to encourage new research perspectives on the development of sociology in both France (Borzeix and Rot 2010) and the United States (Dubois 2014a, 2014b).

3 "Moreover, the book contains factual errors and rough guesses that make it an unreliable source for future historians. To take an example that has the disadvantage of being personal but the advantage that I can judge it directly, Mendras claims, 'Stoetzel sent Boudon to Lazarsfeld at Columbia.' Nobody sent me to Columbia, and certainly not Stoetzel, whom I had not yet met and who was not even aware I existed" (Boudon 2001). [Translator's note: Our translation.] Unless otherwise stated, all translations of cited foreign language material in this article are our own.

the available material and how the nature and significance of the relationship between the Sorbonne's and Columbia's sociology departments have been described therein. The third and final section analyses the available correspondence between these three sociologists. The chapter emphasises the significance of what this new material reveals about the processes surrounding learning, reputation, and academic mobility, as well as highlighting the nature of the scientific ethos shared by the three sociologists.

## THE RAYMOND BOUDON COLLECTION

Originally intended to support the creation of a future archival centre for social science research (Sène 2014), the Boudon Collection was deposited in the French National Archives in 2013. In a series of interviews conducted by Brigitte Mazon to mark the creation of this collection (Boudon 2013), Boudon recounted key moments in his career and reflected on the nature of the archival material itself. When asked, "What do you think your archives contain?" he replied:

It's probably mostly everyday items – that is, everything that makes up the life of a teacher and researcher: thesis reports, candidate evaluations, recommendation letters, administrative documents, funding requests, and so on. Maybe some more original things as well. [...] But most of it is probably routine, everyday documents. I do not think that makes them uninteresting, though, because if one day a historian wanted to write the history of social sciences in the second half of the twentieth century, they might find them useful (Boudon 2013, p. 608).

The archival catalogue shows how this "routine" of academic life had, as one might expect, multiple dimensions. The Raymond Boudon Collection is divided into six main document categories. The first relates to institutional activities within the Centre National de la Recherche Scientifique (CNRS) (French National Centre for Scientific Research), other national and international research organisations, and the Académie des Sciences Morales et Politiques. The second concerns teaching and supervision activities at various institutions, including Paris-V Descartes University (now Paris Cité University), Paris-IV Sorbonne University (now Sorbonne University), the University of Geneva, and Harvard University, and it includes Boudon's contributions to reports from thesis defences that he was involved in.<sup>4</sup> The

4 The inventory of these contributions to thesis defence reports gives us a snapshot of an academic community, made up, in alphabetical order, of: Abdelmajid Arbouche, Michael Ballé, Enric Becescu, Daniel Benamouzig, Henri Bergeron, Emmanuelle

third category concerns trips, conferences, and interviews. Notably, it contains evidence of an initial application for US permanent residency made in 1969, as well as texts from seminars and lectures he gave in France and abroad. The fourth is a collection of various publications and offprints from 1962 to 2011. The collection also includes all books written or cowritten by Boudon plus their various translations, along with interviews and reviews published in the press when these works were released. The fifth category focuses on Boudon's publishing activities, including publishing contracts, press kits, manuscript preparations, and correspondence with publishers – and especially, of course, with Presses Universitaires de France about day-to-day matters related to the renowned “Sociologies” collection (or the “Blue Collection,” as it is sometimes known, for its signature blue covers) (Langlois 2008). Finally, the sixth category, which is the main focus here, contains general correspondence sent and received between 1961 and 2012.

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Was Boudon part of the community of epistolary sociologists? He answered plainly: “There is no correspondence, strictly speaking, in my archives. [...] I mainly had conversations with several people [...] [which] led me to develop certain ideas and texts and revise others” (Boudon 2013). Should this apparent lack of interest in epistolary relationships – which nevertheless gave rise to a not-insignificant eleven archival boxes of correspondence<sup>5</sup> – mean these documents offer no insights into Boudon's career in sociology or into French and international academic life more broadly? This seems doubtful, especially as Boudon, when discussing his “intellectual friendships,” recalled his relationship with Lazarsfeld and Merton:

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Betton, Martin Blais, Raymond Bourdoncle, Alban Bouvier, Alain Boyer, Gérard Bronner, Nathalie Bulle, Shim Chang-Hack, Mme Chchenkova, M. Choi, François Cusin, M. Damoiselet, Jacqueline Deguise-Le Roy, Pierre Demeulenaere, Lilyane Deroche-Gurcel, M. Dies, Annette Disselkamp, Michel Dubois, Éric Dumaitre, Patrice Duran, Joseph Facal, Mme Fericelli, Renaud Fillieule, Erhard Friedberg, Frédéric Gérard, Claude Giraud, Francine Gratton-Jacob, Benoît Grison, Alexandre Guillard, Dominique Guillo, Boris Guroy, Taik-soo Hyun, Agata Jackiewicz, Loïc Jarnet, Marc Lambret, Marc Le Menestre, Philippe Lefebvre, Marc Leroy, Éric Letonturier, Carlo Lottieri, Raul Magni-Berton, Gianluca Manzo, Jean-Christophe Merle, Bruno Milly, Albertina Oliverio, Lucien Samir Oulahbib, Maxime Parodi, Dunia Pepe, Emmanuel Picavet, Emmanuel Plot, Jocelyn Raude, Emmanuel Renaud, Sandra Rocquet, Michel Routon, Bertrand Saint-Sernin, Gérard Spiteri, Anne Staszak, Philippe Steiner, Mohamed Taleb-Khyar, Ada Teller, Dominique Terré-Fornacciari, Laurent Tessier, Kei Tsujihara-Sakata, Patricia Vanier, Fiorella Vinci.

- 5 A quantity that is admittedly modest when it is compared to the available correspondence by some epistolary sociologists, Robert Merton being one example (Dubois 2014b).

You may find some letters from Lazarsfeld in the archives, and perhaps a few from Merton. I don't know whether you'll find the one where Merton wrote to Lazarsfeld saying that he had the impression, upon reading my work, that I had been listening to their conversations at Columbia. Lazarsfeld, thinking I would appreciate having it, sent me the letter (Boudon 2013).

Before offering a more detailed description of the correspondence between the three men, it seems useful to provide an overview, albeit a brief one, of Boudon's general correspondence and the work undertaken on it since autumn 2014.

Work on these eleven boxes of correspondence took place across two phases: first on premises at the École des Hautes Études en Sciences Sociales (EHESS) archives service, then at the French National Archives' Pierrefitte-sur-Seine site after the collection was transferred there. The initial objective, agreed with the head of the GEMASS<sup>6</sup> laboratory, which Boudon founded in the early 1970s,<sup>7</sup> was to examine and catalogue all the correspondence to enable further processing on several subsequent occasions. To date, an epistolary corpus of approximately 2,150 items (letters received, sent, or shared in copy) has been compiled for the 1961–2001 period. About 40 percent of the items were sent by Boudon, while the remaining 60 percent are letters in which he was the primary or secondary recipient (including items sent in copy).

Most of the correspondence in the general corpus relates to the period between 1980 and 2000. Table 1 shows the composition of Boudon's main epistolary circle. Although we will not go into detail regarding the names in this table – some are well known, others less so – two observations are worth highlighting here. First, many names are tied to the French academic environment, particularly the university and publishing spheres. Second, among the foreign colleagues in Boudon's epistolary circle, Paul Lazarsfeld and Robert Merton stand out, being ranked second and third respectively. The correspondence of intellectual friendship between Boudon, Lazarsfeld, and Merton is therefore a key element of Boudon's general correspondence, making this subset and the three sociologists' relationship an essential focus of study.

6 GEMASS stands for Groupe d'Étude des Méthodes de l'Analyse Sociologique de la Sorbonne (Sorbonne Study Group on Methods of Sociological Analysis).

7 See in this regard the pages dedicated to the Boudon archives on the GEMASS website, see <http://www.gemass.fr>.



Table 1: Boudon's epistolary circle, n=40, ordered by decreasing frequency  
(ranked 1<sup>st</sup> to 40<sup>th</sup>)

| Id.           | rank | Id.              | rank | Id.            | rank | Id.           | rank |
|---------------|------|------------------|------|----------------|------|---------------|------|
| Busino-G.     | 1    | Coenen-Huther-J. | 11   | Casanova-J.C.  | 21   | Drouard-A.    | 31   |
| Lazarsfeld-P. | 2    | Peyrefitte-A.    | 12   | Pellicani-L.   | 22   | Forsé-M.      | 32   |
| Merton-R.K.   | 3    | Helle-H.         | 13   | Poussou-J.     | 23   | Lautman-J.    | 33   |
| Eisenstadt-S. | 4    | Saint-Sernin-B.  | 14   | Barker-P.      | 24   | Lazar-J.      | 34   |
| Bouvier-A.    | 5    | Bronner-G.       | 15   | Berthelot-J.M. | 25   | Lindenberg-S. | 35   |
| Prigent-M.    | 6    | Morin-J.M.       | 16   | Birnbaum-P.    | 26   | Marot-G.      | 36   |
| Bunge-M.      | 7    | Paquetteau-B.    | 17   | Massot-A.      | 27   | Montbrial-T.  | 37   |
| Bell-D.       | 8    | Rezsohazy-R.     | 18   | Rotariu-T.     | 28   | Picavet-E.    | 38   |
| Assogba-Y.    | 9    | Terré-D.         | 19   | Sakata-K.      | 28   | Pithod-A.     | 39   |
| Leroy-M.      | 10   | Boyer-A.         | 20   | Delara-P.      | 30   | Renaut-A.     | 40   |

## BETWEEN PARIS AND NEW YORK:

### A CORRESPONDENCE OF INTELLECTUAL FRIENDSHIPS

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Boudon's correspondence with his two "accomplices" from Columbia University (Boudon 2010) spanned a fairly lengthy period, beginning as it did in the 1960s and continuing until the early 2000s.

The archive collection contains thirty-four letters from Lazarsfeld to Boudon (between 1 December 1965 and 11 August 1976) and sixteen from Merton (between 11 June 1970 and 7 November 2001).<sup>8</sup> Both correspondences were cut short by death – Lazarsfeld's in August 1976 and Merton's in February 2003. In both their duration and content, these exchanges demonstrate a profound elective and cognitive affinity, one that endured throughout Boudon's intellectual journey, from his thesis on the mathematical analysis of social data, across his formation of the methodological individualism paradigm, and on to the progressive extension of that paradigm towards a theory of expanded rationality incorporating axiological rationality.

The correspondence also documents shifts in the French and American professional environments. These two aspects – the formation of a sociological paradigm in France; and the national and international transformation of disciplinary frameworks – are what make the correspondence valuable today, for both historians of the social sciences and sociologists themselves.

The epistolary exchange began in the mid-1960s, a period when the social sciences in France were undergoing both institutionalisation and professionalisation. The period also involved substantial financial and

8 On top of these letters, there are those which Lazarsfeld and/or Merton sent in copy to Boudon. It should be noted that the majority of the archived correspondence was sent by Lazarsfeld and Merton. Copies of letters sent by Boudon himself within this correspondence are rare.



organisational investments from American foundations – Carnegie, Rockefeller, Ford, etc. – and international bodies – e.g., the United Nations Educational, Scientific and Cultural Organization (UNESCO) – the goal being to facilitate and speed up the integration of teaching and research, and it was a time in which transatlantic exchanges intensified (Drouard 1982; Miéville and Busino 1996). Reflecting on the state of French sociology in those years, Boudon summed up the view he held as a young *normalien*, a student of the prestigious École Normale Supérieure (ENS): “In the field of French sociology, three men ran the show: Aron, Stoetzel, and Gurvitch” (Boudon 2013, p. 345). They were the three leading figures within sociology at the Sorbonne during this period, where a sociology degree had been established in 1958, and they were essential references for anyone wishing to study in this field. Gurvitch, the sole professor of sociology at the Sorbonne from 1948 before being joined by Raymond Aron, established himself as a key figure in French sociology at the time (Marcel 2001). Boudon, who himself subsequently became a major international figure in sociology, would attest to Gurvitch’s status: “He held the only sociology chair at the Sorbonne, edited the only sociology book series at Presses Universitaires de France, the ‘Bibliothèque de sociologie contemporaine’ collection. He presided over the only flourishing sociology journal of that period, *Cahiers internationaux de sociologie*. He ruled French sociology as a despot” (Boudon 2013, p. 344). Raymond Aron, having joined the Sorbonne in 1955, went on in 1960 to found the European Sociology Centre (Centre Européen de Sociologie, CSE) and the *Archives Européennes de Sociologie*, which is now the *European Journal of Sociology* (Heilbron 2015, p. 172). As for Jean Stoetzel, whom Boudon would later acknowledge as his “true mentor” in France (Boudon 2003, 38),<sup>9</sup> he entered the Sorbonne in 1955 – the same year as Raymond Aron – as chair of social psychology. Prior to that, in 1938, largely inspired by Lazarsfeld’s survey research, Stoetzel had established France’s first polling institute, the French Institute of Public Opinion (Institut Français d’Opinion Publique). He was also behind the founding, in 1960, of the *Revue Française de Sociologie*, whose mission was to publish empirical research findings.

Boudon never hid his reservations about Gurvitch’s sociology or his indifference to Aron’s: “I could see that Georges Gurvitch’s sociology was respectable for the energy it conveyed and contained, but it had little future. Raymond Aron seemed to be above all [...] a great intellectual. He himself told me that he saw his role as popularising the social sciences” (Boudon 2003,

9 Boudon was elected on 29 May 1990 to the seat left vacant by Jean Stoetzel at the Académie des Sciences Morales et Politiques (see Boudon 1992).

p. 38). It was therefore only natural that the young *normalien*, a mathematics enthusiast with a passion for scientific rigour, would place his hopes in Lazarsfeld and his French theoretical counterpart, Jean Stoetzel.<sup>10</sup>

At the time, Columbia's sociology department, which gravitated around Lazarsfeld and Merton, was at its peak (Boudon 2003, p. 37; Pollak 1979). Whereas Parsons's functionalist sociology began declining from the 1950s and the famed Chicago school was losing momentum, Lazarsfeld's empirical, quantitative sociology – founded on the intensive application of mathematics and statistics to produce an “empirical analysis of action” – was gaining scientific prominence and institutional importance. By the early 1960s, Lazarsfeld's Bureau of Applied Research, and Columbia University more broadly, constituted the most prestigious sociology centre in the United States. Reflecting on his career and intellectual journey, Boudon observed by way of a quip: “Scientific sociology exists: I've met it myself” (Boudon 2010, p. 4). One might say, in those years, he encountered it in the form of Lazarsfeld.

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Having secured a Ford Foundation grant through Raymond Aron, Boudon decided, like Stoetzel before him, to go to Columbia, spending the 1961–1962 academic year there: “No one sent me to Columbia, but I decided to learn under Lazarsfeld after my military service because, quite by chance, I came across *The Language of Social Research* while browsing the shelves of the ENS library on rue d'Ulm. Rightly or wrongly, the book struck me as offering an alternative to the Gurvitchian sociology that then dominated in France, which I had always suspected of hiding trivialities within its thickets of typologies and nitpicking definitions, as well as to structuralism, which I thought needed to be handled carefully, both for its totalising ambition and for its Platonism” (Boudon 1996, p. 77).

The dialogue initiated in the early 1960s between Boudon and the top figures from Columbia's sociology department remained intense and lively for many years. However, it was a dialogue that extended well beyond correspondence, which was but a small part of a much broader exchange. Transnational scientific circulation also involved the movement of people, methods, and ideas (Chaubet 2014), and Boudon's dialogue with thinkers from Columbia was no exception to this rule. Boudon, who, as Aron remarked, was “a kind of island of American sociology on French soil” (Boudon 2010, p. 13), made many visits to the US:

I lived in the United States several times: first as a student at Columbia for a year, then in 1972 as a fellow at the Palo Alto Centre in California [the Center

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10 On the theoretical and methodological convergence between Lazarsfeld and Stoetzel, see Blondiaux (1990).

for Advanced Study in the Behavioral Sciences]. I was invited to several universities, including Harvard in 1973, Chicago in 1986, Indiana University, and New York University (Boudon 2013, p. 607).

We also know that Lazarsfeld, born in Vienna and very keen to export his sociological vision to Europe, likewise made many trips to France (Gemelli 1998; Lécuyer 2002). Lazarsfeld's visits began in 1948 with a seminar at the Centre for the Scientific Study of Domestic Policy (Centre d'Études Scientifiques de la Politique Intérieure) of the French National Political Science Foundation (Fondation Nationale des Sciences Politiques), on an invitation from its president, André Siegfried. They peaked in the mid-1960s, starting with UNESCO-related work in 1960 and continuing with two extended teaching periods at the Sorbonne in 1962–1963 and 1967–1968 organised by Stoetzel, and culminated in Lazarsfeld receiving the title of professor emeritus from the Sorbonne.

The transatlantic circulation of scientific ideas also occurs through the production of books, their translation, and their necessary adaptation to particular intellectual contexts. Between 1965 and 1976, the period covered by the available correspondence with Lazarsfeld, Raymond Boudon published several works. Aside from his two theses (*L'Analyse mathématique des faits sociaux*<sup>11</sup> [1967] and *À quoi sert la notion de structure?*<sup>12</sup> [1968]), these included *Les Méthodes en sociologie* (1969), *La Crise de la sociologie*<sup>13</sup> (1971) and the book that established his international scientific reputation, *L'Inégalité des chances*<sup>14</sup> (1973). Lazarsfeld closely supervised the development of Boudon's theses and made sure the translations of his works were of a high quality so the conditions under which the American scientific community received them were as favourable as possible. In return, Boudon helped disseminate Lazarsfeld's thought in France by coediting several of his books: *Le Vocabulaire des sciences sociales* (1965), the first volume of the "Méthodes de sociologie" series; and its second volume, *L'Analyse empirique de la causalité* (1966); and, with François Chazel, *L'Analyse des processus sociaux*, the third volume (1970). He also published selected writings by Lazarsfeld in French (1970) and later in English (1993), after Lazarsfeld's death (Lazarsfeld 1970; Lazarsfeld 1993).

11 Regarding the English translation of this work, see Lazarsfeld's comments in the next section.

12 Translated into English as *The Uses of Structuralism*, by Michalina Vaughan (Heinemann, 1971).

13 Translated into English as *The Crisis in Sociology: Problems of Sociological Epistemology*, by Howard H. Davis (Macmillan, 1980).

14 Translated into English as *Education, Opportunity, and Social Inequality: Changing Prospects in Western Society* (John Wiley & Sons, 1974).

As for Boudon's relationship with Robert Merton, they continued to share their publications with one another and discuss their work until late 2002, shortly before Merton's death (Boudon 2010, 13). As Lécuyer (2002) recalls, Merton's arrival at Columbia in the same year as Lazarsfeld (1941) was the product of the inability of two figures in the sociology department at that time – theoretical sociologist Robert MacIver and methodologist Robert Lynd – to make a choice between them. MacIver wanted to hire Merton, while Lynd wanted Lazarsfeld. Rather than making a decision, the people in charge at Columbia chose to divide up the position, simultaneously recruiting Lazarsfeld and Merton. The complementarity between these two men and, through them, between the two dimensions of sociological analysis – methodology on one hand, theory on the other – had a deep impact on Boudon: “We had the impression that the duo of Lazarsfeld and Merton offered an example of remarkable collaboration” (Boudon 2003, p. 390). Likewise, in one of his last works, he comments, “The symbiosis between the one's methodological inspiration and the other's theoretical tendency impressed me” (Boudon 2010, p. 9).

Rejecting overarching approaches to society, Boudon saw Merton as the person who would make it possible to rethink the scales of sociological analysis: “Merton convinced us all that the concept of middle-range theory raised a vital question, because it contrasted theories explaining well-defined phenomena with ‘theories’ that claimed to address society as a whole” (Boudon 2010, p. 9). Indeed, much of Merton's theory of action and unanticipated consequences became central to Boudon's work, and particularly to *Effets pervers et Ordre social*<sup>15</sup> (1977) and *La Logique du social*<sup>16</sup> (1979). Merton was well aware of the importance of Boudon's novel theoretical contributions, noting, “Your book on *effets pervers* [perverse effects] strikes me as being of the first importance” (letter dated 15 May 1979). That said, Merton sometimes expressed concerns – light-heartedly – about the consequences that the international circulation of Boudon's publications might eventually have on his own publishing projects.<sup>17</sup>

15 Translated into English as *The Unintended Consequences of Social Action* (Macmillan, 1982).

16 Translated into English as *The Logic of Social Action: An Introduction to Social Analysis*, by David Silberman with the assistance of Gillian Silverman (Routledge and Kegan Paul, 1981).

17 Such as the 21 June 1982 letter in which Merton shares his astonished response to the English translation of *Effets pervers et Ordre social*: “Dear Raymond, It was kind of you to have a copy of the English translation of your *Effets pervers* sent to me. [. . .] I was a bit startled and, for a time, put off—just as you must have been when you discovered that the English publishers adopted the title of *Unintended Consequences of Social Action*. Let's hope that this title is not also adopted by

“IN THE MERTON-LAZARSFELD SPIRIT”:  
PROFESSIONALISATION AND SCIENTIFIC ETHOS

On a personal level, what stands out in this correspondence of intellectual friendships is the warm tone running through the exchanges between the three men: “Dear Raymond,” “Dear Paul,” “Dear Bob.” When one reads the letters sent back and forth, it is easy to pick up on the men’s joy at seeing one another again, their disappointment of missing the chance to meet, and the growth of their friendship. The letters provide evidence of – though only partly capture – a sustained dialogue. For instance, while on a flight taking him back to Columbia, Lazarsfeld, who frequently travelled across Europe and between France and the United States, wrote a letter continuing a conversation he had just been having with Boudon in Paris: “Dear Raymond, On my flight back I read your new chapter very carefully and I want to comment on one point in some detail” (letter dated 11 January 1965).

The three sociologists held one another in high regard, and the letters reflect this. Boudon expressed all the admiration he felt for Lazarsfeld, whom he called “his mentor” (Boudon 2013, p. 346), and on many occasions he spoke of his respect for Merton. Reading the letters to Boudon reveals that this feeling was mutual. In a letter dated 27 March 1991, for example, Merton tried to express to Boudon how much he agreed with Mario Bunge’s sentiment that Boudon should be seen, along with James S. Coleman, as one of the “very best brains in all of social science.” This mutual respect and admiration also emerge in responses to Boudon’s early works. Regarding his thesis, Lazarsfeld wrote, “It is a compliment to your thesis that I keep on thinking about it” (letter dated 14 June 1966). Writing a few years later to the prospective American publisher of *L’analyse mathématique des faits sociaux*, Lazarsfeld stressed its importance:

I was always sure that Boudon’s book is very important. It would have a very salutary effect in this country. There are so many mathematical sociologists that break up in little cliques favoring one method or another. Boudon shows that most of these trends are just special cases of the more general idea which he presents competently (letter dated 1 March 1971).

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an American publisher. It doesn’t quite preempt the title of the volume I have been editing, so perhaps no damage will have been done. I know how publishers sometimes make decisions without bothering to notify authors about them and so we’ll both have to live with it as a *fait accompli*. In a way, this is also poetic justice. After all, the volume on unanticipated consequences should have been in print at least two years ago, were it not for a variety of events that intervened [...].”

Beyond this personal dimension of affinity and mutual respect, the correspondence sheds light on different closely linked dimensions of Boudon's years of learning and professional socialisation. Boudon referred to Lazarsfeld as his "mentor," and Lazarsfeld clearly comes across as such in their letters. Upon returning from Columbia, Boudon decided to base his principal thesis, supervised by Jean Stoetzel, on the contribution of mathematics to social sciences and, at Lazarsfeld's suggestion, to dedicate his secondary thesis to the concept of structure, with Raymond Aron as supervisor.

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Many letters demonstrate how seriously Lazarsfeld took his role as mentor. Reflecting on his formative years, Boudon emphasised how demanding Lazarsfeld was: "He was a formidable thesis supervisor. He made an American friend redo his thesis three times before ultimately denying him the chance to defend it" (Boudon 2013). The correspondence between the two men shows Lazarsfeld meticulously read Boudon's thesis; he did not hesitate to have Boudon clarify certain points and be more rigorous about the concept of "structure," so as to define it in contrast to how their "enemies" understood the term (letter dated 14 June 1966). Some suggestions were more exploratory. For instance, Lazarsfeld invited Boudon to introduce the idea of "*sondage sociologique*" in France as an equivalent of "empirical social research":

After all, the word 'sondage' had a more general meaning before it was taken up by the public opinion people. It has a connotation of general inquiry by soundings, mean[ing] indicators, and this is, after all, what we do. While you cannot help that sondage now [often] means [...] public opinion research by sampling, you have a good chance if you turn [things] around and now call sondage sociologique all the other types of empirical soundings on contemporary social topics, irrespective of the specific method used (letter dated 11 January 1965).

Above all, though, the best measure of Lazarsfeld's commitment is his work on the ultimately unfinished translation of *L'Analyse mathématique des faits sociaux* (1974).<sup>18</sup> Finding the initial translation "catastrophic," he personally

<sup>18</sup> This translation endeavour fell through because of a series of difficulties connected to the translator Lazarsfeld himself initially chose: "I was responsible for Little, Brown having commissioned Dr. Kenneth Land for the translation. His wife is French-Belgian and he is a well-trained mathematical social scientist. I promised Boudon to go over the final English translation from a didactical point of view. [...] I got a copy a little while ago. It is mere coincidence that I became aware of the language problem. [...] The translation is still abominably bad [...] I would advise you and urge Boudon not to proceed with the publication of the book without a thorough revision" (letter from Lazarsfeld to Alfred L. Brown dated 1 March 1971).



retranslated parts of the text, finding intellectual satisfaction in the process: “I enjoy working on the translation because it forces me to think through a lot of matters to which I have not given enough attention before. Thus, for instance, I try to develop a direct derivation of what you call complex structures without interaction; I mean a derivative that comes directly from your decomposition of probabilities. I think I am on the right way but in any case, your book should not be burdened with this” (letter dated 9 March 1971).

Concerned about how Boudon’s book would be received by a readership accustomed to empirical and statistical analysis, Lazarsfeld was most demanding when it came to the argumentation’s substance. His letters from this period often included detailed handwritten memoranda urging Boudon to thoroughly revise this or that argument or passage: “Please reread your chapter 3 because it is the most important for the overall strategy of the publication” (letter dated 22 March 1971). Punctilious about the need for scientific rigour, Lazarsfeld let “nothing get by” him, while taking care not to offend or discourage Boudon:

I was glad to learn that my comments were useful. [...] You will see that nowhere do I disagree with your findings. I am essentially concerned with matters of presentation. But for an American edition, this is important because you have here a public which is much better prepared for your book than the French (letter dated 27 October 1969).

Lazarsfeld’s strictness as a mentor was simply the flipside of his unwavering support for the promotion of Boudon’s work in the United States. When *L’Inégalité des chances* was published in America as *Education, Opportunity, and Social Inequality*, Lazarsfeld had the opportunity to express how well he thought Boudon had responded to Robert M. Hauser (Hauser 1976 and Boudon 1976). In a letter Boudon often quoted, Lazarsfeld wrote, “Thank you very much for sending me the debate between you and Hauser. I was impressed by your reply and angry with Hauser. He is a very typical example of [a] statistical zealot although he is undoubtedly competent” (letter dated 15 August 1975).

The correspondence also shows how Lazarsfeld, in the 1970s, and Merton, in the 1980s, each played their part in establishing and strengthening Boudon’s reputation and visibility in American academia. In 1976, Lazarsfeld served on the American Sociological Association’s committee for the Stouffer Award, which rewarded outstanding methodological contributions to sociology. Having won an award in 1973, and with James Coleman having done so in 1975, Lazarsfeld, very understandably, was quick to contact Boudon and ask him to send him the supporting materials for an application:

There is a reasonable chance that you will be one of the awardees. It would be of great help if you were to send me a list of your publications – including translations – and your guest appearances like your stay at Harvard. You have to understand that the final outcome of such a competition often depends upon quite unforeseeable circumstances. So what I am writing you here is a reasonable guess, but by no means a firm forecast. Still it would be worthwhile if you were to send me the material I just mentioned (letter dated 30 June 1976).

A few years later, Merton played a similar role in bringing Boudon to Columbia University. In February 1983, Boudon was invited to deliver the Fifth Paul Lazarsfeld Lecture, a prestigious event organised by Merton. Merton wrote, “Dear Raymond, Interest is mounting in your giving the Fifth Paul Lazarsfeld Lecture. So much so, that we are eager to give it the greatest possible advance publicity – to colleagues, Paul’s many former students and longtime friends, the entire Columbia community, and social scientists from Boston and Washington, some word-of-mouth has gotten round and we have had inquiries about the exact date and other such details in recent weeks” (letter dated 27 September 1982). Barely a week after the lecture, Merton shared his enthusiasm with Boudon and relayed others’ glowing feedback: “Even in short retrospect, it seems that the blizzard of ‘83 did nothing to dim the occasion of your lecture. Quite the contrary; even now, members of that hardy crew who made it through the deepening snow to Low Rotunda are talking of their experience with evident relish. We can count, I think, on this event being told and retold for many years to come. I trust that you had a sense of the warm and interested response to your lecture.” Merton even hints that Boudon should extend his stay at Columbia indefinitely: “All of us here enjoyed your stay beyond easy description. A pity that you can’t manage a truly extended time with us – say, a semester or an academic year (to say nothing of your joining with us for good)” (letter dated 18 February 1983).

Beyond academic reputation and mobility, the correspondence among the three men more broadly reveals a shared scientific ethos. What Boudon found in Lazarsfeld and Merton was above all the idea there could be a “scientific analysis of action” (Boudon 1998, p. 371) and a “sociology [that was] centred on the individual” (Boudon 1970, p. 41) and was organised around the individual’s decisions. Affirming that he had always identified with a “scientific conception of sociology,” Boudon noted that he never struggled to understand Lazarsfeld’s writing, navigating his mental universe with ease (Boudon 1996, p. 76).

This is confirmed by a letter from Merton to Lazarsfeld about Boudon’s introduction to *Philosophie des sciences sociales*, which Lazarsfeld proudly shared with Boudon:



Dear Paul, When you get home from your Parisian triumph – not mere ovation – I want you to know how much I liked Boudon’s introduction to your book. It is splendid. It has caught your ‘obsessions’ right down to their roots. [...] It is almost as though he had been there through the years, listening to some of our endless conversations and now hearing your own insistence on what really matters. Boudon’s introduction belongs in the same class with your introduction to Sam’s book and that is saying a great deal. Not least, his French is as crisp and clear as your written English (letter dated 11 June 1970).

In a letter to Merton dated 24 February 1993, Boudon reiterated his admiration for what he called the “Merton-Lazarsfeld spirit.” To him, this spirit represented minimal concessions to the ideologies of the moment and *hic et nunc* values. And it was also – and above all – about choosing to make the creation of knowledge the discipline’s primary function. This choice, central to Lazarsfeld and Merton alike, clearly resonates throughout Boudon’s account of Columbia in his intellectual autobiography, *La Sociologie comme science*:<sup>19</sup> “The studies produced by the Columbia sociologists attracted me because they created knowledge. [...] They focused on concrete, specific subjects [and] [...] managed to touch the universal through the particular” (Boudon 2010, p. 11).

The attention Boudon and Merton paid to “universalism” as a governing ideal for scientific activity explains to a large extent why, from the early 1990s,<sup>20</sup> their correspondence conveys a sense of two men “converging” in the same intellectual battle against the different variations of scepticism and relativism that were prominent in the social and human sciences at the time. When Merton received from Boudon a copy of his *L’Art de se persuader des idées douteuses, fragiles ou fausses*<sup>21</sup> (1991), he was quick to share his enthusiasm and confidence about the future: “I haven’t the least doubt that the extravagances of radical cognitive relativism are time-bound and that they will be increasingly recognized for the self-deceptive and self-destructive opinions that they are. But it may speed up the process among our faddish tribe of sociologists to have calm analyses such as yours. [...] That we are on the same wavelength of course needs little further demonstration” (letter dated 2 March 1991). Nearly thirty years later, with various forms of relativism clearly receding, Merton’s confident optimism seems a clear case of the fabled self-fulfilling prophecy.

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19 Translated into English as *Sociology as Science. An Intellectual Autobiography*, by Peter Hamilton (The Bardwell Press, 2013).

20 Before the famed “science wars” triggered by the Sokal hoax in 1996.

21 Later translated into English as *The Art of Self-Persuasion: The Social Explanation of False Beliefs*, by Malcolm Slater (Polity Press, 1994).

This chapter is an initial exploration, based specifically on Boudon's correspondence with Paul Felix Lazarsfeld and Robert King Merton, of the Raymond Boudon archival collection. The correspondence of "intellectual friendships" between the three men forms but a subset of Boudon's far-reaching general correspondence, which included nearly nine hundred correspondents between 1961 and 2001. Using new materials, this study examines the variety of interactions between academic communities in France and the United States. More specifically, it highlights how, beyond their initial mentor-apprentice relationship, Boudon and Lazarsfeld acted as mediators for each other within their respective national academic spheres. Such mediators play a critical role in the international circulation of sociological ideas. From this perspective, there is little doubt that the significant dissemination of Boudon's works in the United States – evident in citation analyses referred to in this chapter's introduction – owes as much to their intrinsic qualities as to the attention paid to adapting and integrating them into the receiving academic environment.

Our analysis of the correspondence between Boudon, Lazarsfeld, and Merton also demonstrates how Boudon attempted to bring to France the qualities of a sociological scientific ethos embodied, in his view, by the Columbia duo – what he termed the "Merton-Lazarsfeld spirit": methodological rigour, an appreciation for sociological theorisation, and the determination to define sociology as a science that creates broader knowledge. Described in the preface to the English translation of one of his works as an "Unfrench sociologist," Raymond Boudon always claimed to feel he was part of an international academic community, and in doing so he kept his distance from the numerous ideological conflicts that drove French sociology during the 1970s and 1980s (Boudon 2013, p. 607). This correspondence shows how, for Raymond Boudon, the Columbia school was, from this perspective, a genuine resource, both strategically and intellectually.

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## NOTES ON THE AUTHORS

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## TYPES OF SOCIOLOGY

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Raymond Boudon, as Wikipedia notes, was a “French liberal sociologist”.<sup>1</sup> Concise as it is, this definition captures the essence of Boudon’s sociological profile – deeply in debt as much to the term *liberal* as to the term *French*. Boudon’s stance was more than simply analytical; it represented a political and moral commitment to the liberal tradition of thought, dedicated to protecting individual freedom of choice from external interferences. In the classical liberal tradition of thought, freedom means having the ability to do what one wants without interference and avoiding being compelled to do what one does not want to do. Accordingly, freedom is the absence of interference in one’s choices and will. Isaiah Berlin notably argued that in order to enjoy freedom of choice, each option has to be an “open door” that the agent can choose to enter or not according to her own wishes.<sup>2</sup> Boudon was arguably the least French of the great French sociologists. We might say he was an American in Paris. In his homeland, he always had to row against the tide. He was considered not abstract enough for the grand social theorists, too abstractly theoretical for quantitatively minded sociologists, not paying enough attention to the thick meaning of action for the qualitative ones, too focused on rational choice for micro-sociologists, insufficiently aligned with the weakest and subalterns in the eyes of critical thinkers.

This uncomfortable position is rooted in Boudon’s intellectual biography, as he himself acknowledged. Fascinated by the book by methodologist Paul Lazarsfeld, *The Language of Social Research*, which happened to fall into his

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*I am grateful to John Goldthorpe for his valuable comments and suggestions on an earlier draft.*

- 1 “Raymond Boudon,” *Wikimedia*, last modified April 13, 2025, [https://en.wikipedia.org/wiki/Raymond\\_Boudon](https://en.wikipedia.org/wiki/Raymond_Boudon).
- 2 “What Is Republicanism? A Conversation With Philip Pettit,” *Groupe d’Études Géopolitiques*, June 20, 2024, <https://geopolitique.eu/en/2024/06/20/what-is-republicanism-a-conversation-with-philip-pettit/>, accessed on July 7, 2025.

hands in the *Rue d'Ulm* library in Paris, Boudon decided to do an internship at Columbia University (Boudon 2001). There, he absorbed from Lazarsfeld a commitment to rigorous, empirical research on social issues, while Robert K. Merton instilled in him a theoretical orientation toward middle-range theories. Merton encouraged an analytical approach that bridged empirical data with theoretical insight. At Columbia, Boudon embraced a scientific ethos that remained with him throughout his career: theory and research should evolve in tandem, and the sociologist's principal role is to offer robust explanations and precise descriptions of social phenomena. A conception of sociology well mirrored in his own words: "C'est pourquoi sans doute, une fois devenu sociologue, je me suis arrimé *au principe que la sociologie n'est faite ni pour séduire ni pour influencer, mais pour éclairer*" (Boudon 2001, p. 28).<sup>3</sup>

## MAKING SOCIOLOGY MATTER

The explanatory aim of sociology is the focus of Boudon's succinct paper "Sociology that really matters". It is essential to recall the context of this article, specifically the *European Academy of Sociology's* (EAS) First Annual Lecture (Boudon 2002). To clarify its meaning, it is crucial to recall the EAS mission:

[...] the discipline's status has declined as well as its ability to attract talented students and faculty. To turn the tide, the sociological community must develop rigorous self-regulating standards that help the general public, policy makers, and prospective students to identify research and teaching programs of a high quality.<sup>4</sup>

"Sociology that Really Matters" is not just a paper; it serves as a cultural manifesto for the mission of the EAS, of which Boudon was the first President.<sup>5</sup> The paper counts only 168 citations,<sup>6</sup> while the Bent Flyvbjerg book – to which

3 Ironically, the critical orientation has increasingly dominated American sociology, aligning it more closely with the perspective from which Boudon sought to emancipate the discipline. Consequently, Boudon found himself both alienated from his own intellectual landscape and situated in a context abroad that gradually came to resemble the environment he originally aimed to transcend.

4 "European Academy of Sociology: Mission Statement", *European Academy of Sociology*, <http://www.european-academy-sociology.eu/mission-statement.html>, accessed on May 30, 2025.

5 Boudon's paper would be followed by John Goldthorpe's paper, "Sociology as Social Science and Cameral Sociology: Some Further Thoughts" (2004).

6 Checked on October 30, 2024 (Google Scholar), as for the other quotes and citation counts that follow this one.



Boudon critically responds from the very title of his lecture – has 10,006 (Flyvbjerg 2001). This is a recurring feature of Boudon's work: the analytical rigour of his scholarship has not been matched by a corresponding level of academic dissemination. In absolute terms, while being one of the most cited among European sociologists, Boudon is cited less than Bourdieu or Latour (see Ollion and Abbott 2016, fig. 3, p. 342).

Why such a gap? A tentative answer would be that, throughout his career, Boudon focused more on the theoretical foundations of the discipline, setting aside empirically oriented work. In doing so, he perhaps selected the wrong arena, or a dome with an unfit quality profile. The grand vocation of social theory is clearly at odds with Boudon's preferred analytical style (Van den Berg 1998). His relative marginality in the social theory debates is further evidenced by the stronger impact of his empirical work. For example, his key empirical research *Education, opportunity, and social inequality: Changing prospects in western society* has 5,768 citations, while his theoretical review paper on the so-called "cognitive rationality" model *Beyond rational choice theory* has 752 citations (Boudon 1974, 2003). A striking fact is the surprisingly low number of citations that Boudon's reply to Robert H. Hauser collects: 112 quotations for a piece that is – quite rightly – considered foundational in the mechanisms-based sociology approach (Boudon 1976; Hedström 2005; see also Manzo, in the Foreword of this book, § 2).

Substantively, the distinctive hallmark of Boudon's sociology refers to the development of explanatory, middle-range models connecting the micro and macro levels of social life. This is crystal clear in the fourfold classification that Boudon outlines in his paper, "Sociology that really matters" (Boudon 2002, pp. 371–378):

- Expressive or aesthetic sociology: a style that brings about emotions in the reader and mobilises an empathic understanding that resonates with her subjective experience. This is a sociological style that adopts the canon of literary works, certainly inspiring and useful in many ways, as Boudon himself recognises, but not a scientific way to explain the social world.
- Critical or committed sociology: a style that judges social arrangements focusing on power relations, domination, and exploitation. The Marxist tradition, the Frankfurt school, and the critical theory in its many forms are the most representative streams of this type. Its success depends on specific political conjunctures and resonates with the agenda of social movements, parties, and mass media. A militant sociological approach, driven by political objectives that steer sociology toward achieving social justice goals.
- Cameral or descriptive sociology: service sociology with descriptive or policy purposes. Its key aim is to inform public policy and provide

knowledge to decision-makers, while delivering robust descriptions of various phenomena of public interest, such as inequalities, poverty, and health determinants.

- Cognitive or scientific sociology: aims to explain different kinds of puzzling phenomena with the aid of explicit micro-founded theoretical models. This is the style that Boudon favours over the other three, and he places it at a considerable distance from the first two styles, which he considers as unscientific.

The four types share “fuzzy” boundaries, a point that Boudon notes *en passant* without going into the details: “The genres I have distinguished are ideal types and the borderlines between them are in reality sometimes fuzzy” (2002, p. 376). Walking on fuzzy boundaries is risky, and it should be done with great caution. To begin with, we should avoid over-emphasising fuzziness as an excuse for imprecision; rather, we must recognise that fuzziness does not necessarily obstruct the pursuit of precision, a guiding principle of Boudon’s work. As Amartya Sen remarked:

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I believe that boundary questions are sometimes taken to be more important than they are. Intellectual interest in these issues may distract attention from the fact that imprecision of boundaries can still leave vast regions without ambiguity. It is indeed possible to say a good deal about China and India without asserting that there are no ambiguities as to where the boundary between the two countries lies (Sen 1980).

At the same time, it would be misguided to obscure the problem of ambiguity with a precision that, instead of clarifying problems, only serves to get rid of the ambiguity. As Sen again states: “if an underlying idea has an essential ambiguity, a precise formulation of that idea must try to capture that ambiguity rather than attempt to lose it” (Sen 1992, p. 75).<sup>7</sup> This is the narrow path that must be carefully followed to avoid fuzzy borders.

In what follows, I will contend that Boudon’s scientific or cognitive sociology holds a significant advantage over the other three types when addressing the questions of *why* sociology and *how* to conduct sociology. However, it appears less robust concerning the *what about* sociology questions, specifically regarding the rationale behind the problem of interest. I will first maintain – in the same line as John Goldthorpe’s argument – that while this

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7 As in “On Rigor in Science”, where Jorge Luis Borges disclosed a project that was both ridiculous and useless, the concept of overly precise scientific maps, often interpreted as a commentary on the limits of representation (Borges 1975).

issue finds a compelling solution in the complementary relationships between scientific sociology and cameral/descriptive sociology, the resulting proposal still overlooks some important aspects related to the “problem finding” issue.

## WHY, HOW, WHAT SOCIOLOGY?

About *why* sociology and *how* to conduct sociology Boudon’s position is straightforward. The *why* of sociology lies in the goal of explaining puzzling phenomena. Accordingly, the *how* of sociology refers to the micro-founded explanations of macro-level phenomena, namely to the theoretical design of explanatory models able to make sense of macro-micro-macro processes and outcomes. The *how* of sociology relies on the postulate of methodological individualism, which interprets social phenomena as generated by a combination of individual actions in a macro-micro-macro multilevel schema. This can be formalized in the following way:<sup>8</sup>

Let us assume the existence of any social or economic phenomenon  $M$ , for which an explanation is sought.  $M$  is interpreted as a function  $M(m_i)$  of a range of individual actions  $m_i$ , which themselves are [...] functions  $m_i(S_i)$  of structure  $S_i$  of the situation including the social agents or actors. [...] As for structure  $S_i$ , it is a function  $S_i(M'')$  of a range  $M''$  of defined data at a macrosocial level [...]. Explaining  $M_i$  means, in brief and in terms of the general paradigm, saying exactly what the terms of  $M = M\{m[S[M'']]\}$  are (we can express it more simply as  $MmSM''$ ) (Boudon 1986, p. 194).

How does the third issue relate to the *what about* of sociology? Namely, on what basis do sociologists select their topic of interest? The answer – that sociology studies society – is not acceptable in Boudon’s view, since society is a concept without an empirical referent, and a general theory of social order is a misplaced ambition. In his view, sociology addresses macro-level occurrences of various kinds, such as inequality of educational opportunities, patterns of social mobility, the persistence of magical thinking, the American religious exception, the tendency of democracies to evolve into oligarchies, the secularisation of religious beliefs, the gradual disappearance of moral taboos, and the establishment of the cult of human rights. As it emerges from the

8 This is the backbone of the analytical sociology research program (see Manzo 2021; Hedström 2005). I will not recall here the strengths and weaknesses of the macro-micro-macro canonical view that analytical sociology brings about (see Barbera and Negri 2015, 2021; Barbera 2021).

previous list, Boudon considers as legitimate macro-level *explananda* not just population-like phenomena, namely social phenomena made of entities with variable properties that exhibit aggregate-level regularities of a probabilistic kind. In this latter perspective, *why* questions such as “Why did President Chirac call early elections in 1997, only to lose his majority in parliament?” would not constitute suitable *explananda* for sociology (Goldthorpe 2016). On the contrary, according to Boudon’s view, non-probabilistic phenomena such as the triggering of the fall of the Soviet Empire, the dynamic of revolutions, the topic of regime changes, the mechanisms of scientific discoveries, and so on, are perfectly legitimate macro-level problems to explain.

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The question of “What about?” to study is tackled through a Weberian lens that emphasises the relationship to values and their significance in socio-historical research. For Weber (1949), the selection of the research problem is conditioned by values, but this does not hinder the possibility of objective knowledge. While the *selection* of the research problem is guided by the researcher’s criteria of value relevance, the answer to the research question must follow the rules of the scientific method and must be value-free. The researcher’s values make it possible to select that part of empirical reality that deserves, in the researcher’s subjective judgment, to be considered as relevant. This is the solution advocated by the neo-Kantian German philosopher Heinrich Rickert, whose methodological writings inspired Weber. In contrast to Rickert, however, Weber recognised the fundamental irreconcilability of different value spheres and the impossibility of building a “rank-order” science of values (Bruun 2001). Weber in “Science as a Vocation” reflects on the inherent conflicts between values and the “polytheism” of modernity, discussing how certain values may be revered not for their beauty or goodness but precisely due to the complex, sometimes contradictory nature of what society holds as sacred, beautiful, or truthful (Weber 1946, pp. 382–394). Polytheism does not imply relativism, however. The rational clarification of these values and their implications is possible: “Weber [...] argued that any value judgement can be rationally appraised in terms of whether it has been logically derived from a coherent set of fundamental values and whether the factual assumptions on which it relies are sound (Hammersley 2024, p. 90). Boudon’s view on the matter is fundamentally Weberian or, at least, in line with this interpretation of Weber’s position (Boudon 2014, 2017, 2000).

The so-called Weber’s “Nervi fragment”<sup>9</sup> offers some novel insights in this line, which may help to shed a different light on this thorny issue. In a world

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9 The so-called “Nervi fragment” was published for the first time by Hans Henrik Bruun (2001). See also Massimilla (2011). The fragment dates to the 1903, when Max

inhabited by dissonant polytheism, Weber asks, how do we know what is “worth knowing about” (*Wissenwerth*)? How can we establish the foundations for the *what* of sociology? Weber’s answer looks at those criteria of value that have cultural meaning and general significance for the historical time we are living in. This cultural meaning does not depend only on individual relevance criteria, nor on the puzzling character of the problem matter at hand. The “Nervi fragment” offers some key arguments precisely in this connection (Bruun 2001). The selection of the problems “worth knowing”<sup>10</sup> – Weber goes on in the “Fragment” – must satisfy the interests of the historian’s public, which in their turn may have a near infinity of causes. “Value” (*Wert*), Weber makes it clear, does not mean anything more than “worth knowing” (*Wissenswerth*). Given that establishing a rank-order of criteria regarding what constitutes “worth knowing” is an untenable metaphysical assertion, the choice of *more or less* pressing elements or problems of interest must rely on a “principle of economy” (Bruun 2001). This principle entails prioritising the most urgent needs of the public in relation to the most compelling research interests of historians. In other words, the selection process is neither shaped by a hierarchy of knowledge nor grounded on normative foundations, but rather depends on the immediate concerns of the “public” while *remaining aligned with scholarly pursuits*. To sum up: “In Weber’s eyes, what ‘history’, in Rickert’s broad sense, should select as its subject matter depends on the interests of the historian’s public – and of those of the historian himself. These interests *vary* greatly over time and between individuals” (Bruun 2001, p. 149).

In light of the “Nervi fragment”, the relevance of the problem matter should not be addressed only in connection with the researcher’s individual relevance criteria or the puzzling nature of the phenomena to be analysed. This would not consider the role of the audience(s) (“the publics”, in Weber’s parlance) and that of the *general significance* of the *historical time* that, through multiple causes, shapes the urgency and graduation of the “what is worth knowing”. To address this issue, I will first argue that the “What about?” question is best examined at the intersection of cognitive and cameral sociology. Second, I will revisit a lesser-known contribution by Boudon, which offers a perspective distinct from that presented in his EAS lecture.

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Weber was recovering in Nervi (Riviera ligure, Italy) from a nervous breakdown. The fragment discusses the notion of “value” and that of “value relation” employed by Rickert.

<sup>10</sup> Socio-historical disciplines are in Weber not neatly separable, so when he writes “historians” we can safely read “sociologists”.

## TO WHOM DO SOCIOLOGISTS SPEAK?

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The distinction between scientific sociology and cameral sociology in the third EAS lecture,<sup>11</sup> where he compares the relative merits of cameral sociology against those of cognitive sociology (Goldthorpe 2004). The key passage is: “While all sociological problems will entail puzzlement, the mere fact that you or I, individually, may find some social phenomenon to be puzzling is not in itself sufficient grounds for claiming that a serious sociological problem exists” (Goldthorpe 2004, p. 100). Goldthorpe, following R. K. Merton (1959), goes on to maintain that the justification for a problem may be connected to either knowledge or practice. In other words, this refers to the intrinsic worth of knowledge, as well as its potential to serve purposes beyond itself – the application of knowledge can make meaningful contributions. While Merton recognises that the existing equilibrium between fundamental and applied research within any discipline may generate concerns, he primarily emphasises their essential *interdependence*. He argues that, particularly in sociology, a well-defined problem typically embodies what he refers to as a “double rationale”. In his work, at the same time, Merton warns that the urgency or magnitude of a practical social problem does not ensure its immediate solution: “necessity is only mother of invention; socially accumulated knowledge is its father” (Merton 1968, p. 50). This notion highlights that in advancing the field sociological inquiries need to *simultaneously* address theoretical questions of middle-range *and* practical issues.

To this end, Goldthorpe emphasises that the descriptive power of cameral sociology serves as a crucial tool. First, the primarily descriptive role of cameral sociology offers scientific sociology a substantial reservoir of systematic data to help define the phenomenon to be explained (Merton 1987). This reveals the existence of the more or less complex social regularities that it is the task of scientific sociology to explain. Furthermore, description can itself foster the development of theory when grounded in advanced empirical observation and research methods. This is in the spirit of middle-range sociology, where

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11 The EAS statement reads as follows: “The European Academy of Sociology is a fellowship of respected scholars with expertise in many different areas of sociology, united around the common concern to promote rigorous standards in sociology. The European integration necessitates the development of common standards of excellence, via various bodies of private and governmental evaluation and accreditation. The Academy provides a forum for the formulation of minimum requirements and its fellows are willing to offer their services for international bodies of accreditation and evaluation”: “European Academy of Sociology: Mission Statement”, *European Academy of Sociology*, <http://www.european-academy-sociology.eu/mission-statement.html>, accessed May 30, 2025.

theory, method, and research build on each other's advancement rather than following separate paths. Goldthorpe, drawing on Karl Popper (1957), points out that cameral/policy sociology provides valuable resources for theoretically informed gradualist, rather than "utopian," approaches to social engineering. This is because "application" serves as an important experiment or "quasi-experiment" contributing to the advancement of social scientific understanding through an empirical test of the theory. This supports the idea that sociologists who advocate for a scientific approach *à la* Boudon should be willing to engage directly with social engineering efforts,<sup>12</sup> even if it means confronting the technical and political complexities of real-world applications. This is a key point, which I will return to at the end of this essay.

The relationship between cognitive sociology and cameral sociology thus helps the search for a better balance between the *social* and the *sociological* relevance of the problem. This is key, Goldthorpe goes on, in light of the idea that a kind of social contract exists between the social sciences and society, where society provides resources for the production of sociological knowledge and research provides usable knowledge of some sort. Besides teaching and research, the two main pillars, the social contract between the social sciences and society, asks for the design and application of "solutions". This model, known as "use-inspired research", is in the so-called "Pasteur quadrant" (Stokes 2011). The Pasteur quadrant is named in honour of Louis Pasteur, whose scientific work simultaneously laid the foundations for modern microbiology and helped address important application problems in agriculture and animal husbandry. If research produces only an advance in basic knowledge, we are in the quadrant known as the "Bohr quadrant," in honor of Niels Bohr, a theoretical physicist considered the founder of quantum theory.<sup>13</sup> The purely applicative model or "Edison model" is named in honour of Thomas Edison and involves an engineered solution to a problem (for example, using an incandescent lamp to provide lighting), without also advancing basic knowledge (electromagnetism).

In summary, in his EAS lecture, Boudon leaves the *what* to the researcher's individual relevance criteria and to the puzzling nature of the phenomena of interest. Cameral sociology partly corrects this by introducing a productive tension between the *social* and the *sociological* relevance of the problem

<sup>12</sup> To make it clear, I am thinking here of a "solutions-oriented" sociology well beyond public policies or "service sociology". I will briefly address this point in the final remarks.

<sup>13</sup> Of course, basic research has indirect consequences for possible applications (in the case of quantum physics: lasers and magnetic resonance imaging), but it is not designed from the outset to produce these practical outcomes.



matter. In doing so, in an entirely unintentional way, I would add, it opens up to another proposal about the different “types of sociology”, that of Michael Burawoy (2005).<sup>14</sup> This proposal identifies two dimensions and four types:

1. Professional sociology (academic public, instrumental knowledge).
2. Policy sociology (external public, instrumental knowledge).
3. Public sociology (external public, reflexive knowledge. Sub-type: organic public sociology, when the sociologist works closely with a visible, “dense,” local, and often antagonistic public).
4. Critical sociology (academic public, reflexive knowledge).

Burawoy asks: To whom does sociology speak? What kind of knowledge does it produce? (Burawoy 2005), pp. 4-28). Regarding the first question,<sup>15</sup> both Burawoy and Boudon appear to recognise different audiences. This point is clearly addressed by Boudon (1981) in an older contribution where he distinguishes three audiences or “publics” to whom the intellectuals can address themselves. Type I public is based on “peer judgment” and it consists of the scientific community. Type II public is characterised by an “appeal to a broader audience,” composed of groups beyond the scholarly knowledge of the specific domain. Type III public is that of the “diffuse market.” Here, the intellectual no longer addresses the peers or some key specific groups but rather the “broader public opinion” (Boudon 1981). For both Burawoy and Boudon, therefore, sociology talks to several possible audiences beyond academia, such as social movements, unions, political parties, civic organisations, territorial communities, professional associations, complex organisations, and firms. It also addresses the expert knowledge of journalists, media professionals, and technologists, as well as talking to ordinary individuals in their daily lives.

The position Boudon endorses in his 1981 paper is quite different from the one he defends in his EAS lecture, where he seems more sceptical about the integration of the different sociological styles in relation to different kinds of audiences. In the 1981 paper, he cites Michel Crozier, who wrote some books for Public I, the global academic community, and others for Public II, consisting of the French political, economic, and cultural spheres. He even cites Michel Foucault, who, in some of his works, simultaneously engaged Public I, while addressing Public II’s professionals of the prison and asylum

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<sup>14</sup> Without going into the details of Burawoy’s well-known typology, I emphasise only two points. The fundamenta of the typology are more defined and the paper from which it originated had a much wider circulation than Boudon’s (3977 quotations, Google Scholar).

<sup>15</sup> I shall consider the second dimension (instrumental vs. reflexive knowledge) in the final remarks.



systems, and even Public III, as public opinion became inflamed by the issue of “confinement”. His judgement of Foucault is much more trenchant in his EAS lecture (Boudon 2002, p. 377).

In the EAS lecture, Boudon is much more sceptical. For instance, expressive sociology, he states, successfully speaks to outside audiences because it adopts an *essayistic style* that resonates with everyday life and that feeds the need for meaning of lay members of society. One of Boudon’s examples is the work of Erving Goffman: “His appeal seemed to lie, *not in his scientific merit* but in his literary powers. He won his audience through his powerful descriptions of the hypocrisy of social life, and his books sold in numbers more typical of literary *than scientific work*” (Boudon 2002, p. 372, emphasis added). I consider this as a misplaced case-in-point. Some of Goffman’s contributions might actually fall into this category (notably *Asylum* and *Stigma*), but many others – I would say the majority – do not. I wonder how one would ever feel any kind of emphatic understanding while reading *Frame Analysis*. Similarly, I fail to see any kind of literary and expressive canon in Goffman’s *Interaction Ritual*.

As Boudon himself acknowledged in his 1981 paper, *essayism* has two rather different faces. The first face is a footloose or “unconstrained” type, built just on literary style and expressive – if not seductive – evocations and storytelling. Novelists have far greater success than sociologists in this regard, especially when daily life is concerned (as readers of Rachel Cusk know well). The second is what we might refer to as “constrained essayism”, which *binds* itself to the insights of social research and translates them for a plurality of audiences beyond scholars and policymakers. I am thinking here of the public success that economists have had with this kind of constrained essayism, from Thomas Piketty, to Mariana Mazzucato, to Angus Deaton, to Joseph Stiglitz, to Amartya Sen, to Tony Atkinson, to Kate Raworth.<sup>16</sup> Not to mention anthropologists (David Graeber), urban planners (Carlos Moreno), and psychologists (Jonathan Haidt). Public essayism has certainly a world-making quality (Savage 2024), but – as aptly noted by John Goldthorpe (Goldthorpe, forthcoming) – only if supported by rock-solid descriptions and sound explanations. Constrained essayism of this kind enhances the public value of the discipline and helps to fight the pernicious Gresham’s law of public communication, whereby in the media unconstrained essayism drives out the constrained one. We should never underestimate the power of a bad idea. Not all kinds of sociological research lend themselves to constrained essayism,

<sup>16</sup> A further implication is that “sociological correspondents”, comparable to the “economics correspondents” in the media, are rarer, especially if well-trained in the discipline.

however. Only those researchers that have addressed – as in the case of cameral sociology – topics equipped with *social* and not just *sociological* relevance might attempt to pursue this aim.

The kind of constrained essays of social sciences *other than sociology* seems to have this point much clearer. Economists, anthropologists, psychologists, political scientists, and historians do rigorous research on “wicked problems” or “societal challenges”, namely those problems that resist solutions and that involve complex, multidimensional societal challenges on different scales and levels.<sup>17</sup> They then translate their findings into cultural products for a wider audience and a variety of publics. It is entirely possible, and indeed urgent, for analytically-oriented sociologists to adopt this approach: namely, to work with middle-range theories, models, and mechanisms on wicked problems and societal challenges of *general concerns* for a *variety of publics* and audiences.<sup>18</sup>

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This stance should not be confused with the most radical form of public sociology from Burawoy’s typology, which aligns with Boudon’s critical sociology – namely, organic public sociology. I do not defend the idea that the selection of the research problem should be done on the basis of a *political positioning* of the discipline in privileged, if not exclusive, contact with a visible, dense, local, and often antagonistic public of a leftist kind. This answer would radically wipe out the *sociological* dimension in favour of the *social* one. This is a weak response and one that harms both the scientific and public vocation of sociology. How many sociologists have been drawn away from the world of politics due to a committed call of organic public sociology? And with what consequences for the quality of the political class? And how many promising, budding sociologists have been driven away by the discipline’s overly normative stance, only to end up in the fields of economics or demography? I thus concur with Turner, who wrote that if opting for organic public sociology: “Sociology will no longer be considered a science worthy of much attention inside and outside of academia, except by students hungry for a critical approach to the study of society” (Turner 2019). Turner warns that if sociology positions itself primarily as a political project rather than a scientific endeavor, it risks forfeiting its ability to apply its rich body of knowledge to address the practical challenges faced by various organizations (Turner 2019). Moreover, to complement this

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17 Solving these problems is not conceivable through a technocratic, top-down approach, but calls for consensus-building mechanisms, trust and legitimacy to innovate in conditions of radical uncertainty: see Alford and Head (2017).

18 For example, nearly three decades ago, Gramling and Freudenberg (1996) urged that greater attention be given to middle-range efforts in environmental sociology. However, environmental sociology’s contributions have often been overlooked in favor of the ongoing quest for overarching, grand theories (Hannigan 2024).

point, whoever defends the organic public sociology posture has to be ready to accept that the politicisation of sociology could be taken over by rightwing scholars. Leftist scholars should not delude themselves into thinking they can effortlessly maintain a monopoly over a politicised field.

## CONCLUSIONS

I argued that a journey at the fuzzy intersections of Boudon's types of sociology might help to make sense of the double rationale of social research, as illustrated by Goldthorpe in his third EAS lecture. I then maintained that this highlights the relevance of multiple audiences in selecting the research problem to address, as in Weber's "Nervi's fragment" and in connection to Burawoy's typology. I emphasised a difference between Boudon's EAS lecture and his earlier work. This difference should not be overstated, however, for in both papers Boudon argues that the diversity of sociology audiences lies fundamentally in the demand for different kinds of sociology. Accordingly, my educated guess is that, in the '80s, he still believed that this demand was balanced, and allowed the co-existence and perhaps synergy of these different sociologies. In the final pages of his EAS lecture, a different tone or feeling seems to emerge – perhaps, justifiably so, for what Boudon might call "good reasons", invoking one of his preferred analytical categories that refers to the rational grounds individuals may have for their beliefs or actions, even when these do not align with objective truth. Although I believe that the posture taken in the EAS lecture is not the most useful for "making sociology matter", I have argued that *organic* public sociology is not the approach to follow either.

To conclude, if we are to enhance the public value of sociology, I see no reason – unlike Burawoy – to equate policy sociology with instrumental knowledge for policymakers, and public sociology with reflexive knowledge for ordinary people and social movements. Sociology can provide both instrumental *and* reflexive knowledge on public problems *and* policy issues. I would therefore defend the idea that sociologists can provide applied solutions<sup>19</sup> or "piecemeal social engineering" of various kinds that reflexively enhance the public value of the discipline while being engaged in *solving real-world wicked problems* (Goldthorpe 2004, p. 99). For instance, Manzo and de Rijt (2020) show how targeting "hubs" robustly improves containment of SARS-CoV-2, while Sabel

19 A solutions-oriented sociology has key analytical consequences also for professional sociology. As Watts (2017) argued, one possible way out from the theoretical incoherency problem of sociology is to reject the traditional distinction between basic and applied science.

and Victor illustrate that an experimentalist approach can effectively meet the challenge of climate change. I would argue that pursuing this approach would strengthen the public legitimacy of the discipline far more effectively than relying solely on narratives shaped by the *Zeitgeist* or spirit of the times. Providing means-end solutions – while reflexively eliciting different reactions to current problems to generate novel possibilities of action – is a rather different endeavour than narratively buffering meaning that resonates with mundane experience and collective concerns. Again, this kind of solutions-oriented social science falls into Pasteur’s quadrant: use-inspired research that advances fundamental understanding and is distinct from Bohr’s quadrant (traditional basic research) and Edison’s quadrant (traditional applied research). A solutions-oriented sociology of this kind would help social science to be more visibly useful to the world, thereby improving its status with an increasingly sceptical public (Watts 2017).

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In parallel, I would defend the idea that policy sociology greatly benefits from promoting reflexivity in public policies and, more broadly, in the operational design and management of applied solutions. This approach broadens – both analytically operationally – the perimeter, scope, actors, viewpoints, interests and quality conventions at stake in the policy domain or the substantive process or outcome of interest. As Sandro Busso (2023, p. 260; see also Barbera, 2025) notes, this is a task that concerns “the perimeter of actors involved and with their public role, and consists in creating the conditions for the recognition of a *plurality of voices*, including that of the poor” (emphasis added). Reflexive and instrumental knowledge are tightly intertwined here, as deliberate attempts may be made to subvert or counter the intervention by individuals who see it as being contrary to their interests and objectives (Goldthorpe, forthcoming). Resistance to applied interventions can gradually manifest as organised dissent and conflict at the political level concerning priorities and goals, thus questioning what is worthwhile and eventually nurturing visions and aspirations of marginal groups while squeezing those belonging to vested interests. Sociology is crucial in analysing the potential for such developments and understanding the limitations that may affect applied knowledge, whether in public policies or other settings. From this standpoint, sociology can provide insights into the interplay between piecemeal solutions, theoretical advancements and broader political discourse.

I have argued that a discussion of the different kinds of sociology forces us to consider the importance of middle-range theories applied to research problems of public relevance, the selection and identification of which calls into question the public value of sociology. Such middle-range theories must deal with a multiplicity of target audiences and with *their* relevance principles.

This orientation requires a multiplicity of criteria to assess the merits of different types of sociology. These criteria may not be commensurable with each other and, therefore, may impede a hierarchical ordering of the different types of sociology in terms of their distance from some benchmark, as Boudon seemed to do in this EAS lecture and quite differently from his 1981 paper. That is, the relevance criteria for different types of sociology are heterarchical and do not adhere to a single metric (Stark 2011). Appreciating the *Mona Lisa* in a particular way does not help one choose between a Dalí or a Picasso. This requires the professional habit of thinking with criteria that can only partially order the world. The ambiguity that follows should not be frightening and push for misplaced precision. To describe the operational situation of a hunter running after a rabbit, a blurred picture of a rabbit in motion is more realistic than a picture in focus, but with the rabbit stationary.

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PART II

**THINKING BY SOCIAL MECHANISMS**



## GENERATIVE MODELS, ACTION THEORIES, AND ANALYTICAL SOCIOLOGY

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Jon Elster (e.g., 1989) repeatedly emphasized that the social sciences are essentially grappling with two core questions:

1. Why do individuals do what they do?
2. What do individuals collectively bring about when they do what they do?

These two questions are also at the core of analytical sociology, a sociological tradition that Raymond Boudon had a considerable influence upon (see Hedström and Swedberg 1998b; Hedström 2005). Boudon developed persuasive arguments regarding how we ought to go about answering these questions.

I believe Boudon's most distinctive contributions in this respect are the following:

1. His view that sociological explanations should be actor rather than factor-based.
2. His generative and mechanism-oriented view of explanations.
3. His view that individual reasons is the proper "rock bottom" for sociological explanations.

In this chapter, I address each of these points, and I am very much in line with Boudon as far as the first two points are concerned. Thereafter, I present some general reflections on how his work relates to current-day analytical sociology.

### ACTORS AND FACTORS

In his 1974 book on education and inequality, Boudon used simulations to try to make sense of several "paradoxes" reported in the social mobility literature. He argued that an important distinction should be made between statistical and theoretical models, and that theoretical models are needed to explain the results of empirical analyses. In order to explain, Boudon argued, "we must go beyond the statistical relationships to explore the generative

mechanism responsible for them” (1976, p. 117), and further, to use Macy and Willer’s (2002) apt expression, that actor-based rather than factor-based explanations are the proper way forward. As Boudon (1974) expressed it:

To pursue this line [of research] requires that men not be considered as ... a set of juxtaposed variables, but that they be seen as actors, able and willing to take decisions depending on their resources and on the context.

The centrality assigned to actors leads us over to the second, and closely related area concerning generative models and explanations that seek to show how the activities of actors bring about or generate the macro-outcome to be explained.

## GENERATIVE MODELS AND EXPLANATIONS

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Boudon succinctly summarized his Weberian-inspired explanatory strategy with the following expression:

$$M = M(m[S(M')]).$$

What he meant was that a social phenomenon,  $M$ , should be explained as a function,  $M$ , of actions,  $m$ . These actions, in turn, should be explained with reference to the social situation,  $S$ , in which the actions take place, and these social situations, in turn, should be explained with reference to yet another social phenomena,  $M'$ , and the actions that brought them about (see Boudon 1986). This perspective is similar to Coleman’s view as expressed in his so-called micro-macro graph (Coleman 1986). The similarities between Boudon’s and Coleman’s approaches are evident in the following quote where Coleman lays out the dynamic recursive nature of his approach:

Structure at one time (macro-level) generates the conditions which together with existing interests shape the actions of the actors (micro-level) that jointly produce outcomes which modify the structure of a later time (macro-level) which generates conditions that again (through constraints and incentives) shape action (micro-level) that jointly produce outcomes (macro-level) and so on (cited from Manzo 2014, p. 19).

Boudon’s emphasis on the social situation ( $S$ ) as the mediator between macro and micro phenomena also highlights the close alignment between his approach and Popper’s concept of situational analysis (see Hedström, Swedberg, and Udehn 1998).

The micro-macro link was thus of fundamental importance to Boudon. He argued that proper explanations of social outcomes must demonstrate how

these outcomes are generated by the actions of relevant individuals. To illustrate what he meant with a generating model, he referred to Schelling's (1971) segregation model as an example, and he described the type of theoretical model he envisioned as follows:

At a very general level, a generating theory can be typically described as a theory containing two logical core elements: (1) a description of the logic postulated to regulate the actions of the individuals observed ... and (2) a description of the social constraints within which the logic of individual actions develops (Boudon 1979, p. 60).

And he continued:

In a generating model, individual actions are *aggregated*: the outcome of this aggregation depends on the individual logic of action or behavior ... and on the ... social context within which individuals act. (Boudon 1979, p. 62)

That is, to explain an aggregate outcome, a generating model is built that shows how actors, constrained and enabled by their social contexts, in interaction with one another generate the outcome to be explained.

Boudon also did pioneering work on how to classify and distinguish between different types of social processes and the generative models that produce them (c.f., Boudon 1979, 1982). He emphasized the significance of interdependent systems and feedback loops, highlighting the need to carefully consider where such feedback loops originate and where they end – whether within the system of interaction itself or in the broader social environment.

With this generative view of explanations, Boudon placed himself in a tradition that includes the likes of James Coleman and Aage Sørensen, and many present-day analytical sociologists.<sup>1</sup> Coleman described one important aspect of this tradition as follows:

The general approach will be (1) to begin with the idea of a process, (2) to attempt to lay out the mathematical model that mirrors this process, and then (3) given particular kinds of data ... estimating parameters of the process. In general the goal will not be one of testing hypotheses but rather one of estimating parameters in a mathematical model designed to mirror a substantive process (Coleman 1981, p. 5).

Similarly, Aage Sørensen emphasized that adequate explanations must specify plausible models of social processes through which outcomes are generated. He is best known for his *vacancy competition model* (e.g., Sørensen

<sup>1</sup> Possibly one should refer to this tradition as “the Coleman, Sørensen, Fararo tradition” (see Manzo 2024).

1977), a differential equation model that links gains in labor market attainment to individuals' resources and mobility opportunities, which are themselves shaped by the rate at which vacancies are created.

In the Coleman-Sørensen tradition, empirical data is not primarily used for testing hypotheses but for developing realistic *substantive* models of the processes believed to have brought about the outcome to be explained, and this is done by empirically estimating the parameters of the substantive model.

As the reference to Schelling's segregation model suggests, the generating models Boudon had in mind were not differential equation models like those of Coleman and Sørensen but were more in line with the type of agent-based models (e.g., Macy and Willer 2002, Manzo 2022) commonly used today. However, the role of empirical data remains the same: it serves as a means of empirically calibrating a substantive model rather than performing hypothesis tests, which are the primary focus of many statistically oriented sociologists. That said, hypothesis tests and traditional statistical models can still be valuable for estimating the parameters of substantive models. Once these parameter values are arrived at, the model can be used for counterfactual what-if analysis. Further, if the substantive model is properly calibrated, these counterfactual analyses can provide important insights into what is likely to happen if we were to make different kinds of interventions in the real world.

In the 1960s and 1970s, Boudon showed significant interest in formal theorizing, emphasizing the explanatory importance of building models that demonstrate how individuals, through their interactions, generate collective outcomes (e.g., Boudon 1979). However, like Coleman, he did not provide concrete guidance on how such micro-macro modeling should be done in practice. In his later work, Boudon's focus shifted toward more discursive and less formal approaches, concentrating on conceptualizing action rather than exploring how generative models could link micro and macro phenomena (see also Manzo 2012).

## ARE REASONS THE END OF THE STORY?

As mentioned earlier, a core component of Boudon's type of generative model is a model of "the logic postulated to regulate the actions of the individuals observed" (Boudon 1979, p. 60). In numerous publications, he elaborated on such models with the aim of addressing what he perceived to be serious weaknesses of traditional rational-choice theories.

Boudon positioned himself firmly within the rational-choice tradition but argued for a broader conception of rationality. He contended that "to get a satisfactory theory of rationality, one has to accept the idea that rationality

is not exclusively instrumental: it also has an axiological dimension and a cognitive one. ... The reasons motivating an actor can be strong without belonging to the instrumental species" (Boudon 1998, pp. 199-200). In other words, Boudon believed that our models of the actors should assume that they act rationally in the sense of having good reasons for their actions, even if those reasons reflect what Elster (1989) referred to as irrational beliefs.

In my view, Boudon's attempt to develop a new type of action theory was not as successful as other parts of his work. Nevertheless, the sheer volume of his writings on this topic suggests that he considered it highly significant. One way to make sense of his persistent efforts to develop a reason-based action theory is his apparent belief that reason-based explanations represent a kind of rock-bottom explanation for sociology. Echoing Hollis's (1977) claim that "rational action is its own explanation," Boudon argued that "when a sociological phenomenon is made the outcome of individual reasons, one does not need to ask further questions." The explanation is "final" (Boudon 1998, p. 177).

Boudon further argued that traditional rational-choice theory was inadequate because it struggled to account for beliefs and desires and relied on what he saw as ad hoc black boxes, such as risk aversion and cognitive biases. In response, he set out to develop an alternative model, free from such black boxes, which he called the *Cognitivist Model*. I will not delve into the details of Boudon's cognitivist model here, as it is discussed in other chapters of this book. Instead, I focus on his thesis regarding the "rock-bottom" (Watkins 1957) nature of reason-based explanations, a position I find difficult to accept.

I can see some merit in Boudon's view if our goal is to explain why a specific individual did *X*. If that person tells us, "I did *X* because of reason *R*," there is little reason to doubt this explanation – provided *R* is a plausible motivation for doing *X* and there is no evidence suggesting the individual is being deceptive.

I find Boudon's position much harder to accept in the following social-science scenario. Imagine a group of men asked to make hypothetical choices about lifelong partners. All participants offered well-articulated reasons for their choices. However, it turns out that higher educated and less educated men systematically differed from one another: all the higher educated men based their choices on reason *R*<sub>1</sub>, while all the less educated men based theirs on reason *R*<sub>2</sub>. While knowing these reasons can be informative, they do not constitute a rock-bottom explanation. The observed difference in reasons poses a puzzle that demands further scrutiny, directly opposing Boudon's principle that "when a sociological phenomenon is made the outcome of individual reasons, one does not need to ask further questions."

Opportunity-based differences present similar challenges to Boudon's thesis. Continuing with the same example, suppose there are not enough women in

the relevant geographical area for every low-educated man to find a partner. As a result, some of these men would live alone while others would have partners, even though they all shared the same reason, *R*<sub>2</sub>. Once again, while knowing the individuals' reasons can be informative, it does not constitute a rock-bottom explanation. The observed behavioral differences among the low-educated men would call for further investigation.

When such heterogeneities are present – which is the norm rather than the exception in the social sciences – Boudon's central thesis must be questioned. While knowing individuals' reasons can be valuable for developing social-science explanations, it is rarely sufficient. These examples suggest that contrary to Boudon's claim, reason-based explanations are rarely final in his sense of the term. It also follows that they do not hold the privileged status he ascribed to them.

## THE FIRST AND THE SECOND-GENERATION ANALYTICAL SOCIOLOGISTS

Analytical sociology is committed to the principle that theories and explanations should be formulated in terms of the processes believed to have genuinely generated the phenomena of interest. This principle assigns a crucial role to individual behavior, as it is the driving force behind the social processes that produce social change.

As I suggested in Hedström (2005), the causal significance of individual actions becomes evident if we imagine a counterfactual scenario in which we can press a pause button that freezes all individuals, preventing them from acting further. All social processes then would come to an immediate halt. Therefore, our explanations must, in some way, reference individuals' behaviors – how they unfold over time and gradually bring about the macro-level outcomes to be explained. Boudon was in full agreement with this.

The specific ways in which individual activities, actions, or behaviors are incorporated into sociological explanations vary considerably. Social scientists differ in how deeply they believe the micro-level analysis must go to provide an acceptable explanation of a macro-outcome. While analytical sociologists agree that macro-explanations must be anchored in individual behavior, they disagree on whether this behavior itself requires further explanation and, if so, what form that explanation should take. For example, as discussed in the previous section, Boudon argued that once we have established the reasons why individuals act as they do, no further questions need to be asked – a position I find difficult to defend.



In Hedström (forthcoming), I discuss these questions in detail and highlight an important shift within the analytical sociology community. First-generation analytical sociologists focused heavily on *intra-individual mechanisms* – examining how different configurations of beliefs, desires, emotions, values, and cognitions explain individual behavior and, consequently, the social outcomes that arise from these behaviors.

This generation included prominent scholars such as Jon Elster, Diego Gambetta, and Boudon. Elster, for instance, argued that “to understand how people act and interact, we first have to understand how their minds work” (2007, p. 67). Much of his work explored mechanisms operating within the individual mind, such as the *sour-grapes mechanism* (Elster 1983), where an individual’s desires adapt to her opportunities, and the *wishful-thinking mechanism*, where beliefs are shaped by what the individual wishes to be true. In Boudon’s case, this intra-individual focus was particularly prominent in his later work on his cognitivist model of behavior.

My own work was also firmly rooted in the first-generation approach. In Hedström (2005), I argued that intentional explanations are crucial because they offer deep, intellectually satisfying accounts that make individual behavior understandable in the Weberian sense. I further maintained that explanations of macro-level phenomena must reference the reasons behind individuals’ actions. The underlying premise was that explanations that do not incorporate individuals’ mental states are incomplete and unsatisfactory.

Inspired by Elster’s work, I based much of my analysis on what I called the *DBO theory* – *D* for desires, *B* for beliefs, and *O* for opportunities. The core idea was that desires and beliefs can be said to cause an action by providing reasons for it. Desires and beliefs have a motivational force that helps us understand and, in this sense, explain an action, while opportunities define the set of actions feasible for the actor. I argued that the proximate cause of an action is a specific constellation of desires, beliefs, and opportunities that makes the action appear reasonable. Elementary action mechanisms differ from one another depending on how desires, beliefs, opportunities, and actions interact.

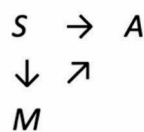
With second-generation analytical sociologists, we observe a shift “from processes within individuals to processes among individuals – that is, from psychology to sociology,” to use Coleman’s (1986a) expression. The theoretical and empirical focus is no longer on what happens *within* individuals’ minds but on the processes that unfold *among* the individuals. Put differently, the focus is on what Schelling (1978) referred to as the “system of interaction” – the ways individuals interact and influence one another, the social processes that these interactions bring about, and the aggregate outcomes they collectively produce.

My thinking on these matters has evolved in a similar direction. I no longer endorse the view I advanced in Hedström (2005) that intentional explanations or other mental-state-centered explanations should form the foundation of the social sciences. As I discuss in detail in Hedström (forthcoming), this change in position is primarily driven by two key observations:

1. Reliable information on individuals' mental states at the moments when they are supposed to causally influence behavior is rarely, if ever, available.
2. Even if we knew an individual's relevant mental states, we would not necessarily know what the individual would do because individuals' do what they do for multiple different reasons.

These knowledge constraints are highly problematic if the ambition is to explain outcomes with reference to the actual processes that brought them about. Seeking to explain why individuals do what they do by referencing their mental states is particularly problematic for sociology, which examines large-scale social processes involving numerous heterogeneous individuals who interact and influence each other over extended periods. Identifying the reasons that motivated someone else to do what they did is challenging enough; doing so for hundreds or thousands of individuals is immensely difficult – likely an unattainable task.

Drawing on Hedström (2021), the situation can be described as follows, where *A* represents an individual's action, behavior, or behavioral disposition, *M* the individual's relevant mental states at the time of acting, and *S* the social characteristics of the individual and its social environment likely to influence both mental states and actions:



As noted above, the first-generation analytical sociologists primarily focused on the  $M \rightarrow A$  part of this scheme. As with any other type of explanation, explaining an individual's actions with reference to certain mental states such as specific beliefs, desires, or emotions, can be correct or incorrect. The explanation is correct if it accurately identifies the mental states that truly motivated the individual's behavior, and it is incorrect if it refers to the wrong set of mental states. However, since we rarely, if ever, have access to the true *M* of individuals, and since *M* can vary both across individuals and over time for the same individual, the likelihood of constructing factually correct  $M \rightarrow A$  explanations is slim indeed. The widespread practice of inventing mentalistic

narratives or models with little empirical foundation in the specific case at hand is not a solution since it contradicts one of the core principles of analytical sociology, that explanations must always reference the actual processes responsible for the outcomes being explained.<sup>2</sup>

If reliable information on  $M$  and the  $M \rightarrow A$  link is unavailable, rather than inventing theoretical narratives to fill this gap, it is more prudent to follow insights from the literature on supervenience and multiple realizability and focus on higher-level difference-makers. These concepts, widely applied in the philosophy of mind to describe the relationship between mental and physical states (e.g., Fodor 1974, Kim 1993, Sawyer 2001), offer a useful framework. A higher-level state  $Y$  is said to supervene on a set of lower-level states  $X$  if two conditions hold: (1) identity in  $X$  necessarily leads to identity in  $Y$ , and (2) identity in  $Y$  does not necessarily imply identity in  $X$ . This asymmetry exists because the higher-level state  $Y$  can be realized in multiple, potentially disjunctive lower-level ways. When this occurs, systematic relationships may be observed at the higher level that do not manifest themselves at the lower level.

Applied to our case, if the same behavior ( $A$ ) can result from a wide range of different mental states ( $M$ ), the absence of detailed information on  $M$ , while regrettable, becomes less significant from an explanatory perspective. This is because knowing an individual's  $M$  would offer only limited insight into what generates  $A$ . As Heath (2024) illustrates with the example of criminal behavior, while understanding the specific motives behind each crime may be desirable, if the  $M \rightarrow A$  link is realized in highly disjunctive ways, “it may turn out that each crime is as unique as the criminal.” In such cases, there would be a token  $M$ -based explanation for each specific act, but no general  $M \rightarrow A$  pattern applicable to the group as a whole. Using Woodward's (2003) terminology, this implies that  $M$  is not an invariant difference-maker for  $A$ , indicating that the explanatory focus should shift to the  $S \rightarrow A$  link, where more stable and generalizable patterns may be found.

In Hedström (2021), I used Schelling's (1971) classic segregation model to illustrate these points. Schelling demonstrated how small-scale interactions can escalate into unintended large-scale outcomes. What matters for the aggregate patterns emerging from the social processes he analyzed is *how* individuals

2 This should be qualified by saying that the statement about “actual processes” assumes that the purpose is to explain a real-world observation. If we instead are in the business of pure and abstract theory development, this restriction does not apply, but as soon as we are to use such abstract theories to explain real-world observations, the statement applies. In Hedström (forthcoming) I discuss in detail the need for clearly separating between the abstract and the concrete in order to avoid what Whitehead (1930) referred to as the fallacy of misplaced concreteness.

react to the behavior and properties of their neighbors – not *why* (in the mentalistic sense) they react as they do. The segregation dynamics remain the same regardless of the underlying reasons for the individuals' behavior. Some may leave their neighborhoods due to prejudice, for example, while others may like their neighbors but fear declining property values as the neighborhood composition changes. The crucial aspect driving the process is not what goes on in individuals' minds, however, but how they respond to their surroundings – whether they choose to stay or relocate. Thus, the social dynamics and the resulting aggregate outcome are determined by the details of the  $S \rightarrow A$  link, not by the  $M \rightarrow A$  link. It is properties of the social context and how individuals react to them, rather than their internal motivations, that are the crucial difference makers that shape the process.

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The focus on higher-level difference-makers that characterizes Schelling's work, also is a defining characteristic of what I have termed second-generation analytical sociology. One example is Bearman et al.'s (2004) study of adolescent sexual and romantic networks. The context of their study was a high school in the United States, and the macro-outcome they sought to explain was the surprising discovery that the students' sexual and romantic network resembled a spanning tree. Through simulations, they concluded that the spanning-tree structure was most likely the result of boys avoiding relationships with their prior girlfriends' current boyfriends' prior girlfriends, and vice versa for the girls. There can be many different reasons why students avoid such relationships, and they may vary over time and between individuals. However, what matters for the aggregate outcome – the spanning-tree structure of the network – is *that* this avoidance exists, not *why*, in the psychological or mentalistic sense.

Another example is Arvidsson, Hedström, and Collet's (2021) study of gender segregation in labor markets. They show that network-based recruitment, contrary to conventional wisdom, can reduce rather than increase segregation through what they term the Trojan-horse mechanism. Analyzing detailed employment records from Stockholm, they found that when individuals leave organizations where they were in the minority, they were disproportionately likely to be followed by majority-group members from their original workplace. Much like the soldiers in the Trojan horse opening Troy's gates from within, an initially segregating move such as a woman moving from a male dominated to a female dominated workplace, can open the gate for subsequent desegregating moves of men following in her path. As in Schelling's and Bearman's analyses, the core difference-makers do not refer to what goes on within the minds of the individuals. Instead, the difference makers relate to the details of the  $S \rightarrow A$  link. What matters for the collective outcome is *whether* individuals are disposed to follow in the network paths of others, and *whether*

the gender composition of the original workplace influences the gender of the followers; not the various psychological or mentalistic reasons for *why* that is the case.

Another example is Manzo et al.'s (2018) analysis of the diffusion of innovations in pottery across northwestern India and central Kenya. Their goal was to explain a macro-level outcome – specifically, the differences in diffusion curves among four ethnic sub-groups – by focusing on the actions and interactions of the potters. Their main finding revealed that differential motivations among individuals had a negligible effect. At the same time, the structure of the interaction network, particularly the configuration of strong and weak ties, played a major role. As with the other second-generation analyses discussed earlier, the key difference-makers for the outcome concern the details of the  $S \rightarrow A$  rather than the  $M \rightarrow A$  link.

In his book on complex contagions and the spread of behavior, Centola (2018) adopts an approach closely aligned with the one advocated here. He argues that while the collective facts we aim to explain are often well established, and we typically know a great deal about what individuals do, “what is not known is the dynamics. How do individuals interact to produce these collective phenomena?” (Centola 2018, p. 180). To address this, Centola develops a range of generative models – to use Boudon's term – that illustrate how different types of collective phenomena can emerge from individual interactions. Toward the end of the book, Centola reflects on the theoretical and methodological lessons derived from his analyses, and one key insight stands out: what drives the dynamics “is only that individuals are embedded in social networks that provide them with relevant sources of social reinforcement” (2018, p. 173), not whether individuals act rationally or are driven by specific reasons or emotions.

The explanations proposed by these second-generation scholars thus are not framed in terms of the mental states of the acting individuals because (1) reliable empirical data on individuals' mental states is rarely if ever available, and (2) many or perhaps even most social processes that sociologists are concerned with are not dependent upon motivational details but on the details of the  $S \rightarrow A$  link. For these reasons, the primary focus is on the social situation of the individuals and the explanation typically takes the following dispositional form: If individuals of type  $i$  tend to do  $A$  when placed in a social situation of type  $S$ , then individuals of type  $i$  can be said to have a social disposition to do  $A$  in  $S$ , and  $A$  is explained by referring to this disposition. In other words, in the second generation there is a shift in focus from the  $M \rightarrow A$  to the  $S \rightarrow A$  link, and a corresponding move from intentional to dispositional types of explanations (see also Vredenburg 2024 for an illuminating discussion of related matters).

These kinds of dispositional explanations are central to most middle-range theories in sociology and include key behavioral tendencies such as reciprocity, homophily, and social influence. Bourdieu's influential notion of *habitus* (e.g., 1990) is also dispositional in orientation. Although his writing can be difficult to interpret, *habitus* can, in the terminology of this chapter, be understood as a socially conditioned disposition to act or think in certain ways. Consequently, a *habitus*-based explanation of an individual's actions or thoughts refers to the relevant socially conditioned disposition. Bourdieu was primarily concerned with dispositions formed over the *longue durée* – giving rise to stable social patterns in taste and behavior. In contrast, most analytical sociologists focus on more immediate effects of social interactions and rapidly changing social environments, but the underlying explanatory logic remains similar.

## CONCLUDING REMARKS

Boudon was a hugely important source of inspiration for many sociologists, not the least in Europe. In his early work, he showed a strong interest in formal theorizing and emphasized the explanatory importance of building generative models that demonstrate how groups of individuals, through their interactions, produce the collective outcomes to be explained. In his later work, he became more discursive, and he did not attempt to give his theories of action the formal structure necessary to integrate them into the type of generative models he had previously advocated.

In Hedström (2013), I suggested that an important task ahead of us was to bring together these two strands of Boudon's work – his type of generative explanatory modelling and his discursive approach to action theory. However, I am far less convinced today of the merits of doing so than I was a decade ago. Some scholars, such as Jon Elster, have remained deeply committed to the idea that an explanation of a macro-outcome is incomplete and wanting unless it intentionally explains why the involved individuals did what they did. Boudon held a similar position and argued firmly for the centrality of reason-based explanations: "when a sociological phenomenon is made the outcome of individual reasons, one does not need to ask further questions", the explanation is "final" (Boudon 1998, p. 177).

As noted above, there has been a shift in focus of analytical sociology from what occurs within individuals' minds to the processes that unfold among individuals. In relation to Boudon's work, this shift can be described as a movement away from the type of work represented by his cognitivist action model toward the type of work represented by his generative models.



Boudon's own trajectory, however, was in the opposite direction – a somewhat unfortunate development, in my view.

In this chapter, I have explained why I find this shift in focus from the mental to the social so important. While it certainly would be informative to know what individuals were thinking when they acted as they did, obtaining reliable information on this is both difficult and highly prone to error. We can, of course, speculate about what went on in their minds. However, it is highly unlikely that such speculations will provide a factually correct explanation of how the outcome to be explained was brought about – particularly when many individuals are involved, each potentially driven by a different reason.

The fact that we rarely know what goes on within individuals' minds is not always an explanatory handicap. This is because many social processes are not driven by motivational details. Instead, the crucial difference-makers lie in key aspects of the social environments in which the individuals are embedded. In this chapter, I have discussed important work that exemplifies this such as Schelling's analysis of segregation processes, Bearman, Moody, and Stovel's (2004) analysis of romantic networks, Manzo et al. (2018) analysis of diffusion processes, and Centola's (2018) work on how behavior spreads.

This shift in focus away from what occurs within individuals' minds also means that intentional explanations are no longer applicable. In this chapter, I have argued for a dispositional form of action explanation, grounded in empirically well-established behavioral tendencies such as reciprocity, homophily, and social influence. This approach should be coupled with the kind of generative models proposed by Boudon to address the macro-outcomes likely to emerge. This type of approach aligns well with the tradition of middle-range theorizing (Hedström and Udehn 2009) and plays to our strengths in terms of empirical data and methods of inquiry. Much work remains to refine the details of a dispositional explanatory framework, but the effort seems well justified. Following this approach would allow our empirical research to focus on the crucial difference-makers proposed by our theories and, in doing so, help bridge the gap between empirical research and theoretical development.

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## MIDDLE RANGE THEORIZING

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## MERTON AND BOUDON

Sociology is not in a good state, and that is not only its own fault. There has always been a demand for “grand” social theories and readable and historiographical interpretations of social developments on the one hand, and small-scale empirical reports on local social conditions and changes on the other. In between, there is a large gap with questions that are always limited in terms of content and, if at all, theoretical ad hoc concepts whose “range” is unknown or limited. A long time ago, Robert K. Merton pointed out a solution to this unsatisfactory situation, which has been very well appreciated, not only in sociology: the concept of Theories of Middle Range (TMR; Merton 1949). For him, these are generalizations of certain conditions and interrelations for substantively delimited areas without an explicit reference to an all-encompassing “general theory” behind them. Examples would be relative deprivation, the concept of role sets, or the spreading of rumors and self-fulfilling prophecies. According to Merton, one should continue to work on such limited models and, instead of waiting for the grand design, proceed in small steps of empirical research and theoretical generalizations. Over time, this *could* result in a sociological “grand theory” that is more than just an empty conceptual scheme, as with Parsons or Luhmann, or a vague “theorizing”, as is so widespread in contemporary sociology, but also not just a confusing collection of disconnected empirical results. The concept of TMR was immediately well appreciated, probably also because of the encouraging prospect that even small steps could contribute to find the desired masterpiece of a comprehensive sociological theory and that it is by no means necessary to wait for the singular genius – as Newton or Einstein once did for physics.

Raymond Boudon once also took up the concept of TMR – like many other parts in his great affinity with the approach and thinking of Robert K. Merton

(Boudon 1991). He praised it highly and followed Merton, particularly the suggestion to pay special attention to TMR if progress is to be made. Both initially agree on two central points: The gap between “grand theory” and everyday empirical work *must* be closed; and work on concepts of TMR is, as Boudon explicitly writes, “indispensable” to come closer to the ideal of an analytical-explanatory overarching sociological theory. In particular, both agree in their understanding of what constitutes a “theory” at its core. Merton makes this statement right at the beginning of his classic essay:

The term sociological theory refers to logically interconnected *sets of propositions* from which empirical uniformities can be *derived* (Merton 1949, p. 39; emphasis not in the original).

And Boudon has this to say about it:

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... a “scientific theory” is a *set of statements* that *organize* a set of hypotheses and relate them to segregated observations. If a “theory” is valid, it “*explains*” and in other words “consolidates” and *federates* empirical regularities which on their sides otherwise appear segregated (Boudon 1991, p. 520; emphasis not in the original).

These are clear commitments that definitely go beyond the “theorizing” of the “bad sociology” so deplored by Boudon: Even theories of only “middle” range *must* always already be correct and empirically proven “theories” in the sense that they are “logically interconnected” statements that *explain* a state of affairs and can thereby “consolidate” and “federate” the otherwise unconnected empirical regularities in its justification that goes beyond the particular case.

The problem that then arises specifically for sociology is all too familiar: there are no general “laws” at the level of social structures, and even the few regularities that one could think of, for example, as “quasi”-laws, are not without exceptions. Even then, they still depend on many, mostly unmanageable, assumptions. For example, that there are no wars between democracies, the standard example in Cartwright (2020, p. 271ff.) in her discussion of the problem of TMR (see below). This brings this question of the existence of general “laws” into even sharper relief – what is the point of striving towards that overarching general sociological theory as a guideline for all specialized work with limited scope, if this basis of an overarching axiomatic system of sociological laws does *not* exist and never *can* be?

According to Merton, and also Boudon in his contribution specifically on TMR, the path of further development consists in the gradual elaboration and expansion of provisional models of medium scope and range. The vast

majority of those who have dealt with the concept of TMR have been happy to follow this approach, for example with pleas for the limitation of dealing with concrete institutions at the “meso” level or in the now common equation of TMR with the concept of “mechanisms”, the uncovering of the causal process behind the observed covariations and patterns of empirical relationships for particular areas.

## A DIFFERENT VIEW

This all sounds quite plausible: Instead of waiting for the great, all-encompassing sociological theory in which all empirical phenomena can be integrated, one should try to gradually come closer to the general sociological theory step by step through modest work on explanatory sketches.

But is this truly good advice? Karl-Dieter Opp was one of the very few to criticize the route proposed by Merton, and he did so vehemently (Opp 1970). He calls the concept a misapplied “sociological dogma”. The core of the criticism relates to the “strategy for the construction of a general sociological theory” brought into play by Merton (Opp 1970, pp. 243f., 252f.).

Opp distinguishes between a direct and an indirect strategy of theory development. The direct strategy consists of the *immediate* attempt to provide a (“general”) theoretical *explanation* with as much explanatory power as possible for *all* the respective explananda, which, if it does not succeed, must be modified and replaced by an alternative theory. This is the usual procedure according to the rules of scientific discovery and may *immediately* lead to theoretical progress. There is also an indirect strategy: the construction of a theoretical explanation at a provisional and less demanding level of general validity before attempting a more far-reaching theoretical explanation. The indirect strategy corresponds to Merton’s stipulation of embarking on the long step-by-step march towards a general sociological theory by continuing to work on the existing theories of “intermediate” scope.

Opp gives several historical, logical and methodological reasons for the direct strategy. Probably the most important argument is that without an immediately applicable theoretical framework of the most general possible range, there would only be (further) confusion in the concrete analyses of the research objects, which are *always* limited in scope. Moreover, one could add, it would be an impossible attempt to *inductively* reach a “general” theory solely by collecting and generalizing empirical evidence in the detailed work on the respective TMR, without a theoretical justification of its own *beforehand*.

Therein lies the fundamental difference to the concept of TMR and the indirect strategy proposed by Merton: there *is* the possibility of a general nomological

explanation according to the HO-scheme with a *wide* range even without an axiomatic system of universal sociological laws. And that is why one cannot and must not proceed indirectly, but rather strive for an explanation right away.

Opp, like many others at the time, was thinking of the now well-developed macro-micro-macro model of sociological explanation (hereinafter referred to as MSE), as presented by Coleman in the first chapter of his *Foundations* and popularized as the “Coleman Boat” (cf. on the largely overlooked predecessors in the development of the model, to which Opp also belonged: Raub 2021). The MSE emerged not least through Boudon’s significantly earlier contributions. And it was particularly in the context of his work on social action and social change that it acquired its current structure (Boudon 1981, 1986).

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From the outset, Boudon’s special feature was the reference to Weber’s microfoundations and thus also to the *interpretative* and *cultural* dimension of the MSE with categories and aspects that can only be accommodated with great difficulty, if at all, in the narrow and wide variants of Rational Choice Theory (RCT). These include the well-known four types of action and the reference also to “ideas”, which, unlike “institutions” and “interests”, cannot be dealt with in a theory of rational action, as is the case with Coleman in his *Foundations*. It is not without reason that Boudon himself called his version of the MSE the “Weber-Model”.

Against this background, Boudon then also proposed, almost unnoticed, a different concept for Merton’s important question of generalization of a found “local” solution to an explanatory problem: that of “Structural Models” (Boudon and Bourricaud 1982). This means that successful HO-explanations already found in the direct strategy can be generalized for a particular area by abstracting the basic structural pattern to similar cases and thus extending the scope of the respective model, while the range of the explanation can already be general anyway or even grow. The model of “Exit, Voice and Loyalty” by Hirschman (1970) is cited as a prime example. We will return to this briefly at the end of this essay.

## OBJECTIONS AND SUGGESTIONS

The outlined concept of structural models or, more generally, the model of sociological explanation as an alternative to the concept of TMR and indirect strategy put forward by Merton overlaps with several objections and various proposals to classify the concept of TMR in the various currents of the sociological and philosophy of science debate. The key points of three particularly significant contributions will be addressed: James S. Woodward on the fundamental possibility of HO explanations, Nancy Cartwright on the

advantages of dispensing with the search for overarching theories and Alban Bouvier on approaches that could already give direction to the work on TMR, such as the contributions of “analytical sociology” in particular, but which are also more or less limited or can remain too vague.

JAMES S. WOODWARD

The contributions of James S. Woodward (2000, 2005) are the most likely to criticize the deductive-nomological explanatory scheme. The core argument is that the “general laws” necessary for an HO explanation do not exist and that they are also unnecessary. There are always exceptions and only conditional validity, and for the purpose of “generalizing” a theoretical hypothesis it is sufficient that the relationship is “invariant” beyond the individual cases, especially if there are targeted experiments with a controlled manipulation of the relevant conditions that remain robust against different interventions and variations.

Woodward’s general objection would concern the concept of direct strategy, which is always and immediately “general” nomological explanations at its core, as well as all other concepts that presuppose HO explanations, especially the model of sociological explanation. But it is not really a relevant objection: “generality” and the properties of “law-like-ness” are, like everything else in the empirical sciences, unprovable. But there are agreements on rules and procedures in which it is possible to test whether they exist according to these agreements: methodologies of causal analysis and evidence for the conditionalization of the conditions of validity. Experiments are particularly suitable instruments for this, and at their core are targeted interventions and manipulations, which also include practical applications. The results are fairly robust and “invariant” correlations. And that is ultimately all the knowledge that is needed to be able to make an explanation according to the HO scheme and thus follow the direct strategy.

Any remaining philosophical reservations can never be dispelled anyway. This also applies to Woodward’s proposal for experimentally-proven invariance. Probably the most important aspect is that in all cases, the explanatory hypotheses must go beyond the individual case, and the broader this range is, the better. This also includes the fact that it is precisely this generality that determines, among other things, the explanatory power of a theoretical hypothesis, including the logical content of the hypothesis, the valid empirical interpretation of the theoretical constructs via measurement hypotheses and, finally, certainly also the validity in targeted tests and the robustness of the findings after experiments with intervention and manipulation.



Nancy Cartwright (2020) approaches the concept of TMR from the position of the developed natural sciences, which are often cited as a model for the possibility and fruitfulness of axiomatic deductive theory systems. She brings the concept of TMR into play from developments in the natural sciences, which are increasingly deviating from the traditional view and dealing with more “local” questions and partial solutions and would consider this sufficient, for example in chemistry and biology. Cartwright’s starting point is the question of the conditions for an appropriate evidence-based evaluation of political programs, that is, the actual consequences of practical measures based on certain theories, for example, in the field of education or the design of political institutions.

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The core of her answer lies in the aspects put forward in the context of the concepts of “analytical sociology”, first and foremost, the reference to “mechanisms” as “generating” processes of a causal event behind the empirical processes on the surface. Cartwright identifies a number of questions and difficulties with the various approaches: Are all relevant factors covered? Have the activities required for implementation been taken into account? Are covariations really “causal” relationships? Or are there not rather special contexts that provide for an interactive-processual conditionalization of the effects? For Cartwright, all this amounts to *equating* the concept of TMR with the concept of “mechanisms”. This is now a common interpretation. The question is whether one can really see it this way or whether the prerequisites of a general HO explanation must also be fulfilled when referring to “mechanisms” and whether the question of generalization is thus posed differently than just “pragmatically”.

In this context, Cartwright outlines the elements necessary for such a concept below the major theories. These consist of a mixture of elements known from the methodology of MSE: Causal explanations, microfoundations, and a sequential situational logic, all intended as uncovering the initially hidden processes and corrective explanation when there are anomalies or conditionalizations that have not been thought of and that would block generalizations. However, this in turn would require a series of conditions that are difficult, if not impossible, to fulfil, such as the inherent plasticity of interrelationships and the “untidiness” specific to social processes. It is difficult to think of general “laws” anyway. At best, there are “middle range laws”, bound to particular contexts.

However, she continues, this does not in any way imply a discouraging attitude towards working on such projects of inevitably “medium scope” at



best. Instead of chasing after the futile goal of the all-explaining grand theory, it is more advisable to concentrate on the cultivation of successful practices in research communities and to be satisfied with the instrumental usefulness of partial solutions: a “community-practice-centered-instrumentalism”. Criteria of coherence and plausibility are more important here than a goal of approximating the truth as a regulative idea that is ultimately never really achievable. It would be the abandonment of what Weber, Merton, Popper, Opp, Boudon and others regarded as the self-evident scientific-theoretical basis and regulative idea of the social sciences in particular.

ALBAN BOUVIER

Alban Bouvier’s contribution (Bouvier 2023) goes in a similar direction: Even if there are many indications that science is messy and divided, this by no means implies that the real world already is and that science then only reflects this in its disunity. Nor does it mean that the standards of scientific work, the regulative idea of approaching the truth, and the goal of a unity of the (social) sciences, must be abandoned. On the contrary, the idea of TMR should not lead to a situation in which we are content with less precision and less targeted scope of statements, and possibly pass off and rationalize the relaxation gained as an advantage of flexibility, openness, and pluralism – as has obviously happened in economics and biology in the meantime. For the social sciences in particular, Bouvier sees the danger of negligent or deliberate “balkanization” and he insists that this should not be allowed to happen. He calls this variant of the reaction to the many difficulties the “weak option” of dealing with the problem of “unity” (Bouvier 2023, p. 12), an option that Merton himself had inaugurated in his proposal and which so many were then all too willing to follow.

The background to Bouvier’s plea for the revival of an orientation towards a particular methodological thoroughness, especially for social science, are two developments that both understand and describe themselves as “analytical sociology”: the orientation towards the approach of James S. Coleman, which he proposes in his magnum opus *Foundations of Social Theory* (Coleman 1990), and the variant of analytical sociology as it would become recognizable with the *Oxford Handbook of Analytical Sociology* (Hedström and Bearman 2007). Bouvier describes the Coleman approach from the *Foundations* as the “strong option” and that from the *Oxford Handbook* by Hedström and Bearman as an “intermediate option” between the weak and the strong alternatives (Bouvier 2023, p. 14ff.).

The fundamental differences between the two variants are easily identified: In the *Foundations*, Coleman pursues a strong methodological rigorism with clear requirements such as logical content, precision, and parsimony of a theory (see Raub et al. 2022). The overarching theoretical basis is a particularly narrow version of RCT, which Coleman also applies to phenomena with which this becomes more questionable – such as (unconditional) trust, commitment, or authority beyond interest and control alone, both the central categories in Coleman and his narrow version of RCT.

The Oxford variant adopts some of the basic guidelines of this “rigorous” sociology, but loosens them up in some key points: Explanations according to the HO-scheme are not really possible; it is much more productive and sufficient to uncover “mechanisms” by revealing the inner connections. A particularly precise micro-theory is also neither possible nor necessary, and certainly not the narrow version of RCT as used by Coleman. Moreover, there is a whole wealth of phenomena that can hardly be theoretically grasped any further, such as those so extensively described in Jon Elster’s work (Elster 1979, 2000). The solution is the transition from almost any attachment to the concept of the HO-explanation and, in particular, to RCT in the so-called DBO approach, in which the microfoundation is only carried out via the enumeration of the three conditions of RCT (Desires, Beliefs, Opportunities), without further consideration of the respective relationships to explain the selection of an activity (Hedström 2005).

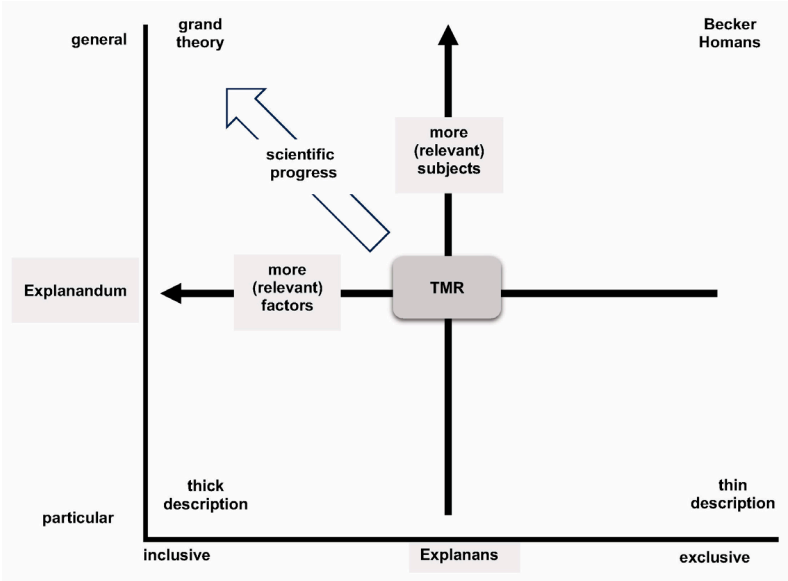
According to Bouvier, this brings this variant of analytical sociology closer to the other pole and Cartwright’s ideas: renouncing explanations, turning to instrumentalist behaviorism and abandoning stronger methodological standards precisely at the point that is crucial to the goal of (general) explanation in MSE: its microfoundation. However, one could also imagine that there would be something like “*intermediate* theories of middle range” in a “hierarchy” of more or less far-reaching theories, those of the more open type of the Oxford approach in contrast to the weak solutions in parts of economics in the meantime, chemistry and biology, and the strong, but also narrow version in the Coleman variant of the MSE (Bouvier 2023, p. 14). Bouvier is obviously not thinking of a wide and at the same time strong solution. But *that* would exactly be the solution to the problem: a general and inclusive, but *also* explanatory social theory for the concrete and always *particular* explananda.

## A SYSTEMATIZATION

The result of the various contributions can be summarized simply: “General” explanations of sociological issues can be made without reference

to an (axiomatic) “general theory”. The “generality” and regularity of the explanation results from the respective action-theoretical microfoundation. But otherwise everything is more or less “limited”, “local” or “particular”, even occasionally: “singular”, as in historical explanations and the reconstruction of the “situational logic” of unique events. The central problem with TMRs, as with structural models, is the question of how to *transfer* solutions once they have been found to structurally *equivalent* cases and what the relationship between “limitation” and “generalization” looks like. Hedström and Udehn have developed an illuminating typology for this problem (Hedström and Udehn 2007, fig. 1).

Figure 1: Theories of Middle Range for Generality and Inclusion



The typology refers to two dimensions: the particular explanandum and the conditions in the explanans, the initial conditions and a general nomological theory according to the HO-scheme. The vertical axis describes the generality of the explanatory problem (from particular to general), the horizontal axis the scope of the explanatory conditions included for a particular explanation – from the inclusion of all possibly relevant factors to the exclusion of only one dominant condition).

In this field, four constellations of theories and analyses with different scopes of explanation are specified: first, “story telling”, where particular processes are only narratively enumerated without further consideration of explanatory conditions; second, “thick description” with the inclusion of

explanatory conditions as well; third, exclusive general explanations; fourth, inclusive general explanation. Accordingly, there are generalizing explanations limited either to a few explanatory factors or including all conceivable relevant conditions. Examples of the thin description would be the simple historical “narrative”, while the thick description could be Goffman’s analyses of role behavior in typical social settings. For the exclusive general explanation, the approaches of Gary Becker or George C. Homans would be examples, and for the inclusive general theory, a “grand theory” of social systems, such as Parsons or Luhmann.

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The TMRs are located in the middle of the typology between the two axes. They reflect a certain “middle-range”-state of research in each case: no longer just particular, but also far from general, not just concentrated on one dominant factor, but also not yet complete in terms of the explanatory conditions. Merton’s proposal then boils down to the fact that the development moves to the top left of the diagram via the further elaboration of existing or new TMRs with the aim of creating an explanatory theory that is as general as possible (see the block arrow).

However, “grand theory” here does not mean the empty “theorizing” so criticized by Boudon, but rather the development of an overarching social theory that is as axiomatized as possible, with which in principle all sociologically significant explananda can be explained, including as many relevant conditions as possible. Simple story telling and “thick” descriptions would of course still be possible within this framework, but one would already have the possibility of embedding their findings in an overarching explanatory framework, and the explanations would also be able to refer to conditions that do not occur in the narrow exclusive approaches and may also lead to certain anomalies and blind spots in the explanation, such as those that Bouvier associates with the weak or the intermediate option (see just above).

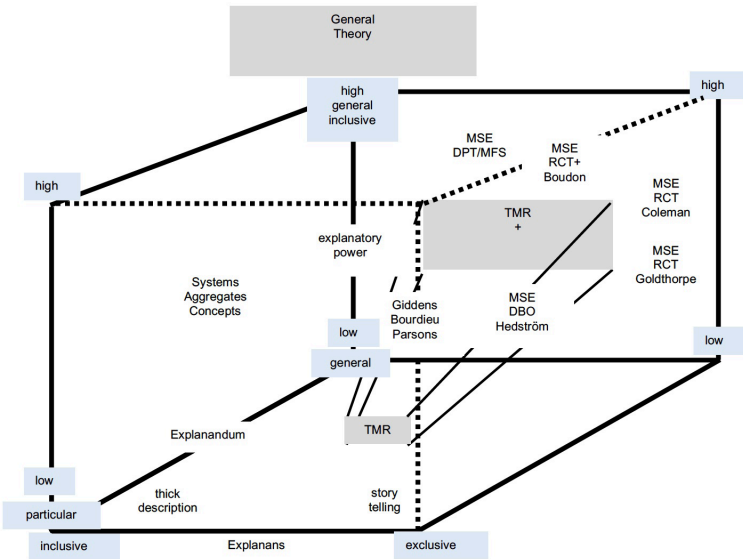
## EXPLANATORY POWER: THE THIRD DIMENSION

The concept looks quite plausible, and implicitly most contributions to the TMR have followed this idea, especially in the assumption that Merton’s proposal would be an early anticipation of later concepts, such as in particular that of “mechanisms”. What is missing in all of this, however, is what Opp so clearly emphasized in his criticism of the indirect strategy: irrespective of all the differences in the scope of content of the explananda and the inclusivity of the explanatory factors taken into account, the *explanatory power* of the underlying theoretical foundation is also of central importance. However, this would be a third dimension of the evaluation of the state of research and

the development towards a general and explanatory sociological theory. It is missing in Hedström and Udehn’s concept.

The explanatory power of a theory consists of a number of different characteristics. Essentially, it is about fulfilling the conditions of a valid HO-explanation (for a given set of explananda and possible explanatory factors and its respective scope): The justification of a relation between a condition and a consequence by a causal “law” that is as general as possible and occurs in at least one place of the complete theoretical argument; the *validity* of the associated initial conditions, bridge hypotheses, auxiliary assumptions and measurements; the *logical content* as parsimony and precision of the explanation, most likely to be achieved by formal modelling, the less or more encompassing universe of the explanation for the respective sets of objects; and finally the successful *empirical corroboration* of a solution once found, best secured by robustness in replications and a “corrective” explanation of any anomalies that may occur by the successful conditionalization of contradictory partial theories (cf. Popper 1964). These properties can be seen as additional levels and aspects within this third dimension, not all independent of each other. Figure 2 shows this third dimension for the two-dimensional concept in Figure 1.

Figure 2: Theory Development in TMR According to Generality, Inclusivity and Explanatory Power



The explanatory power of an approach consists in this conception of *three* dimensions and with regard to the possibilities of theory development from its theoretical potential, not necessarily from empirical evidence. In the context of MSE, this potential relates to two areas: the microfoundation, and the theoretical architecture in the micro-macro transition. The *microfoundation* concerns the respective theory of action with different limitations and possibilities of modeling, such as the narrow and the broad version of RCT. The *theoretical architecture* refers to the coverage of different parts and levels of the MSE. The three most important are: Conceptual systems, aggregations as the individual effects in the MSE and social systems as “emergent” effects of the whole MSE. These form a kind of *hierarchy*: categories are conceptual systematizations without further explanatory power. Aggregate relationships consist of relatively simple statistical operations, such as variable relationships in regression equations. Systems refer to more or less complex forms of interdependencies and interrelations of all kinds, such as markets, groups or organizations. In Figure 2, these references are shown on the left side for the *horizontal* dimension of generalization and the theory-immanent *scope* by its architecture, and *vertically* for the dimension of explanatory power in the *range* of the respective microfoundation and analytic instruments. A truly “general” sociological theory should therefore always have to be a “system”-theory. Conceptual systems have unlimited scope, but the smallest explanatory range. Aggregations would be “intermediate” in between.

In this respect, the transition from conceptual systems for the description of socially relevant functional “spheres”, as in Parsons AGIL-scheme, orientation hypotheses for dominant factors, as in Bourdieu, or for the ideas of the “constitution of society”, as in Giddens, to aggregations as explanandum, for example in mobility research, as in Goldthorpe, would already be clear steps towards more explanatory power and range - the scope of the content-related area kept constant. Ultimately, the goal would be a sociological systems theory with the highest possible generalization, the coverage of all (relevant) conditions, and the highest possible explanatory power. Accordingly, it would go beyond conceptual contributions alone, but also beyond a “sociology as population science” with its restriction to variables-relationships or processual linkages in contagion and diffusion models. The *immediate* reference to the level of systems would be the perspective of the methodology of the MSE and the direct strategy for theory formation and theory development. In this way, questions can be addressed for all levels, on systems and on aggregations and for all forms, contents and areas of sociological explananda: singular and particular as well as broader or completely universal questions such as the universal anthropological foundations of the “human condition” and its development

– as Gintis (2017), for example, has attempted to do in a broad crossover beyond the boundaries of biology, economics and sociology.

The theoretical progress from the constellation of TMR given in Figure 1 can now also be represented in its movement this field of scope and range: The shift in theory development as an expansion in the *scope* according to generality of *explananda* and inclusiveness of *factors*, but *also* of the explanatory *range* in the explanatory power of the respective theoretical framework in all its parts, its *theoretical potential*.

## SOCIOLOGICAL APPROACHES AND THEORY DEVELOPMENT

Against this background, the different approaches and directions in sociological theory development can be categorized for the central aspect of sociological explanation: the *range* of *explanatory power*. We follow the three guidelines discussed above: Opp's call for a direct strategy of theory development, the general methodology of sociological explanation now available with the MSE, and Hedström and Udehn's typology extended by the dimension of explanatory power in Figure 2 just above.

### COLEMAN

We take *Coleman* and his concept of MSE in the *Foundations* as the reference for the further comparisons (MSE/RCT/Coleman as combination of the Model of Sociological Explanation, Rational Choice Theory and Coleman). It is the solution to the question of the development of a general and possibly unifying social theory that Bouvier criticized as overly "strong": the use of the MSE in conjunction with a particularly strict variant of RCT, the economic theory of exchange and markets, in a hitherto unknown consequence to a variety of sociological explananda, including those that are outside the narrow field of economic issues. This means that his approach can be regarded as comparatively "general" for the explananda covered, but also as decidedly "exclusive" for the factors considered, because only a very narrow form of RCT is used in each case. However, this is precisely what lends it a particularly high explanatory power: extreme parsimony, combined with a particularly high risk of falsification (cf. Raub 2024).

### GOLDTHORPE AND HEDSTRÖM

Coleman's social theory is, by his own admission, a *systems* theory. Two other approaches are *not*. First, Goldthorpe's "Sociology as population science". It is



limited to aggregates and causal chains of socio-demographic processes. In doing so, it ignores all interdependencies (according to relations in “interest” and “control”), the core of Coleman’s systems theory. This lowers the explanatory power and thus the range of its validity for entire fields of sociology of relevance – such as the *system*-integration of societies as opposed to the *social*-integration of their individual members, or unconnected decisions as opposed to game-theoretical constellations of strategic interdependence. The scope of the fields of work is correspondingly limited: gender gaps, social inequality, educational decisions, mobility, migration, and integration, narrowed down to processes of inclusion in education and the labor market or emotional identification of migrants with the receiving. These topics dominate large parts of current sociology. Both the scope and the range are correspondingly small, because everything relates only to aggregates and statistical measures, to “variables”. In the diagram, the approach is therefore also below Coleman (MSE/RCT/Goldthorpe). In contrast, no difference is assumed for the scope of the underlying microfoundation: It is a comparatively narrow and strong version of RCT, the value-expectancy theory, with also much about the processes can be explained as with Colemans approach.

The DBO approach according to Hedström (MSE, DBO, Hedström) is broader in its generality and inclusivity, that is, its scope: There are no a priori limitations in the explananda, neither in the explanatory factors. This refers to the particular position of the DBO approach to the many peculiarities of social action, “anomalies” or “heuristics”, as described by Elster, where there has been no attempt to systematically incorporate them into an explanatory microfoundation. RCT is, in this approach, generally rejected as too narrow, and its precision and logical content are abandoned in favor of a loose orientation towards three possible influences on the choice of an action: desires, beliefs, and opportunities. The (causal) function of their effects, which is essential for an explanatory theory, is thus left open. Therefore, the explanatory power of the DBO approach hardly differs from that of a “thick description”, verbally as a “narrative”, via statistics of distributions, or as results of the extraction of patterns from fairly large stocks of “big data” or agent-based models without reference to general theories of action. The DBO approach is therefore at the bottom of the diagram, that is, less than what one might already have with theories of only “medium” explanatory power. This inclination of the DBO approach towards instrumental-behavioristic positions has tended to become even stronger over time (cf. Hedström 2021).

Goldthorpe and Hedström thus represent approaches *below* the standard already achieved by Coleman: a systems theory with high explanatory power. With these approaches, no development in the direction targeted by Merton



and Boudon could be expected. So “Coleman” after all? As already mentioned, Bouvier considers the approach to be too narrow to enable a comprehensive and integrative social theory. Above all, he criticizes the fact that in some applications the possibilities of strict RCT are clearly overstretched and aspects are taken up that go beyond the scope of the particularly strict variant of RCT that he uses: trust, commitment, authority, zeal, for example, and especially the cases in which they are triggered *spontaneously* and are *unconditionally* valid against *any* rational consideration or inclination.

#### BOUDON: BEYOND!

This was then also the gateway for much criticism of the *opus magnum* of Coleman’s *Foundations*, for example in the distinction between power and authority, where, following Weber, it is also about mental *ideas* of the “legality” of inequality, which cannot be accommodated in the concepts of RCT regardless of all efforts. In essence, this involves two types of changes to RCT: extensions of RCT with the addition of other motives and expectation functions versus a complete change in the microfoundation with the development of models of action selection that include RCT and other “action types”, such as routines and emotions, as special cases and can explain when one and when the other type applies. This would enable mutually “corrective” (“deep”-)explanations as well as a nomologically-based integration of different theoretical programs into the MSE, such as those of interpretative, institutionalist, or pragmatist approaches as conditionally special cases alongside RCT in its different variants.

This threshold of a microfoundation extended by interpretative and cultural aspects was actually only attempted by Boudon within his framework of an explanatory sociology, guided by his proximity to Weberian sociology (Boudon 1981, 1986). This is characterized in the diagram by the greater inclusivity of his version of the MSE than Coleman’s, in particular the possible increase in explanatory power and thus the range beyond that of Coleman’s narrow RCT (MSE, RCT+, Boudon).

It should also be added that Boudon himself did not implement this program to any great extent. It was more a question of potential extensions of the range. He did attempt to do so in various works, but more implicitly, and in passing: in connection with his work on educational decisions with the assumption of differences in risk aversion in the event of loss of status and in his RCT reconstruction of processes of relative deprivation under the assumption of “weak solidarity”. There has also been a direct attempt to extend the microfoundation beyond the RCT: the assumption of a “cognitive” or “axiological” rationality of moral feelings (Boudon 2009). This proposal was

very controversial. But it was one of the few and first attempts to overcome the limits of both narrow and broad RCT and to include other *types* of action than rational choice.

The elaboration of a conditionalizing-integrative *and* explanatory theory of action would be a solution to the problem of Elster's findings on the many anomalies of RCT: the inclusion of the various types of action and heuristics in an explanatory overarching microfoundation – in contrast to lowering the demands on an explanatory theory as in the DBO approach. It would also be another solution to the problem for theory progress that Bouvier sees: The approach is not too narrow and not too strong as in Coleman, but also not too broad and not too weak as in the DBO approach. It would be progress in a more inclusive and at the same time more explanatory direction than the other two positions.

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Raymond Boudon has always seen this perspective, especially in his early contributions to the development of the MSE, but also in some concrete applications. His prime example of a structural model, the alternative concept to that of TMR (see below), also has to do with this: "Loyalty" as a mental attitude is difficult to capture with RCT, especially when, as with similar constructs such as trust, commitment or solidarity, we are dealing with "unconditional" attitudes that are also independent of the specific circumstances as rational benefits, costs and risks.

However, the development in the microfoundation of action theory in the direction of conditionalization and overarching integration has not stood still either. For some time now, there have been attempts to reconnect the microfoundation of MSE to Weber's differentiation of action types, to the significance of "ideas" and culturally shaped "meaning", also inspired by Alfred Schütz's theory of everyday action and the connection to developments in cognitive social psychology and recent neuro- and AI research.

This is indicated in the diagram by the entry MSE/DPT/MFS. This refers to two developments: first, the so-called "dual process theories" (DPT), which can explain when cognitive deliberation takes place during action selection and when automatic triggering of action programs happens; and, second the "Model of Frame Selection" (MFS), which additionally provides for the symbolically controlled activation of certain mental models of a "definition" of the situation according to the "Thomas theorem" ("frames") and the willingness and ability to execute certain complete patterns of action sequences ("scripts") as the starting point for every action. These topics have been discussed and empirically investigated in *cognitive social psychology* for some time, e.g. by Chaiken and Trope (1999), Fazio (1990), in *sociology* DiMaggio (1997), Miles (2015) or Vaisey (2009), Lizardo et al. (2016) could be mentioned for the

DPT, as well as for the MFS Esser (1993, 2009), Kroneberg et al. (2010), Esser and Kroneberg (2015) and, more recently, Kroneberg and Tutić (2021), Tutić (2022) and Tutić et al. (2023). There are also attempts to extend *economic* RCT in these directions, as in Rubinstein and Zhou (1999), Bicchieri (2006), or Gintis (2017) with a kind of culturalized game theory. These approaches could significantly expand the *scope* of successfully explained explananda and the conditions that can be used, as well as the *range* of explanatory power of the microfoundation of the MSE.

Overcoming the limitations of TMR would therefore lie in the further development of the *microfoundation* of the MSE in particular. Everything else that is “limited” would be questions of the valid *description* of the bridge hypotheses, the correct *measurement* of the constructs, and consistent *analytic* conclusions, especially in the transformation of the individual effects to the level of the respective collective explananda by a more complex theoretical architecture than aggregations in “Variable.-Sociology”. Nothing particularly new, therefore, or something that we would have to wait a long time for, especially not an axiom system of sociological “laws”.

## CONSEQUENCES

The outlined concept of an understanding of TMR that is *directly* oriented towards theory development and integration, as Opp was practically the only one to introduce it into the debate at an early stage, would, one can assume, have been entirely in the spirit of Raymond Boudon. He did not take it up any further however, perhaps because, for all his verbal approval of Merton’s original concept, he was obviously not really happy with it after all. He then answered the question of the possibility and the particular task of a “general”, but not “grand” explanatory sociology in a different way than by proposing a step-by-step detailed work on theories of “middle” range and scope: with the concept of *Structural Models*.

These are typifying and abstracting generalizations of successful deductive-nomological general explanations of certain particular or local empirical relationships according to the well-known HO-scheme. In addition to the prime example of such a structural model by Boudon and Bourricaud of “Exit, Voice and Loyalty” by Albert O. Hirschman (1970), the model of the race-relation cycle by Park, that of mobility traps by Wiley, of segregation by Schelling, the threshold models by Granovetter, critical masses by Oliver, Marwell and Texeira, group relations by Blau and many more could also be mentioned. They all are limited or particular in scope, but *not* in range. They are also *not* preliminary attempts, *not* well-rehearsed practices, *not* just

successful instruments only with stable evidence that are tried out or changed step by step.

Structural Models are something else than “TMR”. They are *successful*, general *explanations*, possibly also after lengthy step-by step attempts. The key is the validity and applicability of a sufficiently “general” and precise explanatory *microfoundation* and a sufficiently complex *theoretical architecture*. You certainly have also to work through this step by step to make progress. Perhaps there is no other way in science. But there is something different from the blind attempts in Merton’s proposal to work busily on the TMR without knowing in which direction it should go.

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## FORMAL MODELS IN RAYMOND BOUDON'S WORK

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This chapter discusses Raymond Boudon's use of formal models in sociological research. By formal model, I refer to models that are not statistical, such as computer simulations and game theory. To the best of my knowledge, Boudon fully developed and systematically analyzed formal models in three pieces of work. Each model is of a different type. In chronological order, they include a computer simulation (Davidovitch and Boudon 1964), a numerical simulation (Boudon 1974, chs. 4, 6), and a game theoretical model (Boudon 1977, ch. 5). The first part of this chapter describes and summarizes these three models. The second part analyzes the originality and strengths of Boudon's approach. The final section discusses its limitations and proposes ways to address them.

A few preliminary remarks are necessary. Mathematics, statistics, and simulations are deeply interconnected in Boudon's work. However, I will focus primarily on simulation models because they align more closely with my personal interests. Another reason for this emphasis is that other contributions to this book explore his game-theoretical and statistical models in greater depth. This chapter reflects my perspective, but it does not intend to be exhaustive, and other insights could complement it. Finally, beyond the works where Boudon applied formal modeling, I will draw on his writings discussing the epistemology of these models.

## FORMAL MODELS FOR EXPLANATORY SOCIOLOGY

## CASE 1: ABANDONMENT OF LEGAL PROCEEDINGS

Davidovitch and Boudon's (1964) article presents a simulation model analyzing the mechanisms behind the abandonment of legal proceedings in the

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French judicial system between 1879 and 1931. The study explains variations in abandonment rates by examining the interplay between judicial capacity, crime characteristics, and magistrates' decision-making processes. The model is built around two key decision criteria for magistrates: the likelihood of a case leading to conviction and the perceived gravity of the offense. The likelihood of a case leading to conviction (success) is determined by factors such as the availability of evidence, the identifiability of the perpetrator, and the feasibility of proving the offense. The gravity of an offense is defined as the extent of social harm it causes, which Boudon quantifies using the actual penalties imposed in historical cases, such as fines or prison sentences.

Judicial capacity is central to the model, as the system's ability to process cases is constrained and does not scale proportionally with reported crimes. Thresholds based on the likelihood of success and gravity determine which cases are pursued, with those falling below the thresholds classified as abandoned. These thresholds are adjusted annually in response to changes in crime rates and workload.

The model also takes into account the frequency and gravity of offenses. It posits that offenses considered more frequent in the judicial caseload or more severe in their social consequences influence magistrates' evaluations of which cases to pursue. For example, offenses with higher gravity may be prioritized even if their likelihood of success is relatively low. Conversely, offenses that are frequent and less socially harmful are more likely to be abandoned when resources are limited.

The model uses two main parameters: one representing the weight of offense severity and another representing offense frequency. These parameters are estimated by minimizing the distance between simulated outcomes and empirical data. The results of the simulation align closely with historical data, reproducing observed patterns of abandonment rates for different offense types over time. Boudon demonstrates that the increasing rates of abandonment can be attributed to rising crime volumes combined with relatively stable judicial resources.

From a technical perspective, it is worth noting that Boudon in Appendix III (Davidovitch and Boudon 1964, pp. 240-244) gives some details about the algorithm he encoded in the programming language Fortran. One can see the different decisions that the hypothetical judge has to take in different conditions. In this sense, the model is studied at the individual level, and one could say that the method used is *algorithm-based*.

In *Education, Opportunity, and Social Inequality* (Boudon 1974), Boudon constructs two models to analyze the relationship between education and social inequality. The first model examines how social origins influence educational achievement. Boudon distinguishes between primary and secondary effects of social origins. Primary effects are differences in academic performance influenced by family background factors, such as linguistic skills, cognitive development, and learning support, which favor children from higher socioeconomic backgrounds. Secondary effects occur when children with equivalent academic results make different educational choices based on their social origins. This is explained through mechanisms such as opportunity cost, where continuing education imposes a heavier financial burden on lower-income families, and reference group effects, where aspirations are shaped by norms typical of one's social environment. Together, these effects generate educational inequalities.

The second model focuses on how educational credentials are converted into occupational positions. The labor market is modeled as a queuing system with a dominance effect: individuals with higher educational qualifications are prioritized, and among those with equal qualifications, individuals from higher social origins have an advantage. The labor market has a finite number of hierarchically ranked positions. As access to education expands, the supply of highly educated individuals increases, but the number of high-status positions does not follow. Consequently, the absolute value of educational credentials decreases, while their relative value remains.

In the first model, Boudon obtains fictitious educational credential distributions by multiplying an educational achievement distribution with a distribution of survival chances at each bifurcation point – both being dependent on social class of origin. He proceeds in a similar fashion in the second model, where he uses educational credential distributions obtained in the first model and allocates this in a distribution of social positions. Again, he creates a probability distribution of obtaining the different positions as a function of the educational credential and the social class of origin (reflecting the dominance effect). Positions in the top category are filled first until there is no more space, and the second-highest positions are opened, and so on.

It is worth noting that, although the model is formulated at the individual level, it is analyzed at a higher level of aggregation: the group level (Manzo 2014). The transition from one distribution to another does not require going down to the individual level; thus, one could qualify the method used to analyze this model as *distribution-based*.

### CASE 3: THE LOGIC OF RELATIVE FRUSTRATION

Boudon's model of relative frustration (Boudon 1977, ch. 5, Boudon 1979) explains how competition generates dissatisfaction using a lottery framework. The model assumes a limited number of rewards, with the probability of winning decreasing as more participants enter, as winners are chosen randomly from the participants.

Boudon compares two scenarios. In the first scenario, there are many winners, and the expected gains from participating always exceed those from abstaining, making participation the dominant and rational strategy. This leads to universal participation, with more winners but also more losers. These losers, who rationally chose to play, are assumed to experience significant frustration. In the second scenario, there are fewer winners, and the expected gains from playing and not playing are equal. Without a dominant strategy, individuals decide randomly, leading to about half the group participating. This scenario results in fewer winners and losers, and less frustration among losers, as their decisions were based on randomness in the absence of a dominant strategy.

The model shows that situations with more winners and participants can paradoxically generate greater frustration among losers compared to those with fewer winners and lower participation, a phenomenon observable in various historical contexts according to Boudon.

To study the model, Boudon identifies the conditions under which the model is able to produce the paradoxical outcome he is interested in. From a methodological point of view, it is a game theoretical model where the mathematical analyses are mixed with some specific numeric examples.

### STRENGTHS OF BOUDON'S MODELING APPROACH

#### FORMAL MODELS AS A TOOL FOR EXPLANATORY SOCIOLOGY

Boudon employed three distinct modeling techniques, which I have labeled algorithm-based, distribution-based, and game-theory. The trajectory of his work is noteworthy. His first formal model was closely tied to empirical data, involving parameter estimation by minimizing the distance between simulated and observed data. For a project conducted in the 1960s, this approach was ambitious given the technical limitations. The explanandum was specific and concrete. In his second application (Boudon 1974), while still engaging with empirical data, Boudon aimed for a higher level of abstraction. As he explained later in his debate with Hauser (Boudon 1976), the model was not intended to fit a particular data set or replicate a specific situation. Technically,

the models were simple numerical simulations. The goal was to reflect general characteristics of educational systems and labor markets, providing an abstract explanation for common empirical patterns. The last application is even more abstract (Boudon 1977, ch. 5, Boudon 1979). The model aimed to explain qualitative patterns drawn from sociological literature, particularly Tocqueville and Stouffer. It sought to capture shared features across disparate concrete situations, such as pre-1789 France and the US police and military forces in the 20th century. The explanandum was qualitative, and the game-theoretical structure served to generalize insights across cases. Whether the paradox Boudon addressed actually exists has been debated (Berger and Diekmann 2015).

In my opinion, this shows that Boudon's use of formal models was instrumental: he selected modeling techniques based on the specific purpose of each study rather than adhering to a single type. In this sense, Boudon was more pragmatic than dogmatic, and it probably reflects the idea that a model's value depends on its purpose. Boudon's trajectory moved from concrete models explaining specific phenomena to more abstract models applicable across multiple contexts. However, common to all applications is Boudon's view of formal models as tools for explaining puzzling social phenomena. Two words are central here: puzzling and explaining. He championed sociology as an explanatory science, contrasting it with descriptive, critical, or expressive forms of sociology (Boudon 2002). Yet, he recognized the foundational role of descriptive sociology, evident in his careful engagement with literature and empirical data sets in his first two applications. He treated statistical and formal models as complementary, with statistical models aiding description and formal models providing generative explanations: "We must go beyond the statistical relationships to explore the generative mechanisms responsible for them. This direction has a name: theory. And a goal: understanding" (Boudon 1976). Boudon also emphasized the importance of addressing puzzling and intriguing topics which do not always have an immediately apparent explanation. His selected topics – relative frustration, social inequality and mobility, and judicial processes – demonstrate his interest for significant and challenging sociological questions.

#### FORMAL MODELS AS A COUNTERFACTUAL TOOL

One of the strengths of Boudon's approach is to use formal models as a counterfactual tool. To illustrate this, let us revisit his first model from Boudon (1974). As discussed earlier, the model identifies two channels through which social origin affects students' educational attainment: the primary effect, which

directly influences academic performance, and the secondary effect, where students from different social backgrounds but with equal academic results have unequal probabilities of continuing their studies. While these effects can be statistically estimated, Boudon's model allows for the quantification of their macro-level consequences, something hard to achieve given the dynamic nature of the inequality-generating process.

Notably, Boudon simulates a scenario in which the primary effect is eliminated and demonstrates that significant inequalities in educational attainment would persist. The simulation highlights the dynamic nature of the secondary effect: unlike the primary effect, which occurs only once, the secondary effect operates at multiple bifurcation points in the educational system. This repeated operation leads to multiplicative consequences, amplifying inequalities over time.

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The strength of this method lies in its ability to manipulate generative mechanisms – defined as entities, activities, and their interactions in a dynamic system – within the model (although I will stress below that interactions are largely missing in Boudon's work). Assuming the model accurately captures the essential components of the real-world system, one can isolate and deactivate specific mechanisms to study their macro-level impact. This allows Boudon to conclude that erasing the primary effect alone would not resolve educational inequalities.

As I have argued elsewhere (Sage 2022), this approach represents a distinct form of counterfactual reasoning from the notion of counterfactuals used in statistical literature and the potential outcome framework (Morgan and Winship 2014). To understand why, let us assume the existence of a 'true' model that is responsible for the real-world phenomenon we want to study. Let us refer to this as the real-world data-generating process, composed of a set of mechanisms. Now, if we believe that: first, individuals are interdependent because they interact, share information, and influence each other; and, second, that they react to changes in their environment, then we admit that the mechanisms' effects are interdependent: changing the strength of one will change the effects of others. Crucially, this means that the relationships between variables are not fixed but are themselves the product of a dynamic process. When one element in an interconnected system changes, it doesn't just have a direct effect – it ripples through the system and changes how other elements relate to each other. The empirical data and the relationships between variables that they contain are only one realization of the real-world data-generating process, at one point of the true parameter space. Thus, one understands that the interdependence of the mechanisms poses fundamental challenges to the potential outcome approach to counterfactual reasoning

which amounts to asking “What would happen if we had changed X in the system?” To overcome this issue, the potential outcome framework proposes to leverage exogenous variations that submit some individuals to the change (also called the treatment) of interest and not others, and then to compare the average outcome of the two groups, with the underlying assumption that everything else remains constant. Certain formal models (that I will detail below) can offer another possibility: explicitly mimicking the generative mechanisms purported to be at play in the real-world data-generating process with its interdependencies, to then intervene on the system and derive the consequences. This alternative approach directly models the interdependent mechanisms rather than trying to work around them. Boudon’s counterfactuals are a first attempt in this direction (for a deeper discussion of the different understandings of mechanisms, see Manzo 2022, ch. 1).

#### OVERCOMING THE “GENERATIVE SUFFICIENCY IS NOT SUFFICIENT” CRITIC

A major problem that formal models face is the question of their external validity. How can we ensure that what occurs in the model reflects aspects of the real-world data-generating process? A frequent critique faced by formal modelers is captured by the statement: “generative sufficiency is not sufficient”<sup>1</sup> (León-Medina 2017). In other words, how can we establish that the mechanisms within the model resemble those in the real-world system the model aims to mimic? Critics argue that modelers can freely adjust their models to produce the desired outcomes, unlike statistical methods, which are more constrained by externally given data and the relationships between variables within it. This is a significant critique, and although Boudon did not explicitly address the degree of similarity between a model’s mechanisms and real-world mechanisms, his work offers some answers to mitigate this critique. Boudon emphasizes building models with micro-level behavioral assumptions that are plausible and grounded in existing knowledge. This involves injecting as much accumulated empirical and theoretical knowledge as possible into the model. It is not the model’s role to prove the existence of its mechanisms; rather, it is the modeler’s responsibility to draw on the existing literature and evidence.

1 I here quote León-Medina (2017) because he coined this expression. However, the point he actually makes is rather different: he insists on the necessity to understand the way in which an agent-based model produces the outcome of interest, that is to understand its internal dynamic. However, my point is more that several models with a diversity of mechanisms can produce the same outcome, and that the question is about understanding which mechanisms were actually at play in producing the outcome in the real world.



Boudon (1974) and Davidovitch and Boudon (1964) exemplify this approach. In both cases, the authors engage deeply with their subject matter, discussing expert knowledge and carefully analyzing empirical data before proceeding to simulation. For example, Boudon (1974) discusses mechanisms responsible for primary and secondary effects in depth, and reviews multiple sources of evidence and literature that support them. Similarly, in the article on legal proceedings, Boudon explicitly states his assumptions about the behavior of magistrates and the context, showing how these assumptions are incorporated in the model.

Boudon's clarity and transparency in model construction are exemplary. He begins by articulating and justifying the assumptions, proceeds to their formal representation in the model, and then examines their dynamic interactions. This meticulous process ensures the plausibility of the mechanisms modeled and enhances the credibility of their results.

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Overall Boudon's work offers several lessons that remain relevant today: formal models can illuminate important and puzzling topics by formally modelling their underlying mechanisms. Interdependencies between model mechanisms should be taken into account for models to serve as counterfactual tools. Empirical and expert knowledge should inform the selection of mechanisms.

## LIMITATIONS

### INTERACTIONS VS. INTERDEPENDENCE

After having stressed the strengths of Boudon's approach to formal models, I would now like to turn to some of its blind spots. A key interest in Boudon's second (Boudon 1974) and third (Boudon 1977) formal models lies in their ability to generate unintended emergent effects, or composition effects as Boudon called them. Composition effects occupy a central place in Boudon's work (Boudon 1977, 1981) and they can be defined as macro-level consequences of individual actions that no single individual intended or desired to create. In this section, I would like to point out that Boudon only considered one form of composition effects deriving from broad interdependence between individuals and disregarded composition effects stemming from local interaction structure. In Boudon's 1974 model, which examines the link between the distribution of diplomas and occupations, interdependence arises because the occupational structure is predefined, meaning there are not enough positions for everyone. Similarly, in the relative frustration model, the number of winners is exogenously fixed. In both cases, rewards are limited,



which is what creates the interdependence: an individual's chances of obtaining a reward depend on how many others succeed or fail. All individuals' outcomes are interconnected in this sense.

However, another form of interdependence is absent from Boudon's models: local interdependencies. This notion can be illustrated using Schelling's model of residential segregation (Schelling 1971). In this model, individuals of two ethnic groups are distributed randomly on a grid. Each individual is content with their location, so long as a certain proportion of their direct neighbors belong to the same group. If this condition is unmet, then they move to a vacant spot. Patterns of segregation emerge even when individuals have mild preferences for diversity. This is caused by the following phenomenon: although most agents are originally satisfied, it is always the case that, by chance, a few agents will find themselves in a neighborhood with an overrepresentation of out-group members. Those agents will thus be unsatisfied and move. Yet, by doing so, they change the ethnic composition of the neighborhood they leave as well as the one they move to. This can make agents living in their previous and new neighborhoods passing from originally satisfied to dissatisfied because of the change in ethnic compositions. The new and old neighbors can, in response to these changes, move again, changing even more neighborhoods compositions and so on. The cascade toward high levels of segregation is inevitable, even though nobody desired it, and agents could have been equally satisfied in a non-segregated world.

This phenomenon highlights an important difference: the composition effect in Schelling's model is not due to limited rewards, as there exist configurations where everyone could be satisfied, and yet there would be no segregation. Instead, the composition effect is due to local interdependencies. If agents considered the entire grid's ethnic composition rather than their local neighborhood, a cascade towards segregation would not arise. Local interactions, not scarcity, drive the emergent effects.

It is interesting to compare Boudon and Schelling, as they were contemporaries, and Boudon was aware of Schelling's work. In his response to Hauser, Boudon compared his approach to Schelling's (Boudon 1976): "My purpose in this respect was similar to Schelling's: to show that equalization of opportunity does not necessarily mean equalization of results in an ideal-typical world, one reduced to some basic mechanisms similar to those which can be observed in the real one." He also cited Schelling's model as a typical example of a composition effect (Boudon 1981, ch. 4). However, Boudon did not appear to recognize the difference between the sources of composition effects in the two models. He seemed less concerned with the local structure of agent interactions, as social networks are absent from his work. In a late

article, he even noted: “Networks are today a popular topic of sociological research. However, they are often treated in a merely descriptive or mechanical fashion, while a connection with the theory of ordinary rationality would make network research more fruitful, as many classical and modern sociological works suggest” (Boudon 2012).

This is surprising because early in his career, Boudon (1965), in an article derived from his doctoral dissertation, highlighted the potential of computer simulations to make mathematical models more realistic. He specifically mentioned diffusion models, noting that simulations could replace the assumption of random encounters with more realistic interaction structures, referencing Hägerstrand (1965). This is a crucial point: compared to mathematical models, simulations allow researchers to relax simplifying assumptions and move toward greater realism, but Boudon did not implement this possibility himself.

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#### WHERE ARE THE INDIVIDUALS AND HOW DO THEY ACT IN BOUDON’S MODELS?

In the second part of his career, Boudon moved away from formal modeling and focused on developing a theory of action consistent with his version of methodological individualism (MI), which he called “cognitive” or “ordinary rationality” (Boudon 1998, 2012). Boudon’s MI principles can be summarized into two key points: first, aggregate phenomena must be explained as the product of individual actions; and second, individuals act based on subjective “good reasons” that can be shaped by their context. It is instructive to assess whether Boudon’s formal models adhere to these principles.

Boudon’s game-theoretic model of relative frustration (Boudon 1977, ch. 5, Boudon 1979) incorporates individual behaviors through a representative agent, but it employs a narrow definition of rationality, focusing on dominant strategies with higher expected payoffs.

In Davidovitch and Boudon (1964), the presence of individual actors is less clear. One could argue that the model implicitly includes a representative judge making decisions for the entire system. The decision-making rules incorporate forms of good reasons, as the model allows the representative judge to adjust decisions based on changes in context, such as an increase in case volume.

In his most influential work (Boudon 1974) the models are formulated at the individual level but are analyzed at the aggregate level of groups, as noted by Manzo (2014). The primary and secondary effects described in the model are not mechanisms themselves but outcomes of underlying mechanisms. For instance, the secondary effect arises because families from different socio-

economic backgrounds evaluate education differently and have unequal resources. These mechanisms are condensed into probabilities of educational transitions, which serve as the only explicit behavioral rule in the model. Actions are highly abstracted and do not explicitly represent the decision-making processes or reasons behind them.

One could argue, provocatively, that the explanatory power of Boudon's second model in *Education, Opportunity, and Social Inequality* (1974) stems more from its structure – where individuals are represented as marbles moving into boxes with limited spaces – than from individual actions or their reasons. The explanation relies on systemic constraints, such as the dominance principle and the predefined number of spaces, rather than emergent phenomena from individual interrelations. In essence, actors are moved by external rules rather than acting themselves.

This abstraction affects the robustness of conclusions drawn from counterfactual scenarios. In an interdependent system, changes to the rules of the game, such as altering the number of educational transition points, would likely cause agents to adapt their behaviors differently based on their socioeconomic backgrounds. This adaptation, absent from the model, limits the reliability of its counterfactual predictions.

Boudon, a careful student of classical sociologists, often highlighted the gap between Durkheim's methodological recommendation to "explain the social by the social" (Durkheim 1982) and Durkheim's actual practice, which Boudon saw as a precursor to MI. To some extent, the same critique applies to Boudon's formal models: they do not always align with the principles of MI he advocated. More precisely, although they are sometimes formulated at the actor level and conform with ordinary rationality principles, the analyses of the model move on to another level where actors are no longer explicitly present.

## DISCUSSION

Boudon is widely regarded as a pioneer of analytical sociology and modern sociological science (Goldthorpe 2021). According to Goldthorpe, Boudon once declared having the feeling of having written only one book (Goldthorpe 2021, ch. 9). While there is an undeniable continuity in his oeuvre, it is reasonable to divide his career into two phases. In the first, Boudon developed influential formal models that earned him international recognition. In the second, he focused on establishing the principles of his version of MI and his theory of ordinary rationality, which he saw as intrinsically linked. Unfortunately, Boudon abandoned formal modeling during this later period. As I have argued, none of his earlier formal models fully aligned with the

MI framework he later championed. Boudon never achieved a synthesis between his early work as a modeler and his later theoretical developments in ordinary rationality.

Interestingly, Boudon (1965) had early insights into the potential of realistic simulation models for quasi-experimentation, or counterfactual analysis. A model that incorporates agents' cognitive decision-making processes could allow for adaptive agents who react dynamically to changes in the game's rules. Boudon's theory of ordinary rationality could serve as a foundation for modeling such behaviors. While some scholars doubt the feasibility of a predictive and useful theory of individual action (Hedström 2021; Watts 2014), advocating for influence-response functions instead of cognitive models (Lopez-Pintado and Watts 2008, for a critical response to these ideas (see, e.g., Opp 2024), Boudon believed ordinary rationality could fulfill this role, but never fully integrated it into his models. Adding locally structured interactions within realistic social networks to these models would also further enhance their power as quasi-experimental tools.

Agent-Based Models (ABMs) offer a promising avenue for achieving this synthesis. ABMs' flexibility and capacity to model diverse behaviors make them ideal for integrating Boudon's theory of action into individual-based models with local interactions. Unfortunately, Boudon neglected ABMs, just as he overlooked the distinction between composition effects stemming from global interdependencies and those arising from local interactions. This neglect is surprising given his early familiarity with ABMs. Boudon referenced Hägerstrand (1965) and Schelling (1971) in his early work (Boudon 1965, 1976) and developed a sophisticated simulation in Davidovitch and Boudon (1964).

Boudon's lack of interest in ABM is evident in his discussion of Manzo's (2009) ABM of educational inequalities. Boudon (2010) mentions Manzo's work as merely adding a social network component and a France-Italy comparison to his own model. He fails to recognize that Manzo's ABM moved beyond mere technical refinements. Thanks to the ABM approach, in Manzo's work, the micro-mechanisms are modeled at the level of the actors themselves – actors who can be heterogeneous, proceed to cost-opportunity calculations, make autonomous decisions, and influence each other. In other words, the probabilities of transitioning at various bifurcation points emerge endogenously, unlike the exogenously set probabilities in his own model. The lack of interest for ABMs is revealed in Boudon's (2012) critical assessment on the development of analytical sociology: "I have the impression, though, that the handbooks on 'analytical sociology' insist on secondary technical details and fail to clearly identify the common paradigm that underlies many

illuminating sociological works [...]” (Boudon 2012). He categorized ABMs as such secondary details, which is surprising for someone who ardently advocated for MI.<sup>2</sup> It is interesting to note that unlike Boudon, Tom Fararo, another pioneer of analytical sociology from the same generation, recognized the methodological value of ABMs, and did this even before the analytical sociology movement popularized their potential (Manzo 2024).

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2 One could argue that his game-theoretic application met the criteria of MI. Game theory is obviously another valuable methodological tool but as explained above, it is more appropriate to ideal-typical situations than to more concrete ones.

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PART III

**SOCIOLOGY OF SOCIAL  
STRATIFICATION**



# INEQUALITY OF EDUCATIONAL OPPORTUNITY: *L'INÉGALITÉ DES CHANCES* FIFTY YEARS LATER

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Reading the English translation of *L'Inégalité des chances* 50 years after its publication affords a number of insights into the changing situation of sociology as a discipline. The book appeared at what seems to have been a high point in post-war sociology when the discipline was much more central to intellectual debate than it has been since. French thinkers, in particular, enjoyed an enviable position: structuralism, exemplified by Levi-Strauss, was enjoying a surge in popularity in the English-speaking world, as was the Marxism of Althusser and his followers. As for Boudon's book, great excitement surrounded it, as even Robert Hauser, in an otherwise highly critical review, acknowledged:

Not since the publication of Jencks's *Inequality* has a book so clearly captured the interest and attention of students of social stratification. At the Eighth World Congress of Sociology in Toronto, the hallways fairly buzzed with favorable anticipation. Moreover, in S. M. Lipset's laudatory foreword, we read, "In this volume, sociological theory comes of age" (Hauser 1976, p. 911).

I believe that a large part of the attraction of the book was that, on the one hand, it presented a puzzle, and, on the other, it saw the solution to this puzzle in understanding society, or, in this case, parts of society, as a system, a set of inter-related parts, which were thought of as the product of a set of simpler, basic relationships and processes. Thinking about society as a system was in vogue at this time. It took a range of different forms: Levi-Strauss's structuralism and Althusser's structural Marxism are examples, but so is work derived from von Bertalanffy's (1968) general systems theory. Boudon's work, although obviously of a very different kind to these examples, analyses educational and occupational inequality as elements of a system, and this systemic approach is one of the major contributions of the book. Rather than analysing inequality of educational opportunity (IEO) and inequality of social opportunity (ISO)

separately, he asks what relationship they have and, in particular, how we might explain the apparent puzzle with which he begins the book: why has ISO remained unchanged even though IEO has declined? An interesting contrast here is with the contemporary approach of scholars such as those of the Wisconsin school. They were concerned with explaining individual-level educational and occupational attainment in terms of sets of individual-level predictors. In contrast, Boudon was interested in the aggregate properties to which the underlying processes generating IEO and ISO, and their interaction, give rise. Boudon (1974, p. 18, n. 8) himself writes, "This 'system approach' to social mobility is hardly new. It can be found in Sorokin (1927) as well as in Kahl's (1957) work."

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Much modern sociology has moved away from this sort of systemic thinking. We have become at once more modest and more specialised in our ambitions: the days of grand theory, for example, are certainly long gone; empirically minded sociologists do not speak about society as a whole, but, rather, prefer to address specific questions, often ones of relevance to policy makers. This might be sensible, but it probably makes the discipline seem less exciting to would-be students. Further, to lay people, it is often sociologists such as Bauman (2001) and Castells (2000) or writers such as the Korean-German commentator Byung-Chul Han (2015) that seem more compelling because they claim to capture the fundamental dimensions of the age in which we live (even though frequently devoid of empirical support). Writers like this are producing what Boudon (2002, p. 372, 375) later called "expressive or aesthetic" sociology, whose aim is to provoke an emotional recognition in the reader of the conditions and circumstances that the author brings to light.

Another lesson the book teaches us about sociology is how far it has advanced methodologically, especially in terms of the quality and amount of data available. Boudon repeatedly seeks evidence for his claims, but the data he uses is often fragmentary and not well-suited for his purposes.

In this paper, I shall focus on the early chapters of *Education, Opportunity, and Social Inequality*, which deal with IEO. However, I want to begin by making some remarks concerning Boudon's overall motivation for the book.

## THE PUZZLE

Boudon (1974, p. XIII) begins with a puzzle that he sets himself to solve: "all Western industrial societies have been characterised since the end of World War II both by a steady decrease in IEO and by an almost complete stability of ISO. Why is that so?"

Boudon (1974, p. xi) defines IEO as “differences in level of education and attainment according to social background”. He defines ISO as “differences in social achievement according to social background”. The former term is still widely used, but in both cases, how they are measured has changed in consequential ways. Boudon measures IEO and ISO mostly using differences in the percentages achieving a given outcome (educational attainment or adult social status) among those from different social backgrounds. This would probably not be the preferred measure today. At much the same time that Boudon was working on his book, Leo Goodman (1969) and others were developing log-linear models for the analysis of contingency tables, which mobility researchers very quickly adopted. So today we would probably examine odds ratios rather than differences in proportions or percentages, and this is usually justified by the margin-insensitivity of the former.

Boudon’s solution to the puzzle was that, while education is free to expand through the choices of individuals and families, the availability of achieved statuses is limited by economic constraints; hence, the supply of well-qualified people exceeds the supply of commensurate positions. The main problem with this conclusion is that there is plenty of empirical evidence, some of which would have been available to Boudon at the time, that his puzzle did not really exist: as Hauser (1976) pointed out in his review of the book, Boudon ignored a lot of relevant work, especially from the US. Did IEO decline? In some countries, it did, but one of those that Boudon mentions several times, namely the US, runs contrary to his claim. It has now been well established that IEO in the US has remained largely stable for the past century (Featherman and Hauser 1978; Mare 1981; Hout and Janus 2011; Hertel and Pfeffer 2020). Whether IEO declined elsewhere has been much disputed by scholars of stratification. In the 1990s and 2000s, there was a consensus that, in the words of Shavit and Blossfeld (1993), there was “persistent inequality” in educational attainment in developed countries over the 20<sup>th</sup> century. More recently, the contrary view has been advanced: in work with Walter Müller, Reinhard Pollak, and Ruud Luijkx (Breen et al. 2009, 2010), I have argued that there was a decline in many European countries in IEO in the third quarter of the 20<sup>th</sup> century.

Was ISO constant? It is difficult to understand why Boudon would have thought it was, given that, at the time he was writing, France was enjoying *les Trente Glorieuses* and that economic growth and upward mobility were common throughout the developed world in the thirty years after the end of the Second World War. As Breen and Müller (2020, p. 289) concluded in their study of social mobility in Europe throughout the 20<sup>th</sup> century:

Perhaps our most striking finding is the sharp contrast between the fortunes of people before and after the 1950s. Among those born in the second quarter

of the 20th century, rates of intergenerational mobility increased: more people came to occupy a place in the class structure different from the one into which they had been born. In particular, upward mobility rates increased as positions at the top of the class structure, in the service class or 'salarial', became more numerous, with a growing surplus of service class destinations over service class origins. At the same time, social fluidity increased: the chances of entering a more desirable class, and avoiding a less desirable one, became more equal between people of different class backgrounds.

## THE MODEL

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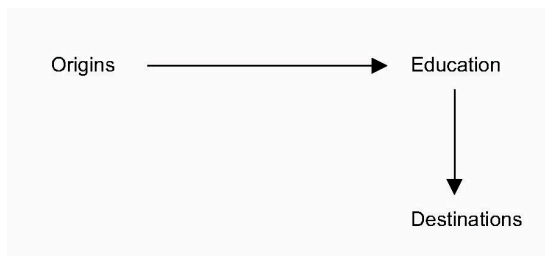
In speaking of "all Western industrial societies ... since the end of World War II" Boudon explicitly supposes that IEO and ISO are generated in these places by "mechanisms that are, broadly speaking, common to all" and thus he presents a single, ideal type, in the form of a model which simulates sets of tables which, he argues, capture the most important empirical aspects of IEO and ISO. Boudon treats ISO and IEO and their relationship as an example of what he called "cognitive" sociology. This is sociology which seeks to explain a puzzling phenomenon, and it stands in contrast to much research on education and inequality that is fundamentally "cameral" – that is, driven by the desire to be "useful" and, particularly, to have relevance to policy. Boudon does not entirely eschew policy, but it is far from central to the book.

Chapter 1, "Level of Educational Attainment and Mobility," presents an example of his expository and explanatory strategy through the use of a set of simulations that link social background, educational attainment, and attained social status, or, as we would say today, origins, education, and destinations. The immediate motivation is what Boudon refers to as two puzzling findings, both of which relate to the same problem: how can it be that some men who are more educated than their father nevertheless end up in a lower social position than their father? Specifically, he refers to the low observed correlation between son's educational level relative to his father's and son's status relative to his father's (the "Centers-Anderson structure", CA, as Boudon terms it) and the low correlation between son's educational level and his social status relative to his father's (the "Boalt-Anderson structure," BA). How can these be, Boudon asks, if, as is generally accepted, industrial societies are largely meritocratic and education is a major determinant of realised status?

He tries to answer this question by building a model of the mobility process, based on some simplifying assumptions. The first is that "achieved social status depends only on educational level" (Boudon 1974, p. 10) and not directly on

origin status. Encoding this assumption in the form of a directed acyclic graph, DAG, makes it transparent:

Figure 1: Directed Acyclic Graphical Presentation of Boudon's Model of Intergenerational Mobility via Educational Attainment



Using this and other assumptions, Boudon's simulation generates mobility tables that capture the main features of observed mobility tables and replicate these paradoxical results. The simulation shows that, despite meritocratic selection into achieved status, neither higher absolute nor higher relative (to one's father) education ensures that a person will achieve a higher status than their father.

He then presents three fictitious tables which he claims capture the main features of mobility tables in Western societies; they show the relationship between sons' social background (which is the same as their fathers' achieved social status) and their educational attainment (OE), between sons' educational attainment and their own achieved social status (ED), and between their fathers' education and achieved social status (say, FO where O from a son's point of view is also his father's D). Given Boudon's assumption encoded in the DAG above, these three tables are sufficient to generate the sons' mobility table showing their social origin by their achieved status.

In generating these tables, Boudon makes a number of assumptions concerning the marginal distributions of the four variables. He assumes three categories of education and three of status, with the numbers in each category increasing as we move from highest (status or education) to lowest. He assumes that, comparing fathers and sons, the lowest category declines while the other two increase with the largest absolute increase in the middle and high categories, but with greater growth in education than in status.

He generates two-way tables from the marginal distributions via an algorithm that biases the allocation of more educated persons to higher status outcomes according to a single parameter (Boudon 1974, pp. 8-9). In the OE tables, this is a "bias parameter" because it tells us how far people from

higher status origins are advantaged in the competition for higher educational positions. However, in the ED table, it is called a “meritocratic parameter” because it tells us how advantaged the higher-educated are in terms of access to higher status destinations. This is set to 0.8 for the background-to-education tables for fathers and sons and to 0.7 for the education-to-status table for sons.

Boudon shows (Boudon 1974, Tables 1.5, 1.6, 1.7, Figure 1.1) the resulting two-way tables and also the four-way table (fathers’ education by fathers’ status/ sons’ origin by sons’ education by sons’ status) implied by these and the DAG. In Table 1.8, he shows the generated CA table, and in Table 1.9, the generated BA table, both of which have the paradoxical features he earlier noted in the work of Anderson and others. The paradox they show can be summarised as follows: despite meritocratic selection into achieved status, neither higher absolute nor higher relative (to one’s father) education ensures that a person will achieve a higher status than their father. Boudon points to two features that lead to this outcome: first, because education depends on social origin, ceiling effects limit how many of the highest educated can acquire a status higher than that of their father, and second, the discrepancy between the sons’ educational and sons’ status distributions ensures that many highly educated sons cannot acquire a high status position. In fact, it is apparent that these findings are mainly driven by the different marginal distributions of fathers’ and sons’ education, and of sons’ education and status. These discrepancies ensure that, no matter how large the bias parameters, there will always be cases of downward mobility, whether this is of the CA type (relative status compared to relative educational attainment) or the BA type (relative status compared to absolute education). The only circumstances in which neither would be observed would be if the various tables displayed marginal homogeneity and there was a perfect association between the pairs of variables in each table. This would place all the cases on the main diagonals of the tables. That there is not marginal homogeneity in these tables, Boudon argues, is because the distribution of social status is determined by exogenous factors, whereas an individual “can go to college if he wants to, provided he is qualified” (Boudon 1974, p. 21).

Nowadays, we would be less concerned with tables of relative position and would look directly at the origin-destination mobility table. I have generated the mobility table from Boudon’s data,<sup>1</sup> and a striking feature of it is that the OD

1 Because Boudon assumes that the son’s destination status depends on his origin only via his own education, the origin by destination status (O by D) table is the product of the origin by education (O by E), and education by destination (E by D) tables. Boudon presents the OE table (Boudon 1974, p. 8, Table 1.5) but one can generate it from the marginal distributions of O and E and the bias parameter by



association is not independent of the bias parameter in the OE relationship. Using the intrinsic association parameter from the OD table as our measure of ISO (Bouchet-Vallat 2022), Boudon's parameters yield a value of 19. However, if we reduce the bias parameter (leading to less IEO), the value falls: for a bias parameter of 0.7, the intrinsic association parameter takes the value 16.7, for 0.6, it has the value 13.9, and for 0.5, 11.3. So, in fact, Boudon's own model suggests that reducing IEO also reduces ISO (bearing in mind that this means IEO and ISO as we would probably measure them nowadays).

In chapter 2, Boudon begins his analysis of change over time in IEO. After a review of different approaches to explaining educational differences by social origin, he presents his own theory, which sees IEO as a function of primary and secondary effects. Primary effects: "the lower the social status the poorer the cultural background – hence the lower the school achievement". Secondary effects: school continuation decisions will vary by social background, even with primary effects held constant. Here he draws heavily on Keller and Zavalloni (1964) to argue that more advantaged children will choose more ambitious educational options because the benefits to this choice increase in family social status and the costs correspondingly decline. In chapter 3, he draws on a wide range of what he terms "school bookkeeping data", largely from the OECD, and argues that it supports his theory.

In chapter 4, he presents "A Dynamic IEO Model", the idea of which will be familiar to contemporary sociologists of educational inequality, in that he presents the educational career as a sequence of binary decision points at which a student must decide whether to remain in education or leave or whether to take a more or a less ambitious educational option. He bases this on several assumptions. First, he assumes three social classes,  $c$ , whose educational decisions are to be compared. He assumes that their initial educational attainment,  $a$ , is taken to be a function of their social origins (call these conditional probabilities  $q|c$ ): these capture primary effects. He further assumes that their probability of choosing one or other of the options at each decision point depends on both their social class and their initial attainment (call these  $p|c, a$ ). These are secondary effects. The  $p|c, a$  apply unchanged to all decision points and in Boudon's model there are eight of these.

From these basic assumptions, Boudon can generate a set of what we now call hazard rates and survivor functions for the educational career of students from different social classes. In chapter 5, he implements this model and draws

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following the algorithm he presents on pages 8 and 9. The same applies to the ED table (given in Table 1.6, p. 9). Transforming the two tables of frequencies to outflow tables and multiplying them yields the OD mobility table.

attention to a number of results. In particular, it is striking that class differences in the probability of reaching a given educational level widen as one moves to higher levels. Using his measure of class disparities (the proportion reaching an educational level among students from the highest compared to those from the lowest social class origin), inequality in at least gaining entrance to college is 1.8 (the former have a probability 1.8 times greater than the latter). For graduating from college, the disparity is 3.7, while the disparity for attaining less than high school is only 2.

128 Boudon also wonders whether primary or secondary effects have the greater impact on these disparities, and he investigates this by running a simulation in which students from all class origins are assigned the same distribution of  $q|c$ , implying that there are no primary effects. The disparities are quite substantially smaller: for entry to college, 9.8 compared to the original 1.8, for college completion, 1.9, and for high school completion, 1.6. Boudon's interpretation of these results, however, strikes me as rather odd. He focuses on the fact that disparities in the simulation still remain high, rather than on the magnitude by which they have been reduced and writes: "we must also accept that the secondary effects of stratification on IEO are, other things being equal, probably much more important than their primary (cultural) effects" (Boudon 1974, p. 84).

Surprisingly, Boudon does not simulate the opposite scenario – no secondary differences by class while preserving the initial primary differences. However, if one does this (as I have), one finds support for Boudon's argument: at all points in the educational career, the class disparities are less than 2.

Finally, Boudon uses this sequential model to simulate changes over time, increasing the probabilities of making each educational transition by 10 percent. He claims that these simulations show IEO declining over time, but, as Hauser (1976, p. 922) pointed out, "the main results of Boudon's variation in survival rates across cohorts are an upward shift in the educational distribution for everyone and an increase in its dispersion. Boudon's fictitious data do not show substantially more equality of opportunity in later cohorts".

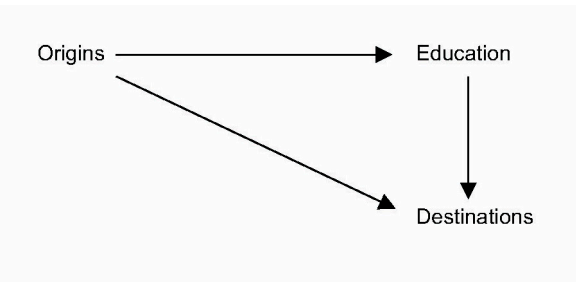
In chapter 6, Boudon draws together some conclusions from his models. The two most important are the following. First, as we have just seen, he argues that secondary effects are much more important than primary effects in generating IEO, and, following from this, his second conclusion is that society, not school, is chiefly responsible for IEO (Boudon 1974, p. 114). Even if schools reduced primary effects, high IEO would still be found. Therefore, greater equality of condition is needed to reduce IEO: "the best strategy seems to lie ... rather than inside schools, in social and economic change rather than in educational change" (Boudon 1974, p. 115).

# ASSESSMENT

Turning first to Boudon's (1974, p. 11) simulation model of O, E, and D, he claims that the conclusions of his simulations do not depend on the exact parameter values chosen and that the same conclusions would be reached given other values "not too remote from the ones given here". Although this is likely true, he does not demonstrate it, either by using data to justify his choice of parameter values or by varying their value to examine the robustness of his results.

Boudon's (1974, p. 10) main assumption, in his simulation model, is that "achieved social status depends only on educational level". He admits that this does not accord with empirical findings, but argues that the "residual influence of social background is very often weak in comparison to the influence of social background on level of education or of level of education on achieved status" (Boudon 1974, p. 10). It is difficult to see how even he could have justified this conclusion, given that he was writing after the appearance of Blau and Duncan's *The American Occupational Structure* (1967) and Christopher Jencks et al.'s (1973) book, *Inequality: A Reassessment of the Effects of Family and Schooling in America*. Certainly, this assumption would not be considered tenable nowadays when there is a whole sub-branch of the study of educational inequality concerned with what it terms "DESO" (direct effects of social origin: see Ballarino and Bernardi 2016). Figure 2 adds this edge, directly linking origins and destinations, to Figure 1. In some cases, the DESO has been found to be larger than the indirect effect of social origins mediated via education. While agreeing with Boudon that education is the most important single factor shaping social status, researchers nowadays would place much greater weight on the role played by other factors related to social background.

Figure 2: Directed Acyclic Graphical Presentation of the Origin – Education – Destination (OED) Triangle, Including the Direct Path from Origins to Destinations



Boudon, in several places, claims that the data support his simulation results when, in fact, they would also have supported a range of possible models of IEO. DESO is a case in point. The full OED triangle shown in Figure 2 suggests an alternative explanation for why declining IEO does not lead to declining ISO. While IEO causes the indirect effect of origins on destinations to weaken, the direct effect strengthens as the more advantaged seek to use other channels by which to preserve their advantages (see Goldthorpe 2016).

Boudon's ideas about primary and secondary effects have been important for later scholars, especially since Goldthorpe and others adopted them at the start of the 2000s. There have been a number of empirical analyses of their role in accounting for IEO in single countries as well as a cross-national study edited by Michelle Jackson in 2013 (Jackson 2013). Most of them have found that, for most educational transitions, Boudon's claim that secondary effects are more important than primary effects has been upheld. My sense, however, is that this approach never really took hold in the US and that, recently, interest in it in Europe has waned somewhat. One reason for this may be the realisation that it is difficult to identify primary and secondary effects given the data available to us (as pointed out by Morgan 2012; Morgan, Spiller and Todd 2013).

It is Boudon's explanation of secondary effects that has proved to be the most influential element of his analyses of IEO. He argued that students from different origins make different educational choices as a rational response to the circumstances in which they are situated, rather than being a consequence of class differences in preferences or time discounting. This rational choice approach has been adopted by sociologists, including Gambetta, Goldthorpe, Erikson, Jonsson, Esser, and many others. The model of educational decision-making that I published with John Goldthorpe (Breen and Goldthorpe 1997), for example, now has almost 4000 citations – a fact I mention not to blow my own trumpet but to show the enormous popularity of a model that is heavily inspired by Boudon's writing. Although Boudon took the idea (which Goldthorpe and I termed "relative risk aversion") from Keller and Zavalloni (1964), it seems unlikely that without Boudon's book it would have spread so widely or been so influential.

Boudon's modelling of education as a sequence of binary decisions was also not an original idea. It can be found in US sociology of the 1960s and as far back as the work of Gunnar Boalt in Sweden from the 1940s. However, contemporary applications of the approach owe much more to the work of Robert Mare (1979; 1980) than to Raymond Boudon. Mare used data to estimate the transition probabilities from a transition model rather than simulate them. It is this approach that has been taken up by the field and has become the standard way in which we analyse educational progression.

To summarise, the influence of Boudon's book on contemporary students of educational inequality has been limited both by factors related to the book itself and by external factors related to the way in which sociology has changed over the past 50 years.

There is no doubt that Boudon's interpretations of data and of his own simulation results are sometimes puzzling. As Hauser (1976, p. 913) pointed out in his review, the "relationship between evidence and conclusions is often weak, is sometimes artefactual, and in a few instances is contradictory. The analytical and observational evidence is frequently flawed by errors of fact, of method, and of logic". In particular, Boudon claims in several places that the data support his argument, when in fact they do not, or his findings are such that they would have supported a range of possible models of IEO, not just his own. There is also the question of the puzzle that motivates the book: Is it really a puzzle? That is to say, did IEO decline while ISO remained unchanged? Even when Boudon was writing, it was far from clear that there was such a puzzle, and, with hindsight, it seems apparent that there was not. Furthermore, although Boudon is concerned with change over time – how does a decline in IEO affect ISO? – his simulations are, with the exception of the material in chapters 4 and 5, entirely cross-sectional.

The improvement in the availability of data and the advent of better statistical models have contributed to making the study of IEO much more empirically sophisticated. However, at the same time, it has also become more descriptive, and studies are often justified by their policy relevance. Theory has not been entirely absent (Breen and Goldthorpe 1997; Erikson and Jonsson 1996; Raftery and Hout 1993; Lucas 2001), but the study of IEO is predominantly what Boudon later called "cameral" sociology. This trend has been exacerbated by the causal revolution. Now the questions we address are more likely to be of the form "does *X* affect *Y* and by how much" rather than "why does *X* affect *Y*" or "what explains *Y*". The design of causal research increasingly comes to try to mimic a true experiment, and in this context, there is little or no room for systemic simulations of the kind Boudon proposed. With some notable exceptions (and excluding the ad hoc theorising one typically finds at the end of an empirical paper seeking to explain its results or the equally ad hoc "hypothesising" that one finds at the start of many papers), the emphasis is on the empirics and not on explanatory theory.

## CONCLUSION

There is much to criticise in Boudon's book (and not only with the benefit of hindsight), but there is also much to admire. The major strength of the

book, and its enduring relevance, lies in its overall approach rather than in its implementation. In particular, the idea that macro-level outcomes (in Boudon's case the relation between IEO and ISO) should be explained by reference to a set of simpler processes that have a wide applicability and can be readily understood (the different degrees of constraint on the expansion of higher education and the expansion of higher status positions) is now widely accepted, especially by analytical sociologists and other proponents of mechanism-based explanations. It is ironic that, with some exceptions, this approach has proven to be much less influential among sociologists of social stratification and educational inequality.

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# INEQUALITY OF SOCIAL OPPORTUNITY: *L'INÉGALITÉ DES CHANCES* FIFTY YEARS LATER

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Are modern industrial societies meritocratic? If so, education should be the main road to achieved social status, whereas, ideally, ascribed status (social background) should have no effect on achieved status once an individual has reached a given level of educational attainment. Today, we know this is still not the case, yet research also shows that the influence of family background typically diminishes at higher educational levels (Mare 1980; Hout 1988; Breen and Jonsson 2007; Torche 2011).

These insights have developed over the last fifty years, partly influenced by Raymond Boudon, who in 1973 published *L'Inégalité des chances*, a now landmark book on the inequality of educational attainment and social status. One year later, the book was published in English under the title *Education, Opportunity, and Social Inequality. Changing Prospects in Western Society* (Boudon 1974), with a foreword from Seymour Martin Lipset, who wrote that this book "...gives us a brilliant example of the utility of abstract theory, of a formal social model, in explaining behaviour" (Lipset 1974, p. vi).

Boudon (1974, p. ii) argues he aims to address inequality of educational opportunity (IEO) and mechanisms of social mobility that are relevant to understand inequality of social opportunity (ISO). He does this by theoretically assuming that the social mechanisms related to these processes are relatively common across all Western societies, and, inspired by Weber, he then develops a model which "... deals with a kind of ideal-typical processes taking place in Western societies as a whole."

His main ambition is to explain "... *why* the tremendous educational development that occurred in all Western societies following WWII has had so little impact on equality; that is, *why* IEO has decreased so little and *why* ISO, in spite of this development, does not appear to have decreased at all" (Boudon 1974, p. xv, my italics).

He defines the two concepts: “By inequality of educational opportunity (IEO), I mean the differences in level of educational attainment according to social background. By social mobility, or immobility, I mean the differences in social achievement according to social background” (Boudon 1974, p. 1).

Thus, already at the outset, we see the parallels in his conceptualization of the two topics. In both cases, he uses the concept “opportunity,” yet his focus is on outcomes, that is, inequality in achieved education and inequality in achieved social status. Social background affects both. Linking education to social status, the Origin, Education, Destination-triangle (OED) is established. The OED model was well-established in sociology at the time (e.g., Lipset and Bendix 1959; Blau and Duncan 1967), and remains an important model in research on intergenerational mobility (e.g., Eriksen and Goldthorpe 1992; Breen et al. 2004).

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In the first part of the 1974 book, Boudon develops a theory of how individuals form their educational choices, a theory later refined by Breen and Goldthorpe (1997). In this chapter, I will summarize Boudon’s arguments in the second part of his book, where he develops his ‘box model’ of ISO (Boudon 1974, ch. 7) and then establishes what he calls a formal theory of ISO (Boudon 1974, ch. 8).<sup>1</sup>

Second, I will briefly refer to the reception of the book, in particular the well-known critique by the American sociologist Robert Hauser, and Boudon’s reply to Hauser. This discussion addresses differences between descriptive and explanatory models. Boudon is clear that the aim of sociology should be twofold: first, we need to be able to describe the reality; and second, to try to explain the patterns we find. To do so, he develops models to improve our insights into processes generating both IEO and ISO.

Before I conclude, I will also discuss the standing of his work today.

## FROM INEQUALITY OF EDUCATIONAL OPPORTUNITIES TO INEQUALITY OF SOCIAL OPPORTUNITIES

Intergenerational mobility “... is the product of a variety of factors, historical as well as social, economic, and demographic” (Boudon 1974, p. 121). Boudon’s aim is to build a formal model of ISO, give the parameters realistic values, derive conclusions from this model, and then compare them with empirical

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1 See in this book, Richard Breen’s chapter for discussions on the first part of *Education, Opportunity, and Social Inequality*, and Louis-André Vallet’s chapter for discussions on the third part, which only exists in the French version.

studies. He also noted that at the time, there were fewer empirical studies if ISO than of IEO.

Some factors “... play a determinant role with respect to social immobility”. First, inequalities in educational and social opportunities change over time. Second, the educational and the social structures change over time. Third, the degree of meritocracy matters, that is, “the role of educational attainment with respect to social status” (Boudon 1974, p. 121). Fourth, Boudon (1974, p. 121) assumes “social heritage” matters for ISO, and defines this concept as the “influence of social background on social status”. Finally, he argues, demographic factors, in particular differential fertility, also matter for ISO. He returns to these factors later, and as we will see, to simplify his model, he freezes the social structure. That is, he does not show the impact of changes in the social structure, as he commented above.

He first develops what he calls a “box model”, arguing that this model “has the advantage of being very close to sociological theory”, yet simpler than the “refined statistical models currently used in social mobility analysis” (Boudon 1974, p. 122).

#### “THE BOX MODEL”: SOCIAL BACKGROUND, EDUCATIONAL ATTAINMENT, AND SOCIAL MOBILITY

Using data from a British empirical study (Glass 1957), Boudon develops an introductory model by tabulating the son’s social status (five categories) as a function of his educational level (four categories) and the father’s social status (five categories), i.e., the OED-triangle.<sup>2</sup> This table (7.1) shows that in these British data, inequality of educational opportunities is high.

Boudon then splits this empirical information into two smaller sub-tables; one (table 7.2) where he calculates the proportion of sons who has reached each educational level as a function of father’s social status, showing that IEO is high, and another table (7.3) where he calculates the proportion of sons in each status category as a function of their own educational level, showing that achieved status increases with educational level, in line with what we would expect in a meritocratic society.

2 The socioeconomic categories used by Glass are: C<sub>1</sub> – Professional and high administrative, managerial and executive; C<sub>2</sub> – Inspectional, supervisory, and other nonmanual, higher grade; C<sub>3</sub> – Inspectional, supervisory, and other nonmanual, lower grade; C<sub>4</sub> – Skilled manual and routine grades of nonmanual; C<sub>5</sub> – Semiskilled manual, unskilled manual. The measure of educational attainment has four categories, from S<sub>1</sub> (high) to S<sub>4</sub> (low) (Glass 1957, cited in Boudon 1974, p. 123).

Boudon then produces a third sub-table (Boudon 1974, p. 128, table 7.4), showing proportions of sons in each status category as a function of the father's status. Here he generates three panels: panel a) shows a fictitious tabulation derived from the assumption of a completely meritocratic structure where ascribed status has no effect on achieved status once an individual has reached a certain level of education. These figures are derived by the marginals of the British empirical data, based on the assumption that all associations of sons' and fathers' social status go through education. Panel b) is derived from observation, that is, the British empirical data. Panel c) then, shows the differences between the two panels, that is, the theoretical mobility data generated under the meritocratic assumption, and the actual mobility data.

138 Comparing the outcomes of these three panels, Boudon (1974, p. 128) concludes that the empirical associations (table 7.4, panel b) are closer to the fictitious model based on the meritocratic assumption (table 7.4, panel a) than one might have thought. The fictitious model, based on the meritocratic assumption, captures the situation for people in positions in the middle of the status hierarchy, yet not for people in positions at the top, who are less likely to experience downward mobility than the meritocratic model predicts, and not for people in positions at the bottom, who are more likely to be immobile than the meritocratic model would predict.

The rationale for the box model is very simple. First, "... people are ordered in a lexicographic order as a function of their educational level and social background." (Boudon 1974, p. 129)<sup>3</sup> This means that Boudon assumes educational level is more important than social background for access to status positions. Second, he assumes that "the distribution of the available social positions is determined by exogenous factors" (Boudon 1974, p. 129), i.e., the structure of social positions does not change over time. As I will argue later, in line with Hauser's critique, this is a very unrealistic assumption. Third, "the available social positions are granted to candidates as a function of a) their position in the lexicographic ordering and b) a set of parameters measuring the *dominance* of each group in the ordering."<sup>4</sup>

3 S1C1 = Highest educational level and highest background comes first, S1C2 = highest educational level and second highest background, and so on until S1C5; then S2C1, ..., S2C5; then S3C1.... etc., until finally, S4C5 = low + low.

4 Boudon introduces two categories of parameters: Xij's – for instance xij1 – is a measure of the proportion of people in group SiCj who obtain positions of level C1. This is a measure of their dominance of groups located lower in the lexicographic ordering. Thus, dominance is a hierarchical concept. Yij's – for instance Yij1 – is a measure of the proportion of positions of level C1 still available to be filled by lower groups (see p. 129). As he assumes a hierarchical distribution of social status positions, he argues that "The distribution of social positions begins naturally

He constructs the dominance parameters (Boudon 1974, tables 7.5 and 7.6) so that the model shows the following: “For the same type of social background, the dominance (power to obtain the best positions) of a group is higher, the higher the educational attainment of its members” (Boudon 1974, p. 131). Applying these assumptions to the empirical data, Boudon concludes that the effect of class dominance or heritage is unevenly distributed: “... preventing *upper-class* people from being downward mobile and *lower-class* people from being upward mobile”.<sup>5</sup> This means, he concludes, that the overall picture “... is rather complex.”<sup>6</sup>

#### MERITOCRACY AND DOMINANCE IN OTHER CONTEXTS

What is the picture in other societies? Is the relative weight of meritocracy vis-à-vis social heritage different in other countries than in Britain? Boudon finds data from the USA and France yet concludes that he cannot use them for his purpose.

For France, he discusses if a table from Praderie (1966, 1967), based on data from the French Bureau of Census, can be applied. He argues, however, that the occupational categories used by the Institut National de la Statistique et des Études Économiques (INSEE) cannot be ordered into meaningful sociological categories, thus it is impossible to apply the ‘box model’, and therefore impossible to compare France with Britain (Boudon 1974, p. 133).

For the US, he refers to Blau and Duncan’s classic study on the American occupational structure from 1967. They found that education has much stronger effects than social background on people’s socioeconomic status attainment. Their study applies path-analyses, a well-known regression design. Boudon argues that, as the British data shows curve linear effects, he cannot use linear methods (such as path analysis), since these methods will probably

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enough at the highest level C1, proceeding in that order, levels C2 through C5.” Certainly, a simplified assumption, not necessarily in line with the way the labour market operates.

- 5 Interestingly, in this part of the chapter, comparing his work to Blau and Duncan’s analyses, he conceptually switches from status to class positions. In the conclusion of this chapter, however, he is back talking about status positions again.
- 6 “[...] the analyses reveals that the main effect of the dominance structure is to give people with C1 background a disproportionate ability to achieve the best social positions, even when their level of education is rather poor, whereas people with C5 background demonstrate a disproportionate weakness in reaching good social positions, even when their level of education is rather high. The effects of dominance appear less marked insofar as intermediate background groups C2 to C4 are concerned” (Boudon 1974, p. 133).

underestimate the effect of dominance. Again, he concludes he cannot apply his 'box model' to these data.

Boudon nevertheless concludes that we might *assume* that "the weight of social heritage relative to meritocracy is likely to be smaller in the United States than in either European country" (Boudon 1974, p. 136). The reason being that he believes that in societies with a more developed educational system, like the USA, meritocratic principles matter more than "social heritage" (Boudon 1974, p. 137).

#### TOWARD A FORMAL THEORY OF INEQUALITY OF SOCIAL OPPORTUNITIES

140 Boudon is particularly preoccupied with over-time change of intergenerational mobility. He reminds the reader that the IEO model has shown an "... overall increase in school attendance at each educational level; change in the composition of the student population at each educational level; change in the probability of reaching each educational level as a function of social background, and so on." (Boudon 1974, p. 141), whereas the 'box model' has shown that social status are dependent on social background and educational attainment.

He now (Boudon 1974, ch. 8) develops a theoretical analysis of the mobility model. As with the IEO model, "... we are dealing with an ideal-typical society, and not any actual society" (Boudon 1974, p. 141). He also reminds the reader that there is very little available empirical information on inequality of social opportunities.

#### RATIONALE FOR THE ISO MODEL

The ISO model builds on the following assumptions: First, that the axioms of the IEO model hold. This implies that the over-time series derived from the earlier analysis (see, in particular, pp. 86-100) are considered valid also for the ISO model. Second, that all members of a cohort are in a mutually competitive situation. Third, the ideal-typical society is characterized by both meritocracy and social dominance (social heritage). In addition, Boudon assumes, as above, a pyramidal structure of social positions, yet now he reduces the social status positions into three groups, with 10 percent in the highest category, 30 percent in the middle category, and 60 percent in the lowest. He also assumes that the structure of status positions is unchanged over time, and identical to the structure of social background (i.e., father's status positions). As noted earlier,

this assumption is not very realistic, yet for simplicity, he keeps the social structure fixed over time.

Boudon also introduces a “meritocratic parameter”. This stochastic parameter, which can take on any value between 0 and 1, is set at 0.70, i.e., a constant. Boudon gives no empirical justification as to why he chooses this number. He applies the meritocratic parameter in a symmetrical way, assuming a queuing process, which means that “... when  $x$  candidates whose education is relatively better compete for  $y$  relatively better social positions, 70 percent of these candidates will receive the desirable positions if  $x$  is smaller than  $y$ , whereas 70 percent of the positions will be given to the relatively better candidates if  $x$  is greater than  $y$ ” (Boudon 1974, p. 143). This means that:

We suppose that most of the best social positions will go to those with higher level of educational attainment, and, among those with the same level of attainment, to those with a relatively better social background. When all the available best social positions have been distributed, the second-best social positions will be distributed according to the same procedure. The process will continue until all available social positions have been distributed, the number of available social positions being assumed to be equal to the size of the cohort (Boudon 1974, p. 143).

The aim of this model is to show that: “The social status an individual is likely to achieve at any of the time periods is a function not only of his social background and his level of educational attainment, but also what we call the *social structure* (number of positions available at each level) and the *educational structure* (number of people assignable to each level of educational attainment)” (Boudon 1974, p. 142-143).

#### OVER-TIME CHANGE IN THE STATUS EXPECTATION ASSOCIATED WITH EACH EDUCATIONAL LEVEL

Boudon starts with a simple model without dominance/social heritage effects, nor differential fertility. The distribution of social structure is constant over time, whereas the educational distribution changes, as more and more people take higher education. He assumes that we have four cohorts ( $t_0 - t_3$ ), which can be used to measure change over time. Each cohort is set to include 100,000 individuals. He simplifies the educational variable used in the first



section of the book, from 9 to 6 categories.<sup>7</sup> And, as we saw above, he classifies social status positions into three categories.

This model (Boudon 1974, Table 8.4) shows the proportions reaching each social status position as a function of their educational level. Since dominance effects are not included, info on social background is not included, thus, the model is based on the distribution of educational attainment within each cohort, on a fixed social status structure (i.e., the distribution of social status positions does not change across cohorts) and a meritocratic parameter equal to 0.70. Boudon concludes that the model shows "... over-time changes (that is, across cohorts, my comment) in the structure of status expectations associated with the various educational levels" (Boudon 1974, p. 149). He argues the main endogenous factors responsible for the increase in educational demand are a "... complicated function of the combined educational and social structures. A completely accurate picture of the behaviour of this function would require a general mathematical analysis" (Boudon 1974, p. 150), and, in a more complicated version of the ISO model, he argues, this endogenous factor might be introduced.

He is also aware that "The findings derived from table 8.4 are somewhat dependent on the particular and arbitrary assumptions noted in connection with social structure" (Boudon 1974, p. 150). He does not himself undertake these calculations, but he is aware that "If it had been supposed that not 10 percent but, say, 5 percent of the available social positions at each point in time were C<sub>1</sub> (highest level), the picture would have been different" (Boudon 1974, p. 150). Certainly, this would go for both the number of status categories and their relative sizes. He nevertheless believes that the ideal-typical social structure he sketches here may be realistically assimilated to industrial societies in the present state of their development. He also argues, without documenting, that we can be satisfied that "there is good evidence that the parameters employed are realistic" (Boudon 1974, p. 150).

#### INTRODUCING FURTHER ASSUMPTIONS

Boudon then introduces *dominance effects*. "Dominance has been supposed to be higher, the higher the educational level and, within each educational level, the higher social background" (Boudon 1974, p. 155). Applying the

7 1 College education, 2 Some college, 3 High school graduation, 4 More than 3 years of high school on HC, 5 Not more than 3 years of high school on HC, 6 Elementary school (see Boudon 1974, table 8.1).



dominance effect, set at 0.70,<sup>8</sup> he finds that the dominance effect does not “... modify significantly the results obtained in the previous sections” (Boudon 1974, p. 156).

He also discusses what would happen if he assumed that the *social structure changed over time*. He does not calculate anything to see what might happen if one assumes changes over time, but provides an intuitive discussion on this topic, and argues that the main conclusion remains “... unless we suppose that social structure moves as rapidly as educational structure does, the foregoing conclusions remain valid: the expectations associated with each educational level will change according to a chain reaction pattern; the structure of social mobility will be scarcely affected; and such changes in the structure of social mobility as observed will not follow any general pattern or trend” (Boudon 1974, p. 160).

Finally, Boudon (1974, p. 160) addresses differential *fertility*, assuming that “... fertility is greater, the lower social class.” Again, he does not show any calculations but concludes that the model is flexible enough to incorporate additional assumptions. He therefore concludes that “... the outcomes drawn from the simple version of the model hold under very general conditions” (Boudon 1974, p. 160).

## A SUMMARY OF BOUDON’S ARGUMENTS

Boudon argues that his model describes an ideal-typical society. Second, he argues that his model demonstrates that “... over-time change in social mobility is small and does not follow a general trend or pattern. This conclusion derives from the inability of individuals to control the consequences that are due to the aggregation of their individual decisions” (Boudon 1974, p. 161) about how much to invest in education. This is an important social mechanism.

More specifically, industrial societies are meritocratic, and being better educated will increase people’s chances of reaching a desirable social position. Thus, we would expect increasing educational attainment over time. However, when people in the younger cohorts have higher educational attainment than people in similar situations in the older cohorts, *and* the social structure does not change, there will be more competition for access to the highest status positions, and the outcome would be an increased likelihood for downward

8 This means that “0.70 represents the power of people with background C<sub>1</sub> and educational level S<sub>1</sub> to reach positions of level C<sub>1</sub>. But it stands for also the power of those with background C<sub>1</sub> and education S<sub>1</sub> who have not received C<sub>1</sub> positions to obtain priority for C<sub>2</sub> positions. Again, all those who are not located in C<sub>1</sub> or C<sub>2</sub> will be located in C<sub>3</sub>” (Boudon 1974, p. 156).

mobility. Therefore, people in younger generations are inclined to pursue more education.

This basic mechanism explains that inasmuch as the educational structure is the result of the aggregation of individual decisions rather than being directly influenced by over-time change in the social and/or economic structure, the discrepancy between educational and socioeconomic structures persists over time (Boudon 1974, p. 161).

Boudon argues that his model shows that “other things being equal, if the stratification system is less rigid, hence if dominance effects are weaker, ISO will decrease or, alternatively, mobility will increase” (Boudon 1974, p. 161). Thus, “... lessening the rigidity of the stratification system is again the only factor that, other things being equal, could reduce ISO (Boudon 1974, p. 162).

Although this has not been shown, Boudon argues that the basic conclusion derived from the ideal-typical model is also valid in egalitarian societies. Also in these societies, high IEO and high dominance are insufficient to prevent, in particular, downward mobility.

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## RECEPTION

Boudon's book received a lot of attention. Here I will only focus on the most well-known critique, and Boudon's answer.

### HAUSER ON BOUDON'S MODEL

In a review essay, published in *American Journal of Sociology*, January 1976, Robert Hauser delivers an important critique of Boudon's model of social mobility. He first points to the fact that the book was very well received at the Eighth World Congress of Sociology in Toronto, which took place in 1974, where “... the hallways fairly buzzed with favourable anticipation” (Hauser 1976, p. 911). He therefore read the book with high expectations but concludes that “...the argument lacks cogency” (Hauser 1976, p. 912). Hauser's critique includes Boudon's method and logic, his use of analytical and observational evidence, and his interpretation of his own findings.

Hauser is skeptical about the structural constraints that Boudon installs in his model. Boudon argues that his model is based on the premise that educational attainment is determined endogenously, whereas the occupational distribution (both social background and achieved status) is identical and fixed over time. The first part reflects the fact that after WWII, more and more young people have chosen higher education. The second part, though, is unrealistic, as one might expect that several factors, such as technological change, and

increasing numbers of well-educated graduates over time would contribute to changing occupational distributions. These structural constraints imply that the conclusion "...that the structure of mobility appears to be almost constant over time" (Boudon 1974, p. 153), should not be surprising, given the way he specifies his models (Hauser 1976, p. 925).

Second, Boudon does not show that educational attainment is endogenous; rather, Hauser argues, this factor is included in one of the parameters. Thus, the so-called paradox, between increasing educational attainment and a fairly stable pattern of intergenerational mobility, is not a paradox. In any case, Hauser would suggest developing a formal demonstration of the paradox, instead of, as Boudon does, using "... only a numerical example based on fictitious data" (Hauser 1976, p. 913).

Third, Hauser has re-analyzed some of Boudon's tables, and criticizes his analyses and interpretations for being wrong, confounding main effects with interactions, and arbitrary mixing absolute and relative measures of effect sizes. For instance, addressing the 'box model', Hauser argues that "Regardless of social background, in most instances it gives persons with the lowest educational category a higher probability of entering the highest social status and a lower probability of entering the lowest social status than persons at the next higher educational level." (Hauser 1976, p. 924.) Which is not what one might expect, given the hierarchical structure Boudon suggests. Hauser also argues that Boudon's argument against linear regression in Blau and Duncan's work is not valid and refers Boudon to read their appendix for a discussion on linear associations.

We should also note that despite his critique, Hauser's review clearly shows that he found the book worthy of a decent academic discussion. Hauser also agrees with Boudon's main policy implication, namely that decreased inequality in educational attainment will not by itself lead to a reduction of social and economic inequalities in society. Thus, they agree that changes in the educational system can only indirectly contribute to reduced socio-economic inequalities.

#### BOUDON'S REPLY

Boudon's comment on Hauser's review was published in March, the same year. He argues that his aim was "... to try to answer a set of questions, not of the *how much* type, but of the *why* type" (Boudon 1976, p. 1176). He then claims that there are two sorts of models; descriptive models, that must fit the data, and explanatory models, that may not fit the data, yet still increase our

understanding of the mechanisms we want to understand (on this point, see Hedström's chapter in this book).

Boudon argues that his starting point was available descriptive information on the aggregated statistical relationship between education and status. He decided to build a model that could account for "a set of "qualitative" statements – statement of the "more-or-less" type" (Boudon 1976, p. 1177) instead of a model that fitted to a particular context. To answer the *why* questions, he wanted to build an ideal-typical, theoretical model that could describe the basic mechanisms causing this relationship. To build such a model, he argues, one needs to apply the strategy of simplicity, and here he refers to Thomas Schelling's well-known segregation model, which shows the logic of an apparent paradox, namely how segregation could arise as an unintended consequence of fairly tolerant people's behavior. He argues that his goal was similar to Schelling's; he wanted to show "... that equalization of opportunity does not mean equalization of results in an ideal-world" (Boudon 1976, p. 1179). He therefore rejects the critique that his model is "wrong" (as many of Hauser's detailed comments suggest) and argues that his main interest has been in the general phenomena, not that the model should fit a particular data source.

Boudon also gives a better explanation here for his logic of reasoning related to ISO. In a society where inequality in educational attainment has decreased, whereas the structure of social status positions has remained stable, the competition in each cohort for access to the higher positions has intensified, causing people in later cohorts to demand even more education. That is, we are dealing with a prisoner's dilemma game; as each individual does not know what the others in their cohort will do, it is rational to undertake more education to increase one's likelihood of being competitive in the labor market, although the collective outcome of this situation is less than optimal for the individuals as a group (on the link between Boudon's ISO model and game theory, see Dickmann's and Raub's chapters in this book).

Boudon concludes that descriptive models of the kind Bob Hauser applies are useful. He, nevertheless, would defend his own work, since we also need to "... go beyond the statistical relationships to explore the generative mechanisms responsible for them. This direction has a name: theory. And a goal: understanding" (Boudon 1976, p. 1187).

Forty-five years later, commenting this debate, Goldthorpe (2021, p. 182) argues that although Boudon's book can be regarded as successful, the timing of the book "... was unfortunate", as it was published "... just before the implications of the log-linear modeling of mobility tables [...] became fully recognized". This methodological breakthrough (e.g., Goodman 1970), implied that the

previous distinction between structural and exchange mobility was replaced by the distinction between absolute and relative mobility. Goldthorpe also argues that Boudon might have found “...stronger support for his arguments and enhanced his modeling” (Goldthorpe 2021) if he had paid more attention to the data inconsistencies that Hauser addressed.

## DISCUSSION

The debate between Boudon and Hauser is well-known in the circles of quantitative scholars of social stratification. Some people argue that Hauser won the battle, whereas Boudon won the war. Hauser was right, addressing Boudon’s models from a methodological point of view. However, referring to Schelling’s model of segregation, Boudon argued that his model was explanatory, and explanatory models do not have to fit a particular set of data, as they are set up to better understand a social mechanism. This argument is appealing, yet perhaps an easy way out of Hauser’s methodological critique.

Schelling’s (1971, 1978) important model is set up to reveal one specific mechanism, that is, why the outcome of a process generated by individual action can be unintended for all involved. This model is very important as it illustrates a social mechanism, yet at the outset, the model is not empirically oriented. In line with Schelling, Boudon also wants to demonstrate why the outcome of a process generated by the actions of individuals can be unintended for all involved. However, his model is based on fictitious data with specific constraints, in terms of fixed structures of social status (both social background and achieved social status), which, as Hauser argues, matters to his conclusions. Thus, the comparison with Schelling’s model is a tall call.

Boudon’s models are initially based on an increasing structural mismatch between supply and demand. Given this structural constraint, his ideal-typical model shows mobility patterns that derive from the inability of individuals to control the consequences that are due to the aggregation of their actions. This is an important mechanism in many contexts, yet in this case, this conclusion is in part a designed outcome.

Boudon argues that his approach is more theoretical than predictive (at the individual level), more analytical than statistical, and he aims to develop “... a formal theory of mobility where opportunities rather than individuals, lead the moves” (Boudon 1974, p. 139).<sup>9</sup> This is fine. Yet his structural models are

9 In a footnote to chapter 7, Boudon compares his approach to the work of Leo Goodman (1965, 1969a, 1969b), White (1970a), McFarland (1969), Coleman (1971) and Spilerman (1972) with respect to intergenerational mobility. He argues that his

deterministic, and he sets a fixed value (0.70) for the only stochastic parameter he includes (the meritocracy parameter), without any clear justification, and without any sensitivity tests, to test the impact of other values.

#### A STRUCTURALIST?

148 In several parts of the book, Boudon's arguments are close to what Jon Elster (e.g., Elster 1985) would call *free-floating intentions without subjects* (*aktørløse intensjoner*). In his critique of functionalist theory, such as, but not only, large parts of Marxist theory, Elster points to the often-occurring notion of free-floating intentions, that is, purposes assigned to structures, as if structures have agency, without any references to agents. Boudon talks about social structures interacting with each other, which is a language we would not use today. We can also note that, in several paragraphs, his agency arguments are close to the over-socialized conception of man (Wrong 1961). Yet, I would emphasize that this was written in the early 1970s, when different versions of structural-functionalist theories were highly valued on both sides of the Atlantic. And, to be fair, it is clear that Boudon's models are based on a theory of individual action. In fact, he *develops a theory of action*. In the first part of the book, he emphasizes that actors (including their families) make educational decisions based on their opportunity structure, including their expectations of succeeding in the educational system. These decisions are driven by a desire to avoid downward mobility. The same logic underlies his model of ISO. In a meritocratic society, people's education is the main resource for entry into high-status positions. Boudon assumes that the educational system is expanding, whereas the occupational system is not. Thus, his ISO model is based on a mismatch between labor market supply and demand. His arguments are in line with a prisoner's dilemma game, where increased opportunities make people invest in more educational resources, which results in increased competition in the younger cohorts for access to status positions. One might argue that Boudon here foresees the so-called "educational inflation" phenomenon, where it takes more education to fill positions than before.

Thus, Boudon cannot be characterized as a structuralist. Even here, with an explicit aim of developing a formal theory of intergenerational mobility, he is attentive to the consequences of his structurally deterministic model for the individuals, and he argues that part of the changes in opportunities are due to

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own approach may not be better, but it is different. Their approach is "... basically statistical", whereas his approach is "...algebraic and attempts to relate closely measurement to sociological theory" (Boudon 1974, p. 139, n.6).

the unintended consequences of individual action. Thus, underlying the whole argument is a theory of action; more specifically, a theory about educational investments, which can be modeled (e.g., Manzo 2022).

#### MEASUREMENT OF SOCIAL POSITIONS

Boudon (1974, p. 163) refers to the discussion on how sociologists measure social stratification. In a footnote, he writes: “In spite of the plentiful supply of literature, we have no satisfactory theory on stratification in industrial societies. Dahrendorf’s (1967) impression that present stratification theory is an “*Oedes Land*” (i.e. a “desert”) is probably common to many sociologists. Since mobility theory is largely dependent on stratification theory, a completely satisfactory theory of mobility belongs to the future.”

On the occupational distribution, Boudon assumes a pyramidal shape. Most theories of social status, socioeconomic status, or social class are based on some sort of hierarchical logic. But, he argues, the classification and ordering of the occupational categories “... is always more or less arbitrary” (Boudon 1974, p. 150). He constructs this structure so that “10 percent of the available social positions are C<sub>1</sub> (highest level), 30 percent are C<sub>2</sub>, and 60 percent are C<sub>3</sub> (lowest level)” (Boudon 1974, p. 143). He admits that the results of his models might be different with a different classification of social status, yet, as Hauser also points out, it is strange that he did not try to manipulate the values of his parameters to see if this mattered to his outcomes. Given the fact that he knew about Schelling’s segregation model, where Schelling performs sensitivity analyses, and given that Boudon did include some sort of numerical simulation, by varying parameters, in his other models, it is surprising that he did not do so for his model of social opportunities.

I would take this one step further. All occupational-based typologies can be criticized, both for their theoretical basis and for their classifications. In my earlier work, I was involved in the class-and-gender-debate in the late 1980s and early 1990s, which in part also discussed the most influential class model (Erikson and Goldthorpe) for neglecting women’s work.<sup>10</sup> This class model was revised, so that several female-dominated non-manual occupations are now usually seen as part of the working class (Eriksen and Goldthorpe 1992), and most of us who work with social stratification today would rely on the

10 This class typology was empirically developed based on information about men’s work- and market-situations, and if one includes women into the usual class categories, the heterogeneity within the classes was noticeable (Birkelund 1992; Birkelund, Goodman and Rose 1996).



revised version of this model. Yet I still think occupational classifications into social classes are somewhat arbitrary, thus I agree with Boudon who argues that this is a complicated process “... which raises questions of what *social classes* might be distinguished as underlying an arbitrary list of *sociooccupational categories*” (Boudon 1974, p. 157).<sup>11</sup> An alternative strategy could be to not to use social class schemes at all if one has better information available. And often this is the case. Boudon argues that “[i]ncome categories could be used as well as social status categories without altering the conclusion” (Boudon 1974, see footnotes, p. 162-163), and many sociologists now measure social inequality in the labor market using information about people’s income or earnings, often recoded into a rank variable, for instance, with deciles from 1 (highest), through 5 (middle), to 9 (lowest) (e.g., Bloome 2015; DiPrete 2020; Engzell and Mood 2023). This gives us a relative measure of inequality, in line with Boudon’s hierarchical conceptualization of social status.<sup>12</sup>

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## CONCLUSION

Nearly 50 years after the publication of Boudon’s book on *Education, Opportunity and Social Inequality*, we can conclude that the IEO model is still important. The model has been successfully developed to help us better understand individual-based rational action regarding educational attainment (Breen and Goldthorpe 1997).

Yet, Boudon’s ISO model is not equally important today. His model is deterministic. Although he includes a stochastic parameter, he sets this parameter exogenous. Moreover, his assumption that the social structure does not change over time is clearly unrealistic. This means that if we want to use his model of social opportunities today, we will need to make it more realistic, address sensitivity analyses more clearly, and reveal a stronger connection between individual agency and the unintended aggregated outcomes.

At the theoretical level, however, his insights are still important. It is not enough to describe reality; we also need to understand it, and an important tool then is to build a theoretical model that can help us understand the social

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11 Boudon (1974, p. 157) argues that “[i]t is beyond the scope of this book to go further into this complicated process, which ... is certainly one of the most heavily debated topics among sociologists”.

12 The growing focus on earnings mobility has likely been spurred by the increase in income inequality that has taken place in several countries, particularly in the US, as well as evidence that high levels of inequality tend to go together with low levels of mobility – the so-called Great Gatsby Curve (Krueger 2012; see also Durlauf et al. 2012).



mechanisms involved, causing the outcomes we observe. Descriptive analyses are very useful, as they also rely on analytical models, often set up as a causal map of interconnected variables (such as the OED-triangle). Yet, to understand a social phenomenon, we need a theory of individual action which situates individuals within their opportunity structure. In the case of ISO, changes in the educational system and changes in the social structures could imply that individuals find themselves in a prisoners' dilemma situation, with increased competition for social positions, where the best individual strategy does not produce the optimal outcome for them all.<sup>13</sup>

I would summarize my comments as follows:

First, Boudon addressed important societal topics, asking what kind of society we are living in, and how we can understand it.

Second, he was specific about the fact that sociology and social science need to theorize – and that the main tools are our conceptualization of social structure and individual action. This insight is still important.

Third, he saw the need for developing a formal model to better understand social mechanisms, which could help us address the *why* questions. This insight is still important.

Fourth, he was empirically oriented, and aimed to calibrate and compare his theoretical models with real-life data. Again, this is also important today.

Fifth, the debate with Robert Hauser shows two different sociological profiles at their best. Today, we can rely on them both. Following Hauser, we need to emphasize methodological skills and logic reasoning. Following Boudon, this can best be done if we are explicit about constructing analytical models based on sociological theories, including assumptions about agency and structures.

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<sup>13</sup> Alternatively, aware of the so-called educational inflation process, some people might be less interested in pursuing higher education. In the years after Boudon's book, there has been a notable decline in post-high school education among men: On average across OECD countries in 2023, 54 percent of younger women have a tertiary degree compared to 41 percent of younger men (OECD 2024, p. 19). This development is, in part, consistent with Boudon's thinking; perhaps a decline in the desirability of higher education could be a response to fewer opportunities? And increased competition from women? Certainly, this topic deserves more research, spanning time and space.

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# ON THE RELATIONSHIP BETWEEN INEQUALITY OF EDUCATIONAL OPPORTUNITY AND INEQUALITY OF SOCIAL OPPORTUNITY

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I was introduced to *L'Inégalité des chances* as a Master's student in Social Psychology during the 1978-79 academic year. I remember quite well that one of my professors at the Catholic University of Angers presented the book, and I quickly sought it out in my favorite bookshop, where I bought the second edition, dated 1978. The following year, I had the opportunity to discover large-scale empirical research on social mobility when Claude Thélot accepted me for a fifty-day research training period in the regional headquarters of the INSEE, the French National Statistical Office, in the town of Nantes. At that time, he was working on the 1953 French social mobility data – the very first mobility data that was statistically representative for France and collected within the Labour Force Survey (the *Enquête Emploi*) – and he was also working with more recent data coming from the 1970 *Formation-Qualification Professionnelle* survey, another INSEE survey he was previously responsible for. At INSEE, I discovered the extensive representative surveys conducted by the French National Statistical Office, as well as statistical modeling of contingency tables using multiplicative or log-linear models. I also began programming with the FORTRAN computing language. At the end of this period, I decided to switch from Social Psychology to Sociology to prepare a doctoral thesis on a topic related to social mobility, with Raymond Boudon as my PhD mentor. I had, *and still have*, great admiration for the Boudon of the first period, the

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*This chapter closely corresponds to the presentation the author delivered at the International Symposium "Engaging with Boudon: Insights for Contemporary Sociological Science" in Sorbonne University on 27 June 2024. The statistical analyses evoked in this chapter were performed on survey data collected by INSEE. The author would like to thank the French National Statistical Office as well as the large research infrastructure PROGEDO and ADISP (Archives de Données Issues de la Statistique Publique) for providing him with these survey data at no cost for secondary analysis.*

man who wrote *L'Analyse mathématique des faits sociaux* (*The Mathematical Analysis of Social Facts*) and who edited famous textbooks in French together with Paul Felix Lazarsfeld, *Le Vocabulaire des sciences sociales* (*The Vocabulary of Social Sciences*), *L'Analyse empirique de la causalité* (*The Empirical Analysis of Causality*), and, with also François Chazel, *L'Analyse des processus sociaux* (*The Analysis of Social Processes*) – all books that I introduced into my own library in 1979, 1980 or 1981. I was simply happy to go in that direction, thinking it might well be an appropriate way to reconcile my interest in science, especially statistical science, and my interest in society.

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But, coming back to *L'Inégalité des chances*, I must simultaneously admit that, over the decades, I have been *haunted* by a statement that Boudon made in the foreword of the 1978 second edition, and that I have spent a significant part of my academic life discussing it. On the very first page of this foreword, Boudon explained that he wrote the volume to account for an apparent paradox: “*All industrial societies have been characterized for several decades by a certainly slow, but also significant and steady decrease of inequality of educational opportunity. However, this reduction has had only modest effects on the level of social heritage.*” This is my translation of Boudon’s words. I discovered quite late, during the 1990s, in the *American Journal of Sociology*, the debate between Robert M. Hauser and Boudon, that is, the rather sharp review of the American version of the book written by the former, and the response by the latter. Evoking this fascinating exchange in a footnote within a 1996 *European Sociological Review* paper, John H. Goldthorpe (1996, p. 121) nicely wrote that “Hauser wins most of the battles but Boudon wins the war”. At a dinner I had with Leo Goodman, Mike Hout and Donald Treiman – the evening before the August 2001 Conference of the Research Committee on Social Stratification and Mobility that Mike organized in Berkeley – Leo, who unfortunately passed away in December 2020, told me that the shock between Hauser and Boudon was also a shock between two mentors as the former was sponsored by Otis Dudley Duncan while the latter was supported by Paul Lazarsfeld.

In this chapter, I will question Goldthorpe’s 1996 view that Boudon actually “wins the war”. Indeed, I will argue that *L'Inégalité des chances* is a great book, certainly for the part on Inequality of Educational Opportunity (IEO),<sup>1</sup> but not so much for the part on Inequality of Social Opportunity (ISO). Over the last twenty-five years, a collective effort undertaken by a group of social

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1 With the introduction of the model in which individuals and families take decisions about continuing with education or not by considering the risks, costs, and benefits associated, these parameters being differentially assessed according to social position. This model has had a profound influence in sociology of education over the next decades.

stratification researchers I had the great chance to belong to, has provided considerable empirical evidence that Boudon's statement in the foreword of the second edition is simply wrong. *Within modern societies, Education and change in Inequality of Educational Opportunity are key elements and ingredients to create and to understand change in Inequality of Social Opportunity.* I will demonstrate this based on my own work about France. I will also briefly reference comparative work that shows that what is observed for France can also be observed in many other societies.

I will immediately add that we should not blame Boudon too much for putting forward a questionable statement about the relationship between IEO and Inequality of Social Opportunity. *L'Inégalité des chances* was written in the early 1970s, at a time when long series of social mobility data within a country were unavailable, and when the statistical apparatus for the modeling of contingency tables was only beginning to emerge. Even the now-classical distinction between the notion of "absolute rates" and the notion of "relative rates" was not yet clearly established at that time. It is quite clear that Boudon was interested in Inequality of Educational Opportunity and Inequality of Social Opportunity – that is to say, interested in *relative rates* on both aspects. However, when we read *L'Inégalité des chances* today, we sometimes get the impression that Boudon confounds educational expansion or "massification", that is, change in absolute rates, with democratization of education per se, that is, change in relative rates. Ultimately, this is probably good news that we are today able to *falsify*, in a Popperian sense, Boudon's statement because that suggests that sociology is indeed able *to function as a science*.

After this lengthy introduction, let me begin by emphasizing that statistical models can be fundamental tools for revealing hidden trends within a society. In the year 1900, George Udny Yule discovered or invented the odds ratio, that is, a statistic that measures the association between two categories of a row variable and two categories of a column variable and which possesses the remarkable property of being independent of the margins of the contingency table. In 1935, the British statistician Maurice Bartlett defined the notion of no three-way interaction in a contingency table that cross-classifies three dichotomous variables: the odds ratio, which measures the association between two variables, is rigorously constant across the categories of the third variable. Now, let me consider a set of social mobility tables observed at different dates in the same country;  $i$  denotes class origin,  $j$  denotes class destination, and  $t$  identifies the year of the survey.

Table 1: Statistical Models Are Fundamental Tools to Discover Hidden Trends  
in Society

The multiplicative model with no three-way interaction,

i.e. the constant social fluidity model (circa 1975)

$$m_{ijt} = \alpha_{it} * \beta_{jt} * \gamma_{ij}$$

The log-multiplicative layer-effect model,

i.e. the model of uniform difference in social fluidity  
(beginning in 1992)

$$m_{ijt} = \alpha_{it} * \beta_{jt} * \gamma_{ij}^{\delta t}$$

(with  $\delta t$  fixed at 1 for the first date and estimated freely for subsequent dates).

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The first model depicted in Table 1 is simply a generalization of Bartlett's insight: the expected count in the  $(i, j, t)$  cell is the product of three parameters. The *Alpha-it* parameter guarantees that the fitted counts will exactly reproduce the distribution of class origins that is characteristic of each date. Similarly, the *Beta-jt* parameter guarantees that the fitted counts will also reproduce the distribution of class destinations observed for each date. The model, therefore, has the capability to account for historical changes observed in class origin and class destination distributions within the society. Finally, the *Gamma-ij* parameter expresses the fact that there is an association between class origin  $i$  and class destination  $j$ , that is, there is inequality of social opportunity, but this association is assumed to be rigorously constant across time. Under this model, all homologous odds ratios are rigorously constant over the survey years. This is the model of Constant Social Fluidity, or, we might say, the model of Constant Inequality of Social Opportunity.

The first paper using this model was published in *American Sociological Review* in 1975 and entitled "Temporal Change in Occupational Mobility: Evidence for Men in the United States". The author, Bob Hauser, along with his students John Koffel, Harry Travis, and Peter Dickinson, concludes that the model satisfactorily fits the observed data. All scholars, including myself, who have subsequently estimated the same model using a series of real social mobility tables across time have been impressed by the extent to which it closely approximates the observed data. So the conclusion that social fluidity – or Inequality of Social Opportunity – is certainly characterized by powerful inertia in real societies!

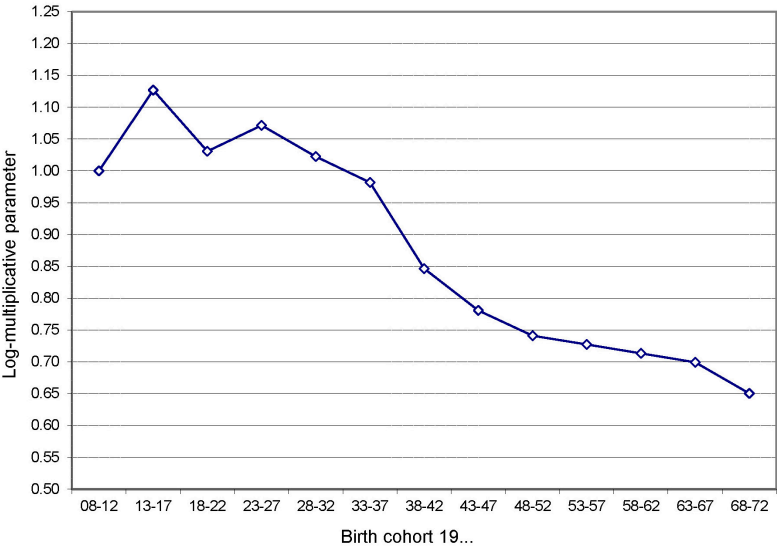
The second model depicted in Table 1 is very close to the previous one. The only difference is that the *Gamma-ij* parameter is now raised to the power *Delta-t*. Conventionally fixed at 1 for the first date, *Delta-t* is estimated freely



for all subsequent surveys. If this parameter goes below 1, that means that the association between class origin and class destination weakens over time and, as a consequence, that all estimated odds ratios are moving toward 1. When it is applied to real mobility tables across time, the second model therefore assumes a constant structure of the association between class origin and class destination while being able to possibly detect a change in what we might call ‘the general strength of this association’ – please note that the first model is just a special case of the second one with *Delta-t* equal to 1, whatever *t*. Interestingly, this very powerful model, that appeared in 1992, was proposed simultaneously from both sides of the Atlantic Ocean: on the one hand, by Yu Xie, from the University of Michigan at that time, under the name of “Log-Multiplicative Layer-Effect Model”; on the other hand, by Robert Erikson and John Goldthorpe, from the Universities of Stockholm and Oxford, under the name of “Uniform Difference Model”.

With the help of this powerful instrument, I will now demonstrate that Inequality of Educational Opportunity has declined monotonically, but slowly and unevenly, across cohorts born in France over the 20<sup>th</sup> century.

Figure 1: Trends in the Association Between Class Origin and Educational Attainment in France



Note: 8 class origins x 7 levels of education x 13 five-year birth cohorts, N=240,367.  
Data: INSEE FQP Surveys from 1964 to 1993, and INSEE Labor Force Surveys 1993 and 1997.  
Sources: Vallet (2001b, p. 200).

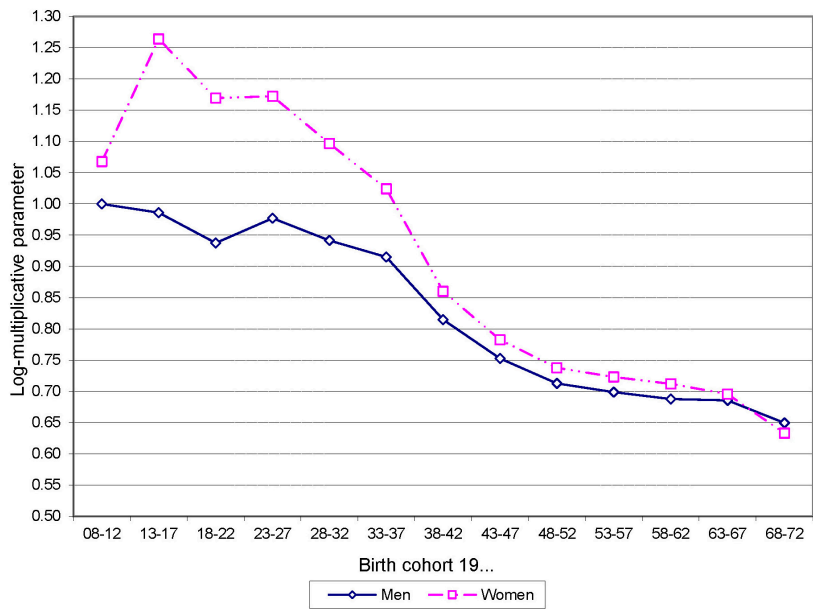
Figure 1 comes from my chapter in a book edited by Boudon, Nathalie Bulle, and Mohamed Cherkaoui in 2001. I presented it at a conference at the Sorbonne held in June 1999, exactly 25 years ago. I also presented it in Brisbane in 2002, at the 15<sup>th</sup> World Congress of the International Sociological Association. In this joint work with Claude Thélot, we compiled seven nationally representative INSEE surveys to get a huge sample of more than 240,000 French-born men and women belonging to 13 birth cohorts, from the oldest (1908-12) to the youngest (1968-72). For each birth cohort, father's class in eight categories is cross-classified with educational attainment in seven categories (from "no diploma at all" to "a degree of at least three years after the *baccalauréat*"). The graph illuminates how, net of changes in the class structure and the educational expansion, Inequality of Educational Opportunity – or the general strength of the intrinsic association between class origin and educational attainment – has evolved through the 20<sup>th</sup> century. This is done by depicting the dynamics of the estimated log-multiplicative parameters (my previous *Delta-t*).

One clearly sees that the trend has been downward, with especially remarkable progress achieved between the 1933-37 and the 1943-47 birth cohorts. The parameter declines from 1 in the first cohort to 0.65 in the last one. But don't be too impressed by this seemingly impressive 35 percent decline! The reason is that it is measured on the very abstract scale of the logarithm of the odds ratio. To be more sociological, it is necessary to use counterfactual analysis to answer the following question: how many members of the very last cohort have different diplomas than those they would have held if nothing at all had changed in France regarding the general strength of Inequality of Educational Opportunity over 60 years? And the answer is: 10 percent, only 10 percent. I also note that, when I extended this analysis with Marion Selz in 2007, considering 7 Labor Force Surveys, more than half a million individuals, 11 class origins, and 19 three-year birth cohorts, I received new estimations that this 10 percent might well be a bit overestimated.

Interestingly, the general and uneven trend observed in nationally representative data is quite consistent with the conclusions of a monographic study by the French historian of education Antoine Prost, who analyzed changes in pupils' social origins in lower and upper secondary schools in the town of Orléans between 1945 and 1980. Moreover, the pronounced progress for the cohorts born in the early 1940s can be interpreted in the context of Boudon's IEO model. In 1941, a reform promulgated by the conservative Minister of Education Jérôme Carcopino integrated the *Écoles Primaires Supérieures* into the secondary school track. As a consequence, the structure of opportunities offered to children of modest class origins has probably dramatically changed, allowing them to eventually achieve ambitious school goals without having to

make decisions that are too risky. After their elementary classes, they still had the possibility of continuing within the primary school track, with its concrete and labor-oriented aspects; however, the reform offered the most able children from lower-class backgrounds the opportunity to prepare for the *baccalauréat* after passing through the *Écoles Primaires Supérieures*.

Figure 2: Trends in the Association Between Class Origin and Educational Attainment in France  
By sex

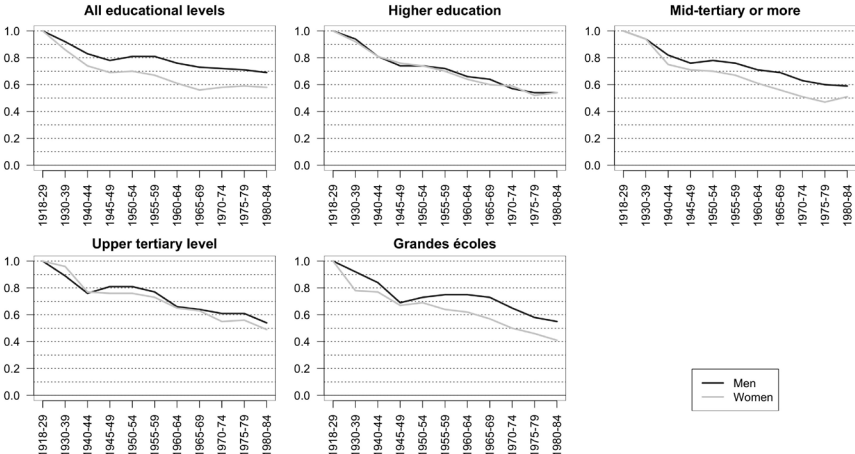


Note: 8 class origins x 7 levels of education x 13 five-year birth cohorts (by sex), N=240,367.  
 Data: INSEE FQP Surveys from 1964 to 1993, and INSEE Labor Force Surveys 1993 and 1997.  
 Sources: Vallet (2001b, p. 201).

When the investigation is replicated after distinguishing the 13 tables for men and the 13 tables for women in the same modeling, a striking conclusion emerges (Figure 2). The decline of Inequality of Educational Opportunity has indeed been more substantial for females than males, especially because, until the end of the 1930s, IEO was much more pronounced for girls than for boys. This difference progressively disappears, and it is even reversed in the 1968-72 cohort – an inversion that is also confirmed when the analysis is extended to later cohorts. This is closely related to the fact that, today in

France, school achievement and attainment are better for girls than for boys, with this difference being especially pronounced within the working class.

Figure 3: All French Labor Force Surveys Between 1982 and 2014, 11 Cohorts Born Between 1918 and 1984, and Much Detail for Degrees in Tertiary Education



Sources: Falcon and Bataille (2018, p. 342), by permission of Oxford UP.

One may wonder whether the temporal dynamics I have exhibited are sensitive to the categorization of the educational attainment variable. In a 2018 *European Sociological Review* paper, Julie Falcon and Pierre Bataille revisited the same research question with all French Labor Force Surveys between 1982 and 2014, 11 cohorts born between 1918 and 1984, and a detailed categorization for degrees in tertiary education – indeed, their lowest educational category is “less than *baccalauréat*” (Figure 3). You can easily see that the decline of the association is very general and more pronounced for women than for men; it also appears for degrees at the upper tertiary level and for degrees from the *Grandes Écoles*. Therefore, there is considerable empirical evidence that Inequality of Educational Opportunity has decreased in France, rather monotonically but also slightly.

What about trends in Inequality of *Social* Opportunity within French society? I will also argue that there is considerable empirical evidence that ISO has diminished, again slightly but quite regularly, at least from the middle of the 20<sup>th</sup> century. In 1999, I published a sixty-page paper in the *Revue Française de Sociologie*, which I also presented at the University of Wisconsin-Madison. This was my very first visit to and conference in the US, and Bob Hauser was in the room! Using again the same powerful model on social mobility tables for French men aged 35 to 59, I found that, fixed at 1 in 1953, the log-

multiplicative parameter is estimated at 0.91 in 1970, 0.87 in 1977, 0.85 in 1985, and 0.81 in 1993. Indeed, the decline appears so regular that I was able to entirely capture it with a linear trend: social fluidity has increased, or Inequality of Social Opportunity has diminished, at the rate of half a percent per year over 40 years. Again, this change of nearly 20 percent in the general strength of the association between class origin and class destination looks impressive, but you now have in mind the problem of the scale. Counterfactual analysis shows that about 4 percent of men in the 1993 mobility table have changed their class destinations, only as a result of the decline in this association over forty years. Only 4 percent. This is quite clearly something that we cannot perceive with the naked eye or in everyday life. Again, the trend was similar in father-daughter mobility tables and slightly more pronounced than in father-son tables.

Table 2: Intergenerational Social Fluidity Has Increased in France, i.e., Inequality of Social Opportunity Has Declined  
Odds ratios (same origins and destinations) for French men (and women in parentheses) aged 35-59

|                                                                                            |           | <i>Professions<br/>Intermédiaires</i><br>(lower service class) | <i>Employés</i><br>(routine non-<br>manual employees) | <i>Ouvriers</i><br>(manual workers) |
|--------------------------------------------------------------------------------------------|-----------|----------------------------------------------------------------|-------------------------------------------------------|-------------------------------------|
| <i>Cadres et Professions<br/>Intellectuelles<br/>Supérieures</i><br>(higher service class) | 1977      | 3.5 (2.7)                                                      | 10.8 (9.4)                                            | 91.7 (410.4)                        |
|                                                                                            | 1985      | 2.5 (2.3)                                                      | 7.6 (11.1)                                            | 110.8 (109.4)                       |
|                                                                                            | 1993      | 2.3 (2.2)                                                      | 4.4 (5.2)                                             | 40.9 (67.1)                         |
|                                                                                            | 2003      | 2.3 (1.8)                                                      | 5.8 (8.1)                                             | 28.8 (63.0)                         |
|                                                                                            | 2014-2015 | 2.3 (1.8)                                                      | 5.4 (6.7)                                             | 24.5 (36.2)                         |
| <i>Professions<br/>Intermédiaires</i><br>(lower service class)                             | 1977      |                                                                | 1.8 (1.8)                                             | 6.3 (9.2)                           |
|                                                                                            | 1985      |                                                                | 1.8 (1.8)                                             | 4.6 (6.4)                           |
|                                                                                            | 1993      |                                                                | 1.5 (1.5)                                             | 4.3 (7.3)                           |
|                                                                                            | 2003      |                                                                | 2.1 (1.6)                                             | 3.8 (6.6)                           |
|                                                                                            | 2014-2015 |                                                                | 1.6 (1.8)                                             | 2.7 (6.0)                           |
| <i>Employés</i><br>(routine non-manual<br>employees)                                       | 1977      |                                                                |                                                       | 3.6 (2.3)                           |
|                                                                                            | 1985      |                                                                |                                                       | 3.3 (2.6)                           |
|                                                                                            | 1993      |                                                                |                                                       | 2.4 (2.5)                           |
|                                                                                            | 2003      |                                                                |                                                       | 2.4 (1.9)                           |
|                                                                                            | 2014-2015 |                                                                |                                                       | 1.9 (2.1)                           |

Data: INSEE FQP Surveys 1977, 1985, 1993, 2003, and 2014-2015. Author's calculations.

It is possible to be less abstract by considering odds ratios computed from the observed or real mobility tables. In Table 2, for all *Formation-Qualification Professionnelle* surveys between 1977 and 2014-2015, I examine the odds ratios that involve the official four socio-occupational groups composed of salaried

people: *Cadres et Professions Intellectuelles Supérieures* (or the higher service class), *Professions Intermédiaires* (or the lower service class), *Employés* (or routine non-manual employees), *Ouvriers* (or manual workers). In computing all odds ratios, I consider the same groups for both class origin and class destination. You can perceive a general tendency for all, or nearly all, odds ratios to move toward 1 from 1977 to 2014-2015. Let me take only one very striking example. In 1977, among French women aged 35 to 59, the odds for belonging to the higher service class rather than being a manual worker were 410 times higher for daughters of a man in the higher service class than for daughters of a manual worker. The same odds ratio declines to 109 in 1985, 67 in 1993, 63 in 2003, and 36 in 2014-2015.

When male social mobility data from the same surveys conducted between 1977 and 2014-2015 are submitted to general statistical modeling, the result I obtained in 1999 exactly reappears (Table 3). The Bayesian Information Criterion shows that the model of uniform change must be preferred to the constant social fluidity model. The former model is also a significantly better fit to the data than the latter one. The estimated log-multiplicative parameter regularly declines from 1 in 1977 to 0.80 in 2014-2015. Finally, this can be captured by a diminishing linear trend of, again, half a percent per year over 38 years.

Table 3: Statistical Modeling of Change in Intergenerational Social Fluidity in France  
Between 1977 and 2014-2015  
*French men aged 35-59*

| Model                                                | G <sup>2</sup>         | df                     | test                   | DI (%)                 | rG <sup>2</sup>        | bic           |
|------------------------------------------------------|------------------------|------------------------|------------------------|------------------------|------------------------|---------------|
| <i>Men (N=41,014)</i>                                |                        |                        |                        |                        |                        |               |
| <i>On the 6 INSEE socio-occupational groups</i>      |                        |                        |                        |                        |                        |               |
| Conditional independence {TO TD}                     | 13 945.1               | 125                    | p < 0.001              | 20.5                   | -                      | 12 617.4      |
| Constant social fluidity {TO TD OD}                  | 268.3                  | 100                    | p < 0.001              | 2.6                    | 98.1                   | -793.8        |
| <b>Uniform change {TO TD <math>\phi_T</math> OD}</b> | <b>215.6</b>           | <b>96</b>              | <b>p &lt; 0.001</b>    | <b>2.2</b>             | <b>98.5</b>            | <b>-804.0</b> |
| <i><math>\phi_T</math> estimated parameters</i>      | <i>1.000</i><br>(1977) | <i>0.960</i><br>(1985) | <i>0.900</i><br>(1993) | <i>0.891</i><br>(2003) | <i>0.803</i><br>(2014) |               |
| Uniform change (constraint 1993=2003)                | 215.7                  | 97                     | p < 0.001              | 2.2                    | 98.5                   | -814.6        |
| <i><math>\phi_T</math> estimated parameters</i>      | <i>1.000</i><br>(1977) | <i>0.960</i><br>(1985) | <i>0.894</i><br>(1993) | <i>0.894</i><br>(2003) | <i>0.803</i><br>(2014) |               |
| <b>Uniform change (linear trend)</b>                 | <b>217.6</b>           | <b>99</b>              | <b>p &lt; 0.001</b>    | <b>2.2</b>             | <b>98.4</b>            | <b>-834.0</b> |
| <i>Annual trend estimated</i>                        | <i>-0.0050</i>         |                        |                        |                        |                        |               |
| Goodman-Hout model {TO TD OD $\gamma_T$ OD}          | 65.8                   | 72                     | ns                     | 1.2                    | 99.5                   | -699.0        |

Note: O for class origin (father's class), D for class destination, T for time (survey).  
Data: INSEE FQP Surveys 1977, 1985, 1993, 2003, and 2014-2015. Author's original analysis.

Results obtained on the corresponding social mobility data for women are quite similar, albeit with an interesting difference (Table 4). Over the covered period that has been characterized by an increasing involvement of women on the labor market, the increase in intergenerational social fluidity, or the decrease in Inequality of Social Opportunity, has clearly been stronger among women than among men: the last parameter attains 0.74 as against 0.80 for men, and the estimated linear trend is -0.75 percent per year compared to minus half-a-percent for men.

Table 4: Statistical Modeling of Change in Intergenerational Social Fluidity in France Between 1977 and 2014-2015  
French women aged 35-59

| Model                                                | G <sup>2</sup>                | df                            | test                          | DI (%)                        | rG <sup>2</sup>               | bic           |
|------------------------------------------------------|-------------------------------|-------------------------------|-------------------------------|-------------------------------|-------------------------------|---------------|
| <i>Women (N=34,811)</i>                              |                               |                               |                               |                               |                               |               |
| <i>On the 6 INSEE socio-occupational groups</i>      |                               |                               |                               |                               |                               |               |
| Conditional independence {TO TD}                     | 7 663.2                       | 125                           | p < 0.001                     | 16.5                          | -                             | 6 356.0       |
| Constant social fluidity {TO TD OD}                  | 216.5                         | 100                           | p < 0.001                     | 2.3                           | 97.2                          | -829.3        |
| <b>Uniform change {TO TD <math>\phi_T</math> OD}</b> | <b>140.6</b>                  | <b>96</b>                     | <b>p &lt; 0.01</b>            | <b>1.7</b>                    | <b>98.2</b>                   | <b>-863.4</b> |
| <i><math>\phi_T</math> estimated parameters</i>      | <i>1.000</i><br><i>(1977)</i> | <i>1.020</i><br><i>(1985)</i> | <i>0.880</i><br><i>(1993)</i> | <i>0.828</i><br><i>(2003)</i> | <i>0.741</i><br><i>(2014)</i> |               |
| Uniform change (constraint 1993=2003)                | 142.5                         | 97                            | p < 0.01                      | 1.7                           | 98.1                          | -871.9        |
| <i><math>\phi_T</math> estimated parameters</i>      | <i>1.000</i><br><i>(1977)</i> | <i>1.020</i><br><i>(1985)</i> | <i>0.847</i><br><i>(1993)</i> | <i>0.847</i><br><i>(2003)</i> | <i>0.742</i><br><i>(2014)</i> |               |
| <b>Uniform change (linear trend)</b>                 | <b>146.6</b>                  | <b>99</b>                     | <b>p &lt; 0.01</b>            | <b>1.8</b>                    | <b>98.1</b>                   | <b>-888.7</b> |
| <i>Annual trend estimated</i>                        | <i>-0.0075</i>                |                               |                               |                               |                               |               |
| Goodman-Hout model {TO TD OD $\gamma_T$ OD}          | 92.8                          | 72                            | p < 0.10                      | 1.4                           | 98.8                          | -660.2        |

Note: O for class origin (father's class), D for class destination, T for time (survey).  
Data: INSEE FQP Surveys 1977, 1985, 1993, 2003, and 2014-2015. Author's original analysis.

The evidence in favor of a decline in Inequality of Social Opportunity is therefore rather strong in France. We now want to appreciate the extent to which changes in Inequality of Social Opportunity have been related to changes in education and changes in Inequality of Educational Opportunity. As education typically is a cohort phenomenon – the average education attained evolves from one birth cohort to another one – it is first necessary to analyze change in social fluidity across cohorts rather than survey years.

Table 5: Change in Social Fluidity in France Across Cohorts and Age

| Men                                                        |                |                 |                  |                   |                    |                 |
|------------------------------------------------------------|----------------|-----------------|------------------|-------------------|--------------------|-----------------|
| Model                                                      | G <sup>2</sup> | df              | p                | Δ(%)              | Bic                |                 |
| <i>Men (N=64,801)</i>                                      |                |                 |                  |                   |                    |                 |
| 1. CSO CSD OD                                              | 1147.06        | 684             | .000             | 4.19              | -6431.03           |                 |
| 2. CSO CSD β <sub>C</sub> OD                               | 1090.18        | 679             | .000             | 4.04              | -6432.52           |                 |
| Difference 1-2                                             | 56.88          | 5               | .000             |                   |                    |                 |
| β <sub>C</sub>                                             | 1 (1906-24)    | 1.105<br>(.027) | 1.030<br>(.026)  | 0.958<br>(.025)   | 0.961<br>(.030)    | 0.897<br>(.036) |
|                                                            |                |                 |                  |                   |                    |                 |
| 3. CSO CSD β <sub>C</sub> β <sub>A</sub> OD                | 1033.20        | 675             | .000             | 3.93              | -6445.18           |                 |
| Difference 2-3                                             | 56.98          | 4               | .000             |                   |                    |                 |
| β <sub>C</sub> (deviation)                                 | 0 (1906-24)    | +0.072          | -0.029           | -0.108            | -0.089             | -0.191          |
| β <sub>A</sub> (deviation)                                 | 0 (middle)     | -0.019<br>(old) | -0.097<br>(old+) | +0.073<br>(young) | +0.187<br>(young+) |                 |
|                                                            |                |                 |                  |                   |                    |                 |
| 4. CSO CSD β <sub>C</sub> β <sub>A</sub> β <sub>S</sub> OD | 1030.05        | 671             | .000             | 3.92              | -6404.01           |                 |
| Difference 3-4                                             | 3.15           | 4               | ns               |                   |                    |                 |
| 5. CSO CSD β <sub>C</sub> β <sub>A</sub> OD                | 1020.85        | 665             | .000             | 3.90              | -6346.74           |                 |
| Difference 3-5                                             | 12.35          | 10              | ns               |                   |                    |                 |

Note: O for class origin (father's class), D for class destination, C for cohort, S for survey, A for age.

Data: INSEE FQP Surveys 1970, 1977, 1985, 1993, and 2003.

Sources: Vallet (2020, p. 108). (French version in Vallet [2017]).

This is what I have done for men, as shown in Table 5. From Model 2 (see the first red line), we get the impression that Inequality of Social Opportunity has only slightly diminished, from 1 in the 1906-24 birth cohort to 0.90 in the 1965-73 one. However, let me emphasize that analyzing change in social fluidity in a cohort perspective is indeed more complicated than pursuing the same sort of analysis across survey years! The reason is that, by design, the oldest cohorts are observed at an advanced age in the initial surveys, while the youngest cohorts are observed at a relatively young age in the most recent surveys. So, there is a risk of confounding generational change in social fluidity with age effect on social fluidity. Further analysis indeed confirms this expectation. In Model 3 that controls for age, change in social fluidity reveals itself as more important than previously seen: from 1 in the 1906-24 cohort to 0.81 in the 1965-73 one; and we also learn that social fluidity increases with age advancement, that is, over the course of occupational career.

In Table 6, the same analysis on women's data reveals that generational change in social fluidity has been considerable in the female part of the population: according to Model 3, from 1 in the 1906-24 cohort to 0.58 in the 1965-73 one; and, interestingly, an age effect on social fluidity again appears, but its size is more limited than among men.



Table 6: Change in Social Fluidity in France Across Cohorts and Age

| Women                                   |                    |                        |                         |                          |                           |                        |
|-----------------------------------------|--------------------|------------------------|-------------------------|--------------------------|---------------------------|------------------------|
| Model                                   | G <sup>2</sup>     | df                     | p                       | Δ(%)                     | Bic                       |                        |
| <i>Women (N = 46,079)</i>               |                    |                        |                         |                          |                           |                        |
| 1. CSO CSD OD                           | 1239.75            | 684                    | .000                    | 5.06                     | -6105.12                  |                        |
| 2. CSO CSD $\beta_C$ OD                 | 1091.44            | 679                    | .000                    | 4.61                     | <b>-6199.74</b>           |                        |
| Difference 1-2                          | 148.31             | 5                      | .000                    |                          |                           |                        |
| $\beta_C$                               | <i>1 (1906-24)</i> | <b>0.966</b><br>(.031) | <b>0.896</b><br>(.029)  | <b>0.790</b><br>(.027)   | <b>0.682</b><br>(.030)    | <b>0.666</b><br>(.035) |
| 3. CSO CSD $\beta_C \beta_A$ OD         | 1063.67            | 675                    | .000                    | 4.50                     | -6184.56                  |                        |
| Difference 2-3                          | 27.77              | 4                      | .000                    |                          |                           |                        |
| $\beta_C$ (deviation)                   | <i>0 (1906-24)</i> | <b>-0.057</b>          | <b>-0.139</b>           | <b>-0.251</b>            | <b>-0.358</b>             | <b>-0.419</b>          |
| $\beta_A$ (deviation)                   | <i>0 (middle)</i>  | <i>-0.024</i><br>(old) | <i>-0.064</i><br>(old+) | <i>+0.072</i><br>(young) | <i>+0.122</i><br>(young+) |                        |
| 4. CSO CSD $\beta_C \beta_A \beta_S$ OD | 1060.00            | 671                    | .000                    | 4.47                     | -6145.27                  |                        |
| Difference 3-4                          | 3.67               | 4                      | ns                      |                          |                           |                        |
| 5. CSO CSD $\beta_{CA}$ OD              | 1049.66            | 665                    | .000                    | 4.41                     | -6091.18                  |                        |
| Difference 3-5                          | 14.01              | 10                     | ns                      |                          |                           |                        |

Note: O for class origin (father's class), D for class destination, C for cohort, S for survey, A for age.

Data: INSEE FQP Surveys 1970, 1977, 1985, 1993, and 2003.

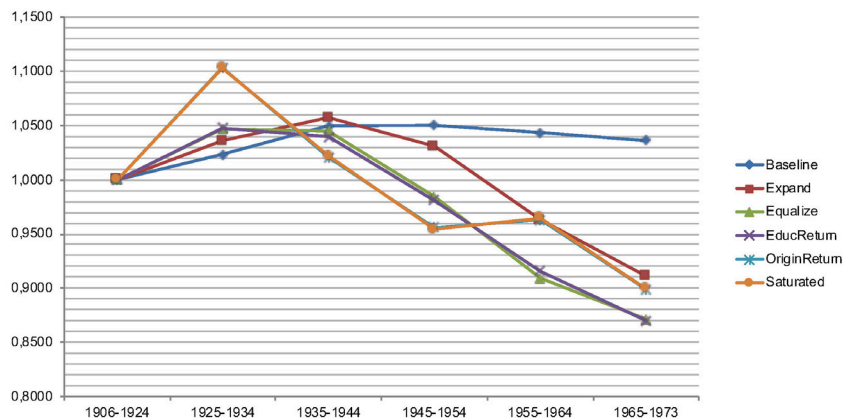
Sources: Vallet (2020, p. 108). (French version in Vallet [2017]).

We are now close to the end of the analytical process. Let me consider the triangle Class Origin – Education – Class Destination. From a theoretical perspective, and in order to explain the declining trend observed in Inequality of Social Opportunity, four basic mechanisms are potentially relevant and can be invoked:

1. the declining trend observed in Inequality of Educational Opportunity, that is, democratization of education per se
2. a change in the association between Education obtained and Class Destination, that is, a change in the (relative) occupational returns to education
3. a change in the 'direct' effect of Class Origin on Class Destination – 'direct' meaning here 'controlling for Education'
4. a subtler compositional effect caused by educational expansion; more precisely, educational expansion increases the size of the more educated groups within the population and these more educated groups are characterized by a weaker association between Class Origin and Class Destination; please note that I was able to demonstrate the latter statement for France in my contribution to the 2004 *Social Mobility in Europe* book (see Vallet 2004, pp. 138-42).

Figure 4: Contribution of Four Mechanisms to the Increase in Social Fluidity Over Cohorts

Men



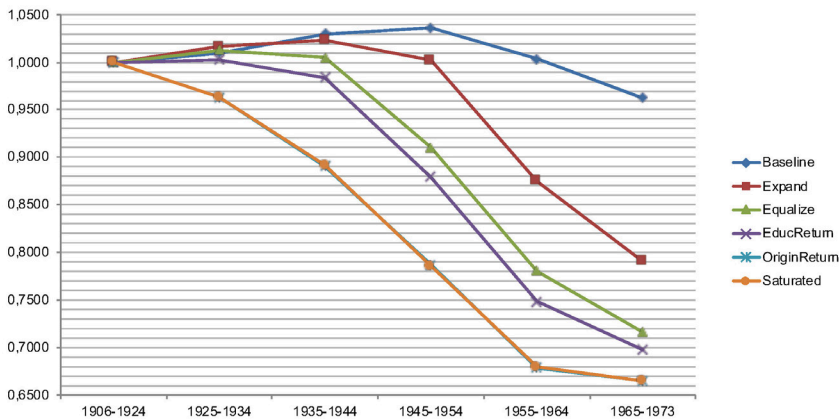
Data: INSEE FQP Surveys 1970, 1977, 1985, 1993, and 2003.

Sources: Vallet (2020, p. 116). (French version in Vallet [2017]).

How can we reveal the relative importance of these four mechanisms for explaining the observed change in Inequality of Social Opportunity in France? We can again use counterfactual analysis or simulation analysis. The general principle is as follows. We start from a very simple model (we can call it *Baseline*) that only incorporates elementary hypotheses: level of education obtained only depends on class origin; class destination depends on birth cohort, and it also depends on class origin, level of education obtained, and their interaction. We begin by simulating the consequences of these baseline hypotheses on the variation of social fluidity over cohorts (this is the blue line, *Baseline*). Then we progressively incorporate within the model the terms associated with the different explanatory mechanisms to reveal, in the same way, their specific impact on change in social fluidity or Inequality of Social Opportunity over cohorts. The terms are introduced in the following order: educational expansion or “massification” and its associated compositional effect (this is the line called *Expand*); democratization of education or reduction in Inequality of Educational Opportunity (this is the line called *Equalize*); change in the relative occupational advantage afforded by education (this is the line called *EducReturn*); change in the direct effect of class origin on class destination (this is the line called *OriginReturn*); finally, the very last terms that saturate the model and therefore exactly reproduce the *observed* variation in social fluidity (this is the line called *Saturated*). Figure 4 for men and Figure 5 for women synthesize all the results of this analysis: between the curves *Baseline*

and *Saturated*, we can perceive the relative importance of the contribution of the four explanatory mechanisms.

Figure 5: Contribution of Four Mechanisms to the Increase in Social Fluidity Over Cohorts  
Women



Data: INSEE FQP Surveys 1970, 1977, 1985, 1993, and 2003.  
Sources: Vallet (2020, p. 116). (French version in Vallet [2017]).

For both men and women, and whether we consider the 1945-54, 1955-64, or 1965-73 cohorts, it is indeed the two changes relating to education that have produced most of the decline in Inequality of Social Opportunity in France. Their relative importance, however, has changed. For men and women born between 1945 and 1954, the effect of the democratization of education is larger than the effect of its “massification”. This is, however, the opposite in the two most recent cohorts, where the latter effect (*Expand*) clearly dominates the former (*Equalize*). Comparatively, the weakening of the relative advantage afforded by education for accessing the different class positions (*EducReturn*) has affected the variation of social fluidity very little, probably because it has concerned men and women from all class origins rather uniformly.

Do the results established for France also apply to any other society? In their concluding chapter in *Social Mobility in Europe*, Richard Breen and Ruud Luijkx (2004, p. 389) wrote: “The results from our eleven countries then point to a fairly clear conclusion: there is a widespread tendency for social fluidity to increase, even though this might not be a statistically significant trend in every case.” The analyzed countries were: Germany, France, Great Britain, Hungary, Ireland, Israël, Italy, Norway, the Netherlands, Poland, and Sweden. In their concluding chapter in the 2020 book entitled *Education and Intergenerational*

*Social Mobility in Europe and the United States*, Richard Breen and Walter Müller wrote on page 287: “Considering the broad picture, taking each country over the whole period we have studied, we find no cases in which social fluidity increased without either an equalizing effect of educational expansion or equalization in the relationship between origins and education, or both.” The eight analyzed countries were: Germany, Spain, the United States, France, Italy, the Netherlands, Sweden, and Switzerland.<sup>2</sup>

Let me conclude by expressing in English two statements that I made in the conclusion of my 1999 presentation at the Sorbonne. I myself am quite surprised to say that, even 25 years later, I have not needed to make any changes to my original statements in reproducing them here.

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First, I do not have an enchanted vision of the increase in social fluidity or the decline in Inequality of Educational Opportunity. That actually means that people are living in a more ‘competitive’ society, but this is also a society less influenced by social determinism, that is to say, a society in which the “games” are a little less decided initially than they were a few decades ago. This point is, in my view, more important than the previous one.

Second, reflections that come from the epistemology of science also apply to sociology and the social sciences. When we study social change and we are particularly interested in statistical relationships that are characterized by powerful inertia – because they are located at the very heart of social organization – we are confronted with a problem of the power of our analytical instruments. In other words, we run the risk of not perceiving a change that, while real, remains tenuous and occurs slowly. It is, in reality, nothing other than the problem of the astronomer and his telescope, and, in matters of quantitative macro-sociology, it is often the statistical model we select for the analysis that plays the role of the telescope.

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PART IV

RELATIVE DEPRIVATION,  
GAME THEORY  
AND SOCIAL  
INTERDEPENDENCY





## COLEMAN'S PROBLEM AND BOUDON'S SOLUTION: RATIONAL CHOICE THEORY AS A TOOL FOR SOCIOLOGY

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Raymond Boudon and James S. Coleman have stimulated modern sociological science through theoretical and empirical work in diverse domains of the discipline. Programmatically, they envisaged sociology as a problem- and theory-guided discipline, with theory construction accounting not only for the behavior and properties of individual actors at the micro-level but also, and specifically, aiming at the explanation of phenomena and regularities at the macro-level of social systems. They likewise envisaged methodological individualism as a key feature of theory construction: macro-level phenomena and regularities are explained by also employing micro-level assumptions, namely, assumptions on individual actors. Hence, theory construction requires linking macro- and micro-levels of analysis, clarifying how system characteristics affect actors and their behavior as well as, conversely, how micro-level behavior leads to macro-level consequences. Furthermore, both Coleman and Boudon advocated for closely aligning theory construction with research designs, empirical research, and statistical modeling. In this way, they pioneered sociology as a science – “rigorous sociology” – currently employed by a family of research programs and developments in the discipline (see Raub, de Graaf and Gërkhani 2022 for a sketch of rigorous sociology; Goldthorpe 2021, ch. 9 is specifically on Boudon and Coleman as pioneers of the approach, including brief biographical sketches and a discussion of common features of their contributions as well as different emphases).

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I will argue that Boudon offers a solution to a problem that Coleman considered as crucial for sociology along these lines. Boudon's solution is broadly in line with applications of rational choice theory in sociology. An important feature of his solution is highlighting that rational choice theory in general and game theory in particular are tools for sociology, not only in the sense of providing assumptions on regularities of individual behavior, such as (expected) utility maximization or game-theoretic equilibrium behavior, but also as tools for tackling the problem that Coleman posed.<sup>1</sup>

## COLEMAN'S PROBLEM

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Concerning the macro-level, Coleman (for example, 1990, ch. 1) considers social systems such as families, cities, organizations, schools, and markets. In addition, we could consider "populations" in the sense of Goldthorpe (2016). Coleman outlines how to explain macro-level phenomena and macro-level regularities. Explanations include, first, assumptions on macro-conditions, that is, assumptions on social systems, including Goldthorpe's populations. Second, assumptions are needed on how macro-conditions affect micro-level conditions for individuals and their behavior. Such "bridge assumptions" (Wippler and Lindenberg 1987) make macro-to-micro links explicit and clarify the "logic of the situation" (Esser 1993, p. 94). Third, additional assumptions on micro-level conditions are needed, such as assumptions on actors' preferences and beliefs. Fourth, explanations require clarification of the "logic of selection" (Esser 1993, pp. 94-96), namely, assumptions on micro-level behavioral regularities, specifying how actors behave under given conditions. Fifth, there are assumptions on how macro-level outcomes depend on actors' behavior. These are "transformation rules" (Wippler and Lindenberg 1987) that make micro-to-macro links explicit, thus clarifying the "logic of aggregation" (Esser 1993, pp. 96-98). One can then derive implications concerning actors' behavior – micro-outcomes – from the assumptions on macro-conditions, bridge-assumptions, additional micro-conditions, and assumptions on behavioral regularities. Also, and particularly, implications for macro-outcomes and for macro-level regularities in the sense of statistical associations between macro-conditions and macro-outcomes follow from an explanans comprising all five kinds of assumptions. Coleman's macro-micro-

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1 Boudon's (e.g., 1998, 2003) further contributions concerning applications of rational choice theory in sociology include his attempts to develop an alternative to what he considered as standard rational choice assumptions and his attempts to "endogenize" preferences and beliefs. These contributions are less pertinent for my present purposes.

macro diagram (for example, 1990, Figures 1.2 and 1.3) is a meanwhile well-known visualization of such explanations. Coleman's sketch largely falls in line with Boudon's programmatic outline of sociological explanations in his textbook-like monograph (1981, chs. 5-6). Boudon (1981, pp. 95-98), by the way, offers a visualization that is remarkably similar to Coleman's diagram (see Raub and Voss 2017, pp. 26-27 for further discussion).

Given this approach to theory construction and explanation in sociology, Coleman (see 1987a for a concise discussion) argues that making transformation rules explicit becomes a key task for sociology. Later, he adds that making bridge assumptions explicit is a complementary key task (Coleman 1993, p. 63; see, for example, Swedberg 1990, pp. 49-50 for an interview with Coleman that includes suggestions on why he addressed macro-to-micro links only later in his programmatic work). Moreover, he argues that much of sociology fails to adequately tackle the specification of micro-to-macro and macro-to-micro links. It should be clear by now that "*Coleman's problem*" refers to including explicit bridge assumptions and transformation rules in theory construction and explanation.

By far not all, but quite a bit, of Coleman's programmatic discussion of linking macro- and micro-levels of analysis is limited to highlighting shortcomings of "classic" contributions. Coleman often focuses on shortcomings of two examples, namely, Max Weber's arguments on the relation between Protestantism and economic organization and on what Coleman calls the "frustration theory of revolution" (Coleman 1990, ch. 1 is the prime source for his treatment of these examples; closely related are 1986a, pp. 1320-1323, and 1987b, pp. 154-157). Coleman's discussion of the Weber thesis has been critically examined by Cherkaoui (2005). In the following, I will show how Boudon's solution for Coleman's problem sheds light, among other things, on the frustration theory of revolution.<sup>2</sup> We will see that Boudon's solution, in various respects, builds upon an intuition of Coleman's, namely, that interdependence is key. As Coleman (1990, p. 21) put it: "several forms of interdependence of actions show the wide variety of ways in which the micro-to-macro transition occurs. The macro-to-micro transition is in some of these cases implicitly contained in the interdependence of actions."

2 I will focus on theory construction. When it comes to empirical research, the specification of bridge assumptions and transformation rules also involves issues related to research designs, operationalizations, and the like. Such issues are beyond the scope of this contribution.

## BOUDON'S SOLUTION

Boudon often relies on examples of sociological analyses to support his programmatic approach to theory construction. In a sense, he offers “case studies” on research questions that have been addressed in classical or modern contributions to the discipline, sometimes including a “rational reconstruction” of explanatory sketches in classical or modern work (see Boudon 1981 and 1982 for case studies from various research fields). Boudon frames quite a few of his examples as stylized games. Since games and game theory are about interdependence between actors and the effects of interdependence on micro-level behavior as well as macro-outcomes of behavior, the relation to Coleman's intuition comes already in sight. I will now attempt to show that these stylized games suggest a useful and more generally applicable tool for solving Coleman's problem.

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## BOUDON'S COMPETITION MODEL

One of Boudon's games is the key element of his competition model (Boudon 1982, ch. 5; 1979b). The model allows for an analysis of an at-first-sight counterintuitive phenomenon: improved opportunities at the macro-level of a social system are sometimes associated with an increase in (indicators of) macro-level frustration. This contradicts the naïve idea of a throughout negative association at the macro-level between opportunities and frustration (see also Coleman 1990, p. 10; Coleman 1993, p. 63). Classical contributions concerning the phenomenon include Alexis de Tocqueville's (1856) suggestion that political reforms and increasing welfare were associated with increasing societal level frustration in the decades preceding the French Revolution. This suggestion is related to Coleman's discussion of the frustration theory of revolution. Émile Durkheim (1897) notes increasing suicide rates in times of economic growth. Samuel A. Stouffer et al. (1949) report lower satisfaction with the promotion system of an organization, the US Army, for branches with objectively better promotion opportunities.<sup>3</sup> Against this background, the competition model can be seen as an example of Boudon's middle-range theories (see Esser's chapter in this book).

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3 Boudon typically focuses on rational reconstruction rather than an exegetic exercise aiming at answering the question of “What did the author really mean?”, quite in line with Merton's (1968, ch. 1) distinction between the “history” and “systematics” of sociological theory, including preference for a focus on the latter.

Employing his competition model, Boudon tries to specify conditions for the emergence of the counterintuitive phenomenon. Raub (1982; 1984, ch. 4) provides a rigorous game-theoretic analysis of the model. For quite some time, the competition model did not receive much attention. Kosaka (1986) and Yamaguchi (1998) are exceptions that study variants of the model. More recently, the model has encountered a kind of renaissance. This includes implementations as an agent-based model, likewise allowing for a theoretical analysis of various extensions (Manzo 2009; 2011). The model has also been used in experimental work testing implications of the model and of variants of the model (Berger and Diekmann 2015; Berger, Diekmann and Wehrli 2024; Otten 2020, 2023).

While this has been largely overlooked in the literature on educational and social inequality, the competition model likewise yields theoretical foundations for Boudon's influential work on inequality of educational and social opportunities (Boudon 1974; 1982, ch. 4; see Raub 1984, ch. 5 for further discussion). Relatedly, Boudon (1979b) has relied on his competition model for exemplifying his notion of "generating models", namely, sociological theories that imply observable statistical regularities and can thus contribute to "reconciling sociological theories and statistical analysis" (Boudon 1979b, p. 62). This notion has become influential in, for example, Coleman's (1981, ch. 1), Cox's (1992) and Goldthorpe's (e.g., 2007, ch. 9) work on how to conceive of causation in sociology and also in analytical sociology (e.g., Hedström 2005, ch. 5).

The substantive idea underlying Boudon's competition model is taken from theories of relative deprivation (Boudon refers specifically to Runciman's 1966 version). Roughly, the assumption is that actors compare themselves with other actors – their "reference group." Actors experience relative deprivation when they are disadvantaged, compared to those in their reference group, with respect to valued outcomes. More precisely, relatively deprived actors are those who could have achieved, but did not in fact achieve an outcome themselves that members of their reference group did achieve.

Formally, the competition model is a noncooperative game with  $N \geq 2$  actors  $i$  ( $i = 1, \dots, N$ ).<sup>4</sup> The structure of the game is assumed to be common knowledge of the actors. Each actor must decide on a costly investment. For social life examples of such an investment, consider an actor's time, effort, and monetary

4 See a textbook on game theory such as Rasmusen (2007) for details on terminology, assumptions, and theorems employed in the sketch of Boudon's model. For brevity and simplicity, I sketch a simple version of the model and brush over technical details.

(opportunity) costs that are associated with following higher education, competing for promotion in one's professional career, or founding an enterprise. Each actor has two pure strategies, namely, to make the investment (INVEST) or not to make the investment (DON'T INVEST). Actors must decide independently and simultaneously in the sense that each actor, when making the decision, is not informed of the decisions of the other actors.

Payoffs are assumed to be (expected) utilities. If an actor chooses DON'T INVEST, the actor receives payoff  $o$  for sure, independent of the behavior of other actors. The actor's alternative strategy INVEST is associated with costs  $K > o$ . INVEST is also risky. Namely, the actor may then receive a prize  $B > K$  so that the final payoff is  $B - K$ , or the actor does not receive the prize and the final payoff is  $-K$ , that is, the actor loses the investment. Given our examples above and in terms of "material" outcomes, the prize could be access to an attractive job opening, promotion during a professional career, or becoming a successful entrepreneur.

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Prizes are scarce. There are  $n^*$  prizes, with  $o < n^* < N$ . Actors compete with each other for the prize due to the rule for allocating prizes. Namely, if  $n \leq n^*$  for the number  $n$  ( $n = 1, \dots, N$ ) of actors choosing INVEST, each of those actors receives the prize. If  $n > n^*$ , so that there are more actors choosing INVEST than there are prizes, each actor who has chosen INVEST obtains the prize with probability  $n^*/n$ . Given this allocation rule, the actors are interdependent in the sense that each actor's probability of obtaining the prize depends on the actor's own behavior – to INVEST oneself is necessary but in general not sufficient for obtaining the prize – and on the behavior of the others, more specifically the number  $m$  of other actors choosing INVEST. The allocation rule implies, moreover, for  $n \geq n^*$ , that the probability for an actor who has chosen INVEST to obtain the prize decreases monotonically in the number of other actors who have chosen to invest. These properties of the game motivate the label "competition model". According to Boudon, these properties also reflect, in a highly stylized way, basic features of the allocation of job openings, of the allocation of promotion opportunities in organizations, and of the success rates of new enterprises.

## MACRO-TO-MICRO AND MICRO-TO-MACRO LINKS IN THE COMPETITION MODEL

We can now show how Coleman's problem is solved for Boudon's competition model. To see this, consider the *normal form* of the game sketched so far. The normal form of a game is specified by providing three elements: the number of actors, the set of pure strategies for each actor, and the payoff

function for each actor, that is, each actor's payoff  $EU(s)$  for each strategy combination  $s = (s_1, \dots, s_i, \dots, s_N)$ , with  $s_i$  as a pure or mixed strategy of actor  $i$ . For the competition model, we have  $N$  actors and two pure strategies, INVEST and DON'T INVEST, for each actor. The matrix in Table 1 summarizes the normal form (see Boudon 1979b and 1981: 10–11 for similar visualizations).<sup>5</sup>

Table 1: Normal Form of Boudon's Competition Model ( $B > K > 0$ ;  $N \geq 2$ ).

|              | Number $m$ of other actors choosing INVEST |     |           |                |                    |     |              |     |                  |
|--------------|--------------------------------------------|-----|-----------|----------------|--------------------|-----|--------------|-----|------------------|
|              | 0                                          | ... | $n^* - 1$ | $n^*$          | $n^* + 1$          | ... | $m$          | ... | $N - 1$          |
| INVEST       | $B - K$                                    | ... | $B - K$   | $EU(n^*, n^*)$ | $EU(n^*, n^* + 1)$ | ... | $EU(n^*, m)$ | ... | $EU(n^*, N - 1)$ |
| DON'T INVEST | 0                                          | ... | 0         | 0              | 0                  | ... | 0            | ... | 0                |

The rows represent the pure strategies of a focal actor and columns represent the number  $m$  of other actors who choose INVEST. Entries in the cells are the focal actor's (expected) payoffs depending on that actor's pure strategy and the number of other actors choosing INVEST. It is straightforward to verify that  $EU(s) = 0$  for a focal actor choosing DON'T INVEST,  $EU(s) = B - K$  for a focal actor choosing INVEST, while  $m < n^*$  others likewise choose INVEST, and  $EU(s) = EU(n^*, m)$  as the focal actor's expected payoff for  $n^* \leq m$  if that actor chooses to INVEST and  $m$  others choose to INVEST, with  $EU(n^*, m) = n^*B/(m + 1) - K = n^*B/n - K$  for  $n^* \leq m \leq N - 1$ .

First, consider *bridge assumptions* in Boudon's model on how macro-conditions affect micro-level conditions for actors and their behavior. It is clear that macro-level opportunities in the competition model depend on the size  $K$  of the costs of investments, the size  $B$  of the prizes, the number  $n^*$  of prizes, and the number  $N$  of actors in the social system. Opportunities improve, *ceteris paribus*, when  $B$  or  $n^*$  increase as well as when  $K$  or  $N$  decrease. Given a game-theoretic model, the relevant micro-level conditions are the actors' (expected) payoffs. Note, then, that the normal form of the game as summarized in Table 1 specifies precisely how each actor's (expected) payoff depends on the actor's own behavior, the behavior of the other actors, and on macro-level opportunities in terms of  $B$ ,  $K$ ,  $n^*$ , and  $N$ . Hence, the normal form of the game specifies the bridge assumptions for the competition model.

Second, consider *transformation rules* on how macro-level outcomes depend on actors' micro-level behavior. For the competition model, transformation rules are needed that specify how macro-level frustration depends on micro-level

5 Concerning notation, it is important to keep in mind the distinction between  $N$  (the number of actors),  $n$  (the number of actors choosing INVEST),  $m$  (the number of other actors than the focal actor choosing INVEST), and  $n^*$  (the number of prizes).



investment decisions of each of the  $N$  actors. Motivated by relative deprivation theory, Boudon's assumption is that the reference group for actors choosing INVEST is the group of other actors who have likewise chosen INVEST, while actors choosing DON'T INVEST compare themselves with others likewise choosing DON'T INVEST. It is then in line with relative deprivation theory to assume that those actors feel relatively deprived who have chosen INVEST but do not obtain the prize  $B$ , and thus lose their investment  $K$ . Following this reasoning, Boudon defines macro-level frustration as the proportion of relatively deprived actors. The proportion of relatively deprived actors is equal to 0 if the number  $n$  of actors choosing to INVEST does not exceed the number  $n^*$  of available prizes and is otherwise equal to  $(n - n^*)/N$ . Given Boudon's specification of the macro-outcome, it then follows that the normal form allows one to derive the (expected) macro-level frustration for each strategy combination  $s$ , that is, for each micro-level outcome. Hence, the normal form of the game, together with Boudon's conceptualization of the macro-outcome, also specifies the transformation rule for the competition model.

The example of specifying bridge assumptions and transformation rules for Boudon's competition model illustrates the general point. The analysis of a noncooperative game requires that the actors' decision situation be exactly specified. The normal form of a game yields such a specification and, by doing so, implies how macro-conditions affect micro-conditions and how macro-outcomes depend on micro-outcomes. After all, macro-conditions are typically a key ingredient of the decision situation, and the normal form of a game also typically allows for deriving macro-consequences of actors' micro-level behavior. In light of Coleman's problem, this is an important contribution of game-theoretic modeling to theory formation and explanation in sociology – but one that has been hardly ever noticed.

#### THE COMPETITION MODEL AS A GENERATING MODEL FOR MACRO-LEVEL ASSOCIATIONS<sup>6</sup>

Of course, there is also another contribution of game theory to the toolbox of theory formation and explanation that is much better known and much more discussed. That contribution concerns the specification of assumptions on behavioral regularities in line with rational behavior. This is consistent with interpreting rational choice theory in general and game theory in particular as a “descriptive” – rather than “normative” – theory of individual behavior. For noncooperative games, assuming Nash equilibrium behavior or assuming

6 The following sketch uses material from Raub (2020, pp. 28–32, 40–41).



behavior in line with a “refined” equilibrium concept are standard examples. Rational choice assumptions on behavioral regularities allow for deriving micro-level outcomes, namely, implications on actors’ strategy choices and their behavior, given the normal form of the game.

For Boudon’s competition model, assumptions about behavioral regularities are needed to answer the key question in light of counterintuitive phenomena like those discussed by Tocqueville, Durkheim, and Stouffer et al.: Can improving macro-level opportunities be associated with increasing macro-level frustration? Standard assumptions on rational behavior in a noncooperative game include that actors will choose a dominant strategy if such a strategy is available and that the chosen strategies are in Nash equilibrium anyway. Moreover, it is usually assumed that rational behavior implies that actors in a symmetric game play a symmetric equilibrium, while it can be shown that a symmetric game like Boudon’s competition model indeed always has a symmetric equilibrium.

These assumptions are already sufficient for tackling our key question. DON’T INVEST is never a dominant strategy. After all, the normal form of the game shows that an actor’s payoff for INVEST is always larger than the payoff for DON’T INVEST as long as the number of other actors  $m$  who choose INVEST is small enough, that is, as long as  $m \leq n^* - 1$ . Conversely, INVEST is a dominant strategy if the (expected) payoff for INVEST exceeds the payoff for DON’T INVEST even if *all* actors choose INVEST. This is the case iff  $EU(n^*, N - 1) > 0$  for a focal actor’s expected payoff when the actor chooses INVEST. In this case, the game of course has a unique Nash equilibrium such that each actor chooses the dominant strategy INVEST. This equilibrium is also symmetric.

Assume now that  $EU(n^*, N - 1) < 0$  for a focal actor who chooses INVEST, so that INVEST is not a dominant strategy. One can then show (Raub 1984, ch. 4) that the game has a unique symmetric equilibrium in mixed strategies: in this equilibrium, each actor chooses INVEST with probability  $p^*$ ,  $0 < p^* < 1$ . Note that in this case the expected proportion of actors who choose INVEST must be smaller than 1.

By now, it is evident that improved macro-level opportunities can indeed be associated with increasing macro-level frustration. For example, consider a scenario with “good” macro-level opportunities, namely,  $N = 10$ ,  $K = 1$ ,  $B = 3$ ,  $n^* = 4$ . For this scenario, INVEST is a dominant strategy since  $EU(4, 9) = 0.2 > 0$ . Rational behavior then implies that each actor chooses INVEST. It follows that  $(N - n^*)/N = (10 - 4)/10 = 0.6$  for macro-level frustration. For a scenario with “bad” macro-level opportunities, assume  $N = 10$ ,  $K = 1$ ,  $B = 2$ ,  $n^* = 4$ . Thus, the two scenarios differ with respect to the size of the prize  $B$ .

Given the “bad” macro-level opportunities, INVEST is no longer a dominant strategy since  $EU(4, 9) = -0.2 < 0$ . Rational behavior in line with the unique symmetric equilibrium in mixed strategies implies that the expected number of actors choosing INVEST is smaller than  $N$ . Then, it likewise follows that the expected macro-level of frustration is smaller than 0.6. Hence, our example shows that better macro-level opportunities can be associated with higher macro-level frustration. As Boudon (1979b) put it: the competition model can generate associations like those discussed by Tocqueville, Durkheim, and Stouffer et al.

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It is important to realize that the competition model not only shows that better macro-level opportunities can be associated with higher macro-level frustration. Rather, the model also shows that improving macro-level opportunities can be associated with decreasing macro-level frustration. To see that, compare the scenario with “good” macro-level opportunities with further scenarios that reflect even better opportunities, namely,  $N = 10$ ,  $K = 1$ ,  $B = 3$  and  $n^* \geq 5$ . In these scenarios, more actors can obtain the prize  $B$ , while the other parameters representing macro-level opportunities are kept constant. Clearly, INVEST remains a dominant strategy and rational behavior again implies that each actor chooses INVEST in these scenarios. It follows that the number of actors who end up relatively deprived decreases and, hence, macro-level frustration decreases in these scenarios for  $n^* \geq 5$ .

Concerning the competition model as a “generating model” and with an eye on empirical content and testability, it is furthermore important that the model is not only consistent with positive as well as negative associations between macro-level opportunities and macro-level frustration. Namely, the model should also allow for specifying *conditions* for either a positive or a negative association. A comprehensive game-theoretic analysis of the model is not needed here, but is available in Raub (1984, ch. 4) and Berger and Diekmann (2015). Such an analysis specifies those regions of the parameter space where better macro-level opportunities are associated with more macro-level frustration, as well as those regions where the association is inversed.<sup>7</sup> Raub (1984, ch. 4) and Berger and Diekmann (2015) also derive implications of alternative assumptions on regularities of behavior, such as behavior in line with asymmetric equilibria in pure strategies or in line with maximin-behavior.

7 To avoid misunderstandings, note that improving macro-level opportunities due to increasing  $n^*$  can be associated with increasing macro-level frustration if INVEST is not a dominant strategy. This can happen, because the expected number  $n$  of actors choosing INVEST may increase more rapidly than  $n^*$ .

This is a useful exercise in line with theoretical pluralism, and helps to assess the robustness of model implications to variants of rationality assumptions.<sup>8</sup>

#### REMARK

To make my point about Boudon's solution to Coleman's problem, I could and did focus on a simple version of the competition model. That simple version includes various assumptions that seem "unrealistic" from an empirical perspective. Assume one would like to replace unrealistic assumptions with more realistic ones. Would that imply that Boudon's solution would become problematic? The answer to that question is "No". Consider more complex versions of the competition model. For example, such versions could allow for heterogeneity in the sense that actors have different payoff functions. Or consider a version with actors choosing sequentially such that actors choosing later know about earlier choices by other actors. Specifying Nash equilibria and deriving game-theoretic solutions in the sense of selecting a "plausible" equilibrium would then become more difficult and perhaps even impossible with analytical methods. But Boudon's solution of Coleman's problem relies on specifying the normal form of the game and does *not* depend on being able to specify Nash equilibria, let alone on specifying Nash equilibria with analytical methods. The point is precisely that game theory offers two *different* tools for sociology: equilibrium assumptions as assumptions on regularities of behavior on the one hand *and* tools like the normal form for specifying the actors' decision situation and their interdependencies in the first place. These two tools can and must be carefully distinguished. It would be no problem in principle to precisely characterize the normal form for more complex versions of the competition model. Even the assumption of equilibrium behavior itself – the *other* tool that game theory offers – could be dropped and replaced by alternative assumptions on regularities of behavior, *given* a normal form.

#### CONCLUSIONS

Boudon has sketched simple game models in quite some further work, such as in his discussion of how the First World War came about (1981, pp. 24–32), of international relations between the two world wars (1981, p. 109, 112), of

8 Together with the careful experimental work on the competition model that is meanwhile available (see the references above) this could also suggest adding some nuance to the perspective on applications of rational choice theory in the social sciences as a mere "glass-bead game" (Hedström 2021, p.498).

the general idea of unintended consequences of goal-directed and incentive guided behavior (1982, pp. 14, 15, 79-80), and of collective action (1982, pp. 144-145). From the perspective of modern game theory, his analyses may not always be technically correct in all respects. Also, as far as I know, he never explicitly made the point himself that the normal form of a game can be a useful tool that allows one to cope with and solve Coleman's problem. This point has been largely neglected in other literature, too.<sup>9</sup> At the same time, the point is clearly an implication of Boudon's work on and with game models for sociological theory formation and explanation.

Why is it that Boudon provided a solution for Coleman's problem, rather than Coleman himself? An answer to this question must remain speculative. A hunch may be that Coleman simply did not frequently employ game theory and game-theoretic reasoning. While his interest in academic social simulation games was conducive to Coleman's path to rational choice theory (see, for example, Coleman 1996, p. 348 and various contributions in Clark 1996), he focused on his sociological version of a theory of exchange systems in analogy with neoclassical economics (for example, Coleman 1990, pt. V), rather than employing game theory as a variant of rational choice theory.<sup>10</sup>

The literature provides further examples of dealing with Coleman's problem by specifying the normal form of games. An instructive case is the macro-association between group size and collective good production (see Raub 2020 for discussion and references). Also, specifying the normal form of a game is not the only way of dealing with Coleman's problem – there are various alternatives. Another tool from game theory for tackling Coleman's problem is the extensive form of a game. This is the tree-like representation that specifies features explicitly that remain "hidden" in the normal form, such as the sequence in which actors make decisions in the course of a game, and the information of an actor about what happened previously in the game when the actor makes a decision. Specifying the extensive form is needed, for example, when one wishes to analyze repeated games, including repeated games in a network of actors. For examples on how specifying the extensive form allows

9 For example, general discussions of uses of game theory in sociology such as Petersen (1994) or Swedberg (2001) and more recent overviews like Breen (2009) and Przepiorka (2021) do not address the issue at all – but see Raub, Buskens, and van Assen (2011, p. 14, n.4) for a brief remark in line with the key idea developed here.

10 Note that "game theory" is not an entry in the carefully constructed subject index of Coleman (1990). Coleman (1986b) is a rare example of work by Coleman that does employ game theory. Coleman (1987b) briefly refers to the Prisoner's Dilemma. Similar references to various game models can be found in other work by Coleman but he typically avoids explicit game-theoretic analysis.

for making bridge assumptions and transformation rules on macro-to-micro and micro-to-macro links explicit, see Buskens, Corten, and Raub (2022).

Moreover, game theory is not the only “supplier” of tools for solving Coleman’s problem. Coleman (1987a, 1990) himself has pointed out that variants of rational choice theory, such as general equilibrium theory of neoclassical economics, as well as social choice theory, include explicit examples for bridge assumptions and transformation rules. Diekmann (2022) provides guidelines for applications of rational choice theory in sociology so that they include clear assumptions on macro-to-micro and micro-to-macro links. And there are other tools than those from rational choice theory. For example, Flache and de Matos Fernandes (2021) provide guidelines for agent-based computational modeling in sociology. Their guidelines suggest how such modeling might be instrumental for solving Coleman’s problem – and how agent-based computational modeling is a tool that can accommodate rational choice assumptions on behavioral regularities but can also accommodate alternative assumptions on such regularities. What is always needed is an exact “protocol” for precisely characterizing actors’ decision situation so that macro-conditions and macro-outcomes are accounted for. The normal form as well as the extensive form of a game are examples of such protocols, but not the only examples.<sup>11</sup>

To put things in perspective, it is good to realize that in many applications, the normal form of a game has to be complemented by further assumptions in order to provide adequate bridge assumptions and transformation rules (the same point holds for the extensive form). We have already seen that in our discussion of the competition model. The normal form of the game as such yields for each strategy combination the (expected) proportion of actors who invest but do not obtain the prize. For the specification of the transformation rule, the normal form has to be complemented by a definition of “macro-level frustration” in terms of that proportion. Given relative deprivation theory, this can be seen as a straightforward step. Still, it is a necessary and important one, also highlighting that rational choice assumptions proper are by far not the only important “ingredients” of sociological theory and explanation.

To see this for a more complex example, consider revolutions, one of the cases that “motivated” the competition model. Coleman (for example, 1990, p. 10; see also 1990, ch. 18) notes that many frustrated actors do not yet necessarily

11 To avoid misunderstandings, it is useful to add that one cannot exclude *a priori* that the specification of links between macro- and micro-levels of analysis is less complex and problematic in some cases than envisaged by Coleman. For example, Goldthorpe (2021 chs. 9, 10) has provided arguments in this direction, possibly with research on social mobility and sociology of education in mind.

induce a revolution. In addition, “social organization” is needed that allows for mobilization, coordinated action, and the like (Coleman 1990, pp. 21-22). It is for this reason that Coleman (1990, p. 21) suggests that “good social history” may help to link micro- and macro-levels in such a case. In particular, Coleman (1990, pp. 482-483) observes that a revolution is a public good and thus presupposes the solution of a free-rider problem. From this perspective, in addition to specifying bridge assumptions and transformation rules that help explain in the first place why improving opportunities can induce more macro-level frustration, a “second step” of theory formation is needed. In principle, this second step could build on a game-theoretic model of public good production. This would involve specifying a normal or extensive form of a game that reveals how macro-conditions, which include, but are likely not restricted to, macro-level frustration, affect individual preferences and beliefs. Also, the normal or extensive form would reveal how the macro-outcome of collective good production, or, respectively, failure of productive good production, depends on micro-level behaviors. Jointly, these two “steps” of theory formation could be conceived as specifying “nested games” (Tsebelis 1990).<sup>12</sup>

My take-home message is that game theory, as a branch of rational choice theory, offers at least two useful tools for theory construction and explanation in sociology. One of these is well-known, though of course much disputed. That is the specification of assumptions on rational behavior for situations with interdependent actors. In Esser’s (1993) terminology: game theory – and rational choice theory more generally – provides a “logic of selection”. The second contribution of game theory is much less well known: tools for specifying a situation with interdependent actors precisely in the first place, in the process allowing for a solution of Coleman’s problem of making macro-to-micro as well as micro-to-macro links explicit. In Esser’s (1993) terminology: game theory is also a tool for clarifying the “logic of the situation” as well as the “logic of aggregation”. It should be clear that simultaneously making use of *both* contributions that game theory offers for the sociology toolbox is in line with Coleman’s arguments for emphasizing the elaboration of bridge assumptions and transformation rules in theory construction and explanation, while keeping the assumptions on behavioral regularities simple and concise. In his more abstract and fundamental work on rational choice theory, Boudon does not agree in all respects with Coleman’s arguments. In

12 Note that Coleman (1990, ch. 18) also sketches an alternative approach to frustration theories of revolution. His alternative does not focus on the relation “improved – opportunities – frustration” but on the relation “improved opportunities – perceived chances of success of a revolution”.

his sociological applications of game theory models for theory construction and explanation, though, Boudon does in fact employ Coleman's approach. Boudon thus highlights by way of example how Coleman's problem can be solved. Reexamining Coleman and Boudon indeed yields nuts and bolts for contemporary sociological science.

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THE LOGIC OF RELATIVE FRUSTRATION.  
EXPERIMENTAL TESTS OF  
RAYMOND BOUDON'S MOBILITY MODEL

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Alexis de Tocqueville's assertion, derived from the French Revolution, that societal progress can incite frustration and conflict has been a topic of enduring interest (Goldhammer and Elster 2011). A comprehensive analysis of historical data suggests that an increase in educational opportunities and a consequent oversupply on the labor market can promote social conflict (Turchin 2012; Turchin and Korotayev 2020). A related phenomenon was observed in a study on social mobility in the US Army (Stouffer et al. 1950). Promotion opportunities were evaluated as worst in those branches that offered the highest objective chances for promotion: a cross-sectional equivalent to the effect of improving conditions coinciding with growing frustration over time.

Whereas the accuracy of Tocqueville's historical narrative is not the focus of this discussion, the proposition that social advancement can foster frustration has become a cornerstone concept of broad interest within the social sciences. The recent rise in right-wing populism has been examined from this perspective

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(Goodwin 2014; Inglehart and Norris 2017; Rydgren 2012; Smith 1995). Globalization and digitalization, while driving economic growth and creating job opportunities, are also believed to have exacerbated social inequality. Since the 1970s, labor market polarization has increased, with job opportunities rising at the top of the income distribution but stagnating and even decreasing in the middle (Agénor and Aizenman 1997; Alderson and Nielsen 2002; Autor et al. 2006; Frey and Osborne 2017; Oesch 2015; Van Reenen 2011). Progress for some and stagnation, if it does not decline, for others may foster frustration among those “left behind” (Goodwin 2014; Smith 1995; Steiner et al. 2023; Swank 2003). Populist movements leverage the frustration of the left behind to gain power (Cutts et al. 2019; Ford and Goodwin 2014; Gidron and Hall 2017; Goodwin 2014; Meuleman et al. 2020; Rico et al. 2017; Rodrik 2018; Rydgren 2012; Smith 1995).

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The frustration of those left behind is elucidated by relative deprivation (Meuleman et al. 2020; Tutić and von Hermann 2018). Relative deprivation pertains to an individual’s sense of disadvantage in comparison to others, a perception often accompanied by feelings of resentment and entitlement (Smith et al. 2012). Such perceptions can erode social trust (Dunn et al. 2012; Freeman et al. 2014) and may incite antisocial behavior or a desire for retaliation against those viewed as oppressors (Gurr 2015; Marx 2020; Skarlicki and Folger 1997).

However, the established theory of relative deprivation concentrates primarily on individuals. Explaining the phenomenon of escalating frustration amidst improving conditions requires a theory that can reconcile social structure with the widespread occurrence of frustration. One such theory is Boudon’s game-theoretical model (Boudon 1977). This model links the prevalence of relative deprivation to the opportunities for upward mobility within a social system, such as a society or an organization. In essence, the model predicts, under certain assumptions, an inverted U-shaped trajectory of relative deprivation and consequent frustration over time as mobility improves.

In Section 2, we provide a brief overview of the research to date on Boudon’s model. Section 3 of this paper outlines the model and our hypotheses. Section 4 details the experiment. Section 5 presents the results, and Section 6 concludes with a discussion.

## PREVIOUS RESEARCH

Despite its potential significance to social sciences, research applying Boudon’s model remains sparse. We begin our short review with research on mathematical investigations of the model and agent-based simulation models.

Initial research has mathematically demonstrated that the primary implications of the model remain stable when the underlying micro-assumptions are varied (Kosaka 1986; Raub 1984). Boudon formulated his model in terms of game theory. However, his predictions were generally not consistent with the Nash equilibrium. Instead, he proposed the relationship between winning and losing events as a tacit coordination mechanism, which he called “quasi-solidarity” (Boudon 1982 [1977]: 114). Rationality theory would predict the mixed Nash equilibrium derived by Raub (1984).

More than two decades later, Manzo (2009) picked up the thread again with an agent-based simulation of the model. Manzo (2009) was able to show that Boudon’s prediction of a reverse U-shaped mobility-frustration function is corroborated by simulation results if certain assumptions of the parameter space are met. Further research using agent-based modelling has extended the theory, suggesting that local network social comparisons (Manzo 2011) and low entry costs into status competition (Otten 2020) both amplify the effect of improving mobility on relative deprivation.

Berger and Diekmann (2015) conducted the first experimental assessment of the model and observed either static or reduced frustration as opportunities improved. This result contradicts the prediction of the model, which assumes an inverted U-shaped curve of relative deprivation, provided that certain restrictions on the parameters that were met in the experiment are assumed. However, the findings are crucially dependent on the measurement of frustration. Inequity aversion, which may reduce competitiveness in Boudon’s model, offers a partial explanation for this inconsistency (Otten 2022). Moreover, an inverted U-shaped curve was observed in pairwise comparisons of results corresponding to the Gini coefficient as a measure of frustration (Berger and Diekmann 2015).

Previous experimental studies primarily used student samples and were conducted in small groups of six. Additionally, relative deprivation and the accompanying frustration were gauged from participant self-reports or were simply assumed to be present in the losers of competition without incorporating behavioral measures for validation (Berger and Diekmann 2015; Otten 2022).

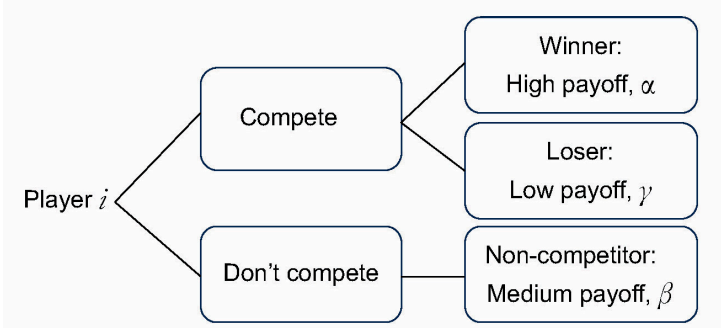
To address these limitations, we conducted an online experiment on Amazon Mechanical Turk (MTurk) to test the model’s prediction of a reverse U-shaped path of relative deprivation with improving mobility with a large, diverse sample of US citizens ( $N = 2,114$ ). Participants competed for status positions within groups that offered varying numbers of such positions, resulting in three distinct mobility levels: low, intermediate, and high. We also varied group size for robustness. Following the competition, we employed three metrics: a structural measure assessing the relative frequency of losers within

a group (termed “prevalence of relative deprivation”), a subjective measure of relative deprivation gauged using a Likert-type scale (referred to as “subjective frustration”), and a behavioral measure derived from the joy-of-destruction game (named “behavioral hostility”). In this game, participants have the opportunity to decrease the earnings of other group members, albeit at a personal expense (Abbink and Sadrieh 2009). This measure is relevant because variation in antisocial behavior within the game has been correlated with the intensity of competition for limited resources in everyday life (Prediger et al. 2014).

### THE MODEL

The model starts with a group of  $N$  players who simultaneously decide whether to compete for one of  $k$  prizes or status positions.<sup>1</sup> These positions are limited. There are more players than positions ( $N > k$ ). Entering the competition requires an investment fee ( $C$ ), akin to obtaining an academic degree as a prerequisite for applying for a well-paid position in the labor market. The competition game assigns each player a status position: high, low, or intermediate. Successful competitors, the winners, secure the desired position and receive a high payoff, calculated as the value of the prize minus the investment fee ( $B - C = \alpha$ ). These winners hold a higher social status than their group members. Those who are outcompeted, the losers, have paid their investment fee but receive nothing in return, resulting in a low payoff ( $\gamma$ ) and corresponding status position. Last, those who opt out of competition, the non-competitors, receive an intermediate payoff ( $\beta$ ) and hold an intermediate status (Figure 1).

Figure 1: Individual Decision Situation



Note: Each player has the option to compete or abstain from competition. Successful competitors, or winners, receive a high payoff ( $\alpha$ ). Unsuccessful competitors, or losers, receive a low payoff ( $\gamma$ ). Players who choose not to compete, or non-competitors, receive a medium payoff ( $\beta$ ). The likelihood of success for those entering the competition is contingent on the number of positions available and the total number of competitors. This figure is a modification of the original presented in Berger and Diekmann’s (2015) study.

Boudon's model crucially assumes that only the losers experience relative deprivation (Boudon 1982 [1977]). This is because the losers, having invested the same fee as the winners, consider the winners as their reference group. However, unlike the winners, they receive nothing in return for their investment, leading to a state of relative deprivation. An example would be university graduates who fail to secure suitable employment, an outcome that has been linked to relative deprivation and subsequent frustration (Peiró et al. 2010; Turchin 2010). In contrast, the non-competitors, who have not paid an investment fee, do not consider the winners as their reference group, and therefore do not experience relative deprivation.

The model's central implication is that, in specific conditions, an increase in relative deprivation prevalence with improving social mobility emerges as the unintended consequence of individuals' strategic decisions. Increasing mobility, represented in the model by a growing number of positions ( $k$ ), increases the expected benefit of entering competition. Consequently, additional positions tempt additional players to compete. When the number of additional competitors grows faster than the number of additional positions, the number of relatively deprived losers increases. Thus, increasing mobility boosts the relative deprivation prevalence.

The following discussion details how the effect of increasing relative deprivation with increasing social mobility derives from the model. The starting point is the following question: When should rational actors enter the competition, and when should they stay out? Intuitively, when the number of competitors matches or undershoots the number of positions, it is best to compete. Unfortunately, before the decisions are made, none of the  $N$  players knows how many of the others will enter the competition. However, given the high payoff ( $\alpha$ ), the low payoff ( $\gamma$ ), and the number of positions ( $k$ ), a rational actor can derive the expected utility of competing for a given number of competitors ( $n$ ) with equation (1). The payoff of the other strategy, not competing, is  $\beta$ , no matter how many actors enter competition. With this information, a payoff matrix can be constructed from the perspective of a focal player ( $i$ ) for a given number of positions ( $k$ ) (Figure 2).

Equation 1:

$$E(k, n) = \begin{cases} \frac{k}{n}\alpha + \frac{n-k}{n}\gamma & \text{for } k < n \\ \alpha & \text{for } k \geq n. \end{cases}$$



Figure 2: Payoff Matrix from the Perspective of Focal Player  $i$

|            |               | Number of other competitors ( $n - 1$ ) |           |           |     |               |
|------------|---------------|-----------------------------------------|-----------|-----------|-----|---------------|
|            |               | 0                                       | 1         | 2         | ... | $N-1$         |
| Player $i$ | Compete       | $E(K, 0)$                               | $E(K, 1)$ | $E(K, 2)$ | ... | $E(K, n - 1)$ |
|            | Don't compete | $\beta$                                 | $\beta$   | $\beta$   | ... | $\beta$       |

Note: The expectation of competing depends on the total number of competitors and is given by equation (1). The payoff of not competing is  $\beta$ , no matter how many group members enter the competition. This figure is a modification of the original presented in Berger and Diekmann's (2015) study.

From a game-theoretical perspective, the competition game outlined here can give rise to two distinct strategic situations. If the expectation of competing exceeds  $\beta$ , even if every actor enters competition, competing becomes the dominant strategy. A rational actor will always compete in this case, which implies that every single group member enters the competition. Consequently, the entire group ends relatively deprived, except those obtaining positions. That is, the relative deprivation prevalence simply amounts to  $1 - k/N$ .

When no dominant strategy exists, things become more complicated. This is the case when the expectation of competing exceeds  $\beta$ , up to a certain threshold of competitors,  $n^*$  and undershoots  $\beta$  thereafter. In principle, it would then be best to reach an agreement about which  $n^*$  members of a group should compete and which  $N - n^*$  should not. However, assuming homogeneous players and the absence of communication or other means of coordination, such a solution, called an asymmetric Nash equilibrium in pure strategies, cannot be realized. Another possibility is a mixed strategy solution, which, according to Harsanyi and Selten's (1988) axioms, is the rational choice in a symmetrical game. That is, each player chooses to compete with an optimal probability,  $p^*$ , and stays out of competition with probability  $1 - p^*$ . To derive  $p^*$ , the overall expected utility of competing for a given number of positions,  $k$ , and all possible permutations of competitors, is equated with the payoff of not competing,  $\beta$ . Solving for  $p$  in equation (2) yields the optimal probability,  $p^*$ .

Equation 2:

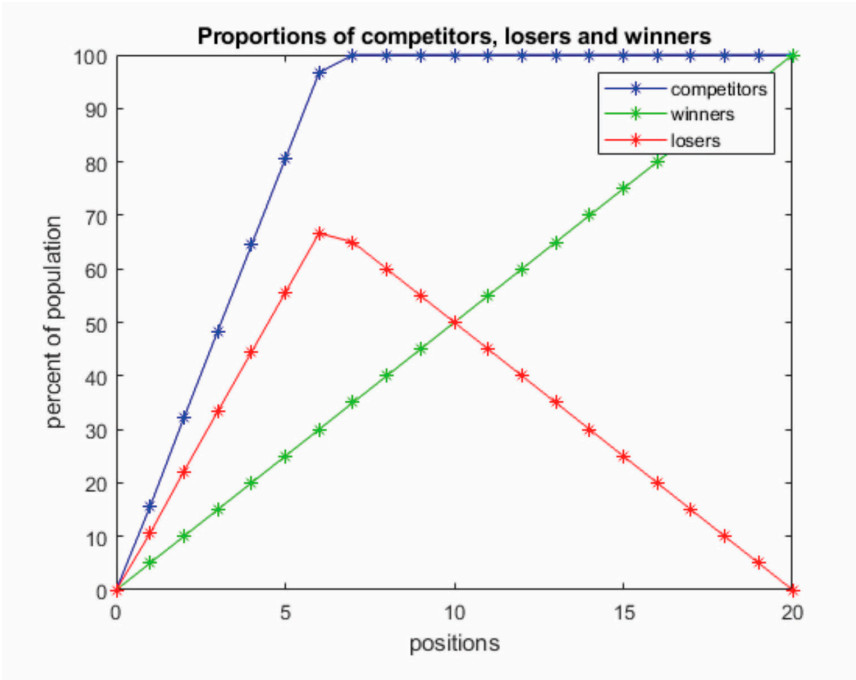
$$E(\text{Compete}) = \sum_{n=1}^N \binom{N-1}{n-1} p^{n-1} (1-p)^{N-n} E(k, n) = \beta$$

This probability also equals the expected proportion of individuals entering competition. That means that the relative deprivation prevalence amounts to  $p^* - \frac{k}{N}$ .



Figure 3 summarizes the model predictions for groups of 20 and the payoffs  $\alpha = 2, \beta = 1$ , and  $\gamma = 0.55$ . With one position available, 15 percent of the group (or three individuals) are expected to enter the competition, resulting in a relative deprivation prevalence of 10 percent (two losers). As the number of status positions grows, the number of competitors grows even faster. For this reason, relative deprivation increases with the number of positions up to a certain point. As soon as competing becomes a dominant strategy ( $k = 7$ ), and everyone enters the competition, additional positions can only diminish relative deprivation. From that point onward, relative deprivation decreases monotonically, approximating zero when virtually everyone gains a status position. From these predictions, we derive our main hypothesis: The association between the relative deprivation prevalence and mobility takes the form of an inverted U: the inverted U hypothesis.

Figure 3: Point Predictions for Groups of 20



Note: the payoffs  $\alpha = 2, \beta = 1$ , and  $\gamma = 0.55$ . Predicted share of competitors, winners, and losers per group, depending on the number of positions  $k$ .

It is worth noting that the association between mobility and relative deprivation does not necessarily take this form but depends strongly on the exact model parameters. Conditions that favor increasing deprivation with

increasing number of positions include a low entry fee into the competition and a large differential between the winners' and losers' payoffs. For a systematic analysis of those conditions, see Raub (1984). We provide the Matlab code used to derive our predictions in part 1 of the online supplementary materials (OSM).

## METHODS

### EXPERIMENTAL TREATMENTS AND DESIGN

We conducted an online experiment using MTurk, a platform previously used for social science research (Arechar et al. 2018). Online lab-style experiments offer advantages over conventional physical labs by allowing larger, more diverse samples and potentially reducing social desirability bias due to the lack of in-person experimenter presence (Belot et al. 2015; Krupnikov and Levine 2014). The study took place in the summer of 2020.

Our experiment incorporated two treatment dimensions: mobility and group size. Depending on the mobility treatment condition, the competition offered either a low, intermediate, or high number of positions (Table 1). Group size was either small groups of six participants or large groups of 20.

Table 1: Experimental Treatments Including Model Predictions (percent, set in italics)

|                       | Small groups (6 individuals) |             |             | Large groups (20 individuals) |             |             |
|-----------------------|------------------------------|-------------|-------------|-------------------------------|-------------|-------------|
|                       | Positions                    | Competitors | Losers      | Positions                     | Competitors | Losers      |
| Low mobility          | 1                            | <i>53.1</i> | <i>36.5</i> | 1                             | <i>15.6</i> | <i>10.6</i> |
| Intermediate mobility | 2                            | <i>100</i>  | <i>66.7</i> | 7                             | <i>100</i>  | <i>65.0</i> |
| High mobility         | 5                            | <i>100</i>  | <i>16.7</i> | 15                            | <i>100</i>  | <i>25.0</i> |

Notes: High payoff: USD 2, medium payoff: USD 1, low payoff: USD 0.55.

By varying mobility – the number of available status positions per group – we aim to examine if relative deprivation assumes an inverted U-shape with increasing mobility, as predicted. The second treatment dimension, group size, allows a robustness check of the results because the model predicts the same qualitative pattern independently of group size. Groups of six have been used in previous experiments (e.g., Berger and Diekmann 2015). Larger groups of twenty offer a broader scope for treatment effects to arise. The number of positions available per treatment was chosen strategically to optimize the likelihood of detecting an increase in losers when mobility improves. We began by selecting both the minimum ( $k = 1$ ) and a near-maximum number of status positions ( $k = 5$  for groups of six,  $k = 15$  for groups of 20). For the intermediate mobility treatment, we identified the number of positions at which the model

predicts the highest number of losers. This number signifies a critical juncture at which the allure of competition becomes so pronounced that it becomes the dominant strategy, prompting the entire group to enter competition. Table 1 summarizes the design, including the predicted shares of competitors and losers per treatment.

EXPERIMENTAL PROCEDURE

The experiment consisted of two parts (Table 2). The first part involved a single round of the competition game. In the second part, participants received feedback about their status after the competition, and the relative shares of winners, losers, and non-competitors in their groups. This feedback was immediately followed by the measures of relative deprivation.

Table 2: Experimental Procedure

|             |    |                                                              |
|-------------|----|--------------------------------------------------------------|
| First part  | 1. | Questionnaire on sociodemographic background                 |
|             | 2. | Competition game (prevalence of relative deprivation)        |
| Second part | 1. | Joy-of-destruction game (behavioral hostility)               |
|             | 2. | Subjective frustration (satisfaction, frustration, fairness) |

In the first part of the experiment, each participant was randomly assigned to a group of either 6 or 20 members, and each group was randomly assigned to either the low, intermediate, or high mobility treatment. Participants then completed a sociodemographic background questionnaire. They were informed about the size of their group and the number of positions available in their group. They learned that the competition would result in three types of players, each with a different payoff: winners received a high payoff of USD 2, losers a low payoff of USD 0.55, and non-competitors a medium payoff of USD 1. The payoffs were expressed in money points (MP) during the experiment, with USD 1 corresponding to 100 MP. Furthermore, participants were awarded USD 0.50 for both the first and second parts.

After reading the instructions, they underwent a comprehension check. On average, participants answered 86 percent of the questions accurately, and any incorrect responses were rectified. For a detailed overview of the test, refer to the instructions in OSM2.

Subsequently, participants chose whether to participate in the competition.

The second part of the experiment began once every member of a group had made their decisions. The competitors were informed about their status as winners or losers, and all participants learned about the number of winners, losers, and non-competitors in their group. Subsequently, we measured subjective and behavioral proxies of relative deprivation. We measured

behavioral hostility using the joy-of-destruction game (Abbink and Sadrieh 2009). In this game, each participant decided whether or not to reduce the payoff of a randomly chosen participant in their group. Participants indicated the amount of money (up to 10 MP) they would be willing to pay depending on whether the randomly selected person was a winner, loser, or non-competitor. The selected person's payoff would then be reduced by five times the amount indicated. This method produces an incentive-compatible metric for antisocial behavior, contingent on the potential target's status (winner, loser, or non-competitor). We also assessed subjective indicators of relative deprivation on a Likert-type scale from 0 to 10. These indicators included frustration with the competition, satisfaction with the competition outcome, and perceived fairness of the competition.

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In finalizing our design, we opted for a survey format over a real-time interaction format. This decision was informed by the known susceptibility of real-time online experiments to substantial dropout rates, which can reach up to 18 percent (Arechar et al. 2018). To mitigate this susceptibility, we allowed participants to read instructions, make decisions, and complete questionnaires at their own pace. The participants were then disconnected immediately after completion. Once all members of a group had completed the first part, a random mechanism selected one or more winners, depending on the specific treatment. Subsequently, all group members were invited to part two by email, typically after 20 minutes.

Despite these measures, we experienced significant dropouts. The primary reason was a longer than anticipated time lag between the two parts of the experiment. In most groups, participants received an invitation to part two approximately 20 minutes after the conclusion of part one. However, in some groups, particularly those in the high-mobility treatment with a large number of winners, the waiting time was considerably longer. This led to a pronounced dropout rate in the high-mobility treatment conditions. We address the limitations arising from this dropout in the discussion section. Table SVIII in OSM<sub>3</sub> details the dropouts for each treatment condition.

## SAMPLE

Our net sample comprised 2,114 US-American MTurk workers, 48.01 percent female and 51.99 percent male, with an average age of 39.49 years. Descriptive statistics are presented in OSM<sub>3</sub> Table SVI. Table 3 lists the number of groups and individuals (in parentheses) completed per treatment.

Table 3: Numbers of Groups per Treatment

|                            | Low mobility | Intermediate mobility | High mobility | Total       |
|----------------------------|--------------|-----------------------|---------------|-------------|
| Groups of 6 (individuals)  | 30 (180)     | 29 (174)              | 20 (120)      | 79 (474)    |
| Groups of 20 (individuals) | 30 (600)     | 30 (600)              | 22 (440)      | 82 (1640)   |
| Total                      | 60 (780)     | 59 (774)              | 42 (560)      | 161 (2,114) |

Note: Numbers refer to groups (individuals in parentheses).

### MEASURES

After the competition, we used three measures. First, we determined the percentage of losers in each group as a structural metric (called “prevalence of relative deprivation”). Next, we gauged participants’ behavioral hostility towards winners, losers, and non-competitors through the joy-of-destruction game (called “behavioral hostility”). Last, we evaluated “subjective frustration” by averaging scores from three scales: satisfaction with the competition outcome (reversed), frustration with the competition, and perceived fairness of the competition. Each scale ranged from 0 (not at all) to 10 (completely). We then constructed a subjective frustration index by averaging the scores from these three scales (Cronbach’s  $\alpha = 0.68$ ; refer to OSM III Table SIV for additional details).

### RESULTS

We observed significant pairwise correlations at the individual level between the three dependent measures (loser = 1, 0 otherwise, subjective frustration, and behavioral hostility) for groups of 6 and 20 participants. These correlations were statistically significant at least at the 5 percent level, as shown in Table 4. The relationships between loser status and feelings of frustration were moderate to strong, with point-biserial correlations just under 0.6. The correlations between loser status and behavioral hostility and between frustration and hostility were notably smaller, with values ranging between 0.1 and 0.2.

Table 4: Pairwise Correlations Between the Dependent Measures

|                        | Groups of six |                        |                      | Groups of twenty |                        |                      |
|------------------------|---------------|------------------------|----------------------|------------------|------------------------|----------------------|
|                        | Loser         | Subjective frustration | Behavioral hostility | Loser            | Subjective frustration | Behavioral hostility |
| Loser                  |               | 0.59***                | 0.21***              |                  | 0.58***                | 0.16***              |
| Subjective frustration |               |                        | 0.10*                |                  |                        | 0.20***              |

To examine the inverted-U hypothesis, we analyzed the entry into competition and the resulting prevalence of relative deprivation, represented by the share of losers per group. We employed logit models with Competing or Loser as dependent variables and three treatment dummies as predictors: intermediate mobility and high mobility, with low mobility as the reference category. We also constructed extended models with additional predictors: female (with male as reference category), age, and risk preference on a scale from 0 for risk averse, to 10 for risk seeking (Dohmen et al. 2011). All models were computed with robust standard errors and were clustered at the group level. We report only the average marginal effects (AMEs) from the restricted model here because the results from the extended model, reported in the Supplementary Online Materials (OSM3-Table SI), were closely comparable.<sup>1</sup>

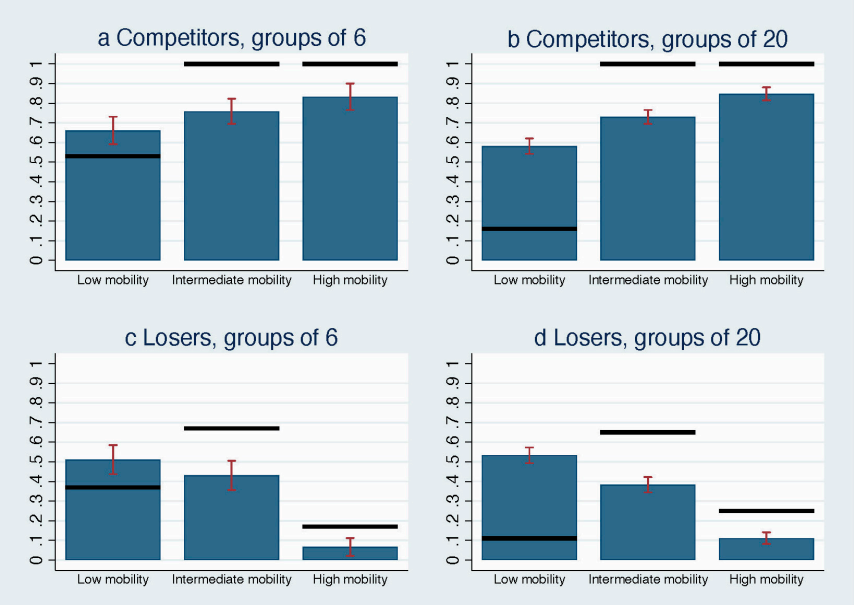
204 Figure 4 summarizes the results. The general pattern that higher numbers of winning places led to higher numbers of participants entering the competition holds for both small groups (panel a of Figure 4) and large groups (panel b of Figure 4). Using the low-mobility treatment condition as a reference, we find that for small groups with intermediate mobility,  $AME = 0.098, z = 1.96, p < 0.05$ ; and with high mobility,  $AME = .172, z = 3.50, p < 0.001$ . For large groups with intermediate mobility,  $AME = .148, z = 5.32, p < 0.001$ ; and with high mobility,  $AME = 0.266, z = 9.13, p < 0.001$ . The entry rates observed are indicated in the note to Figure 4.

Interestingly, we notice significant over-entry in conditions of low mobility and under-entry in conditions of intermediate or high mobility compared to rationality predictions. Under low mobility, 66.1 percent and 58.2 percent of participants enter competition, thus exceeding the predicted rates of 53.1 percent and 15.6 percent, respectively. Conversely, under intermediate mobility, the competition entry rates are 75.9 percent and 73.0 percent, falling short of the predicted 100 percent for each group.<sup>2</sup> However, note that over-entry is excluded by definition from the dominant strategy case.

1 This section draws on Berger and Diekmann (2015). For the game-theoretical model and derivations of the mixed equilibrium strategy, see Raub (1984).

2 We also computed a full model that includes the two treatment effects (number of positions and group size) with their interaction effects (refer to OSM Table SII of Berger, Diekmann, Wehrli 2024). However, in the main manuscript, we present individual regression models as opposed to the full model. This approach was chosen for two reasons. Firstly, the interaction effects were found to be statistically insignificant. Secondly, the theoretical model's predictions for large and small groups are distinct. Conducting separate analyses simplifies the comparison of predicted results with observed outcomes.

Figure 4: Shares of Competitors and Losers as a Function of Upward Social Mobility



Note: Red lines represent 95 percent confidence intervals, and black horizontal lines indicate predictions. **a** Shares of competitors per group in groups of six. Low mobility, 0.66 (predicted, 0.53); intermediate mobility, 0.76 (predicted, 1.0); high mobility, 0.83 (predicted, 1.0). **b** Shares of competitors per group in groups of 20. Low mobility, 0.58 (predicted, 0.16); intermediate mobility, 0.73 (predicted, 1.0); high mobility, 0.84 (predicted, 1.0). **c** Shares of losers per group in groups of six. Low mobility, 0.51 (predicted, 0.37); intermediate mobility, 0.43 (predicted, 0.67); high mobility, 0.07 (predicted, 0.17). **d** Shares of losers per group in groups of 20. Low mobility, 0.53 (predicted, 0.11); intermediate mobility, 0.38 (predicted, 0.65); high mobility, 0.11 (predicted, 0.25). The sample sizes are  $n = 474$  for groups of six and  $n = 1640$  for groups of 20.

Due to the discrepancy between predicted and actual competitiveness, the shares of losers in the groups do not peak as expected in the intermediate-mobility treatment. Instead, the shares of losers in both small and large groups consistently decrease as mobility increases (Figure 4b and c). For small groups with intermediate mobility,  $AME = -0.15$ ,  $z = -5.30$ ,  $p < 0.001$ ; and with high mobility,  $AME = -0.444$ ,  $z = -11.64$ ,  $p < 0.001$ . For large groups with intermediate mobility,  $AME = -0.167$ ,  $z = -6.33$ ,  $p < 0.001$ ; and with high mobility,  $AME = -0.422$ ,  $z = -15.36$ ,  $p < 0.001$  (SOM3-Table III).

In summary, we observed an over-entry in competition within the low-mobility treatment, juxtaposed with an under-entry in the intermediate-mobility treatment. Over-entry is particularly pronounced in the larger group with 20 actors and is less apparent in the smaller 6-person group. Consequently, the shares of losers exceed predictions in the low-mobility treatments and

fall short in the intermediate-mobility treatments. Therefore, contrary to the model’s prediction of an inverted U-shaped trend in relative deprivation prevalence, we found that the percentage of losers consistently decreases with increasing mobility.

SUBJECTIVE FRUSTRATION

We constructed ordinary least squares (OLS) regression models with subjective frustration as the dependent variable to achieve two primary objectives. First, we sought to examine a fundamental micro assumption that individuals who lose in a competition experience a higher degree of frustration than those who do not compete or those who win. Second, we sought to scrutinize the inverted-U hypothesis through a subjective proxy of relative deprivation.

206 The data strongly support the micro assumption. On a scale from 0 to 10, losers report roughly 2.7 points higher average frustration than non-competitors and roughly 3 points higher frustration than winners. These effects are statistically significant with  $p < 0.001$  (Table 5, Models 1 and 3).

Table 5: Subjective Frustration

|                       | Model 1             | Model 2              | Model 3             | Model 4              |
|-----------------------|---------------------|----------------------|---------------------|----------------------|
| Loser                 | 2.709***<br>(11.16) |                      | 2.693***<br>(20.70) |                      |
| Winner                | -0.322<br>(-1.37)   |                      | -0.305**<br>(-2.65) |                      |
| Intermediate mobility |                     | 0.161<br>(0.65)      |                     | -0.257<br>(-1.76)    |
| High mobility         |                     | -1.206***<br>(-4.73) |                     | -1.269***<br>(-9.46) |
| Constant              | 2.382***<br>(13.12) | 3.509***<br>(19.10)  | 2.453***<br>(26.80) | 3.767***<br>(31.92)  |
| N                     | 474                 | 474                  | 1640                | 1640                 |

Note: OLS regression models with subjective frustration (index) as dependent variable. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . t-values in parentheses. Standard errors clustered at the group level. Models 1 and 2 refer to groups of 6; Models 3 and 4 refer to groups of 20. Reference categories: non-competitor, low-mobility condition.

Contrarily, the inverted-U hypothesis does not receive any support from our findings. We observed no significant differences in frustration levels between the low- and intermediate-mobility treatments in either small or large groups. However, frustration levels were approximately 1.2 to 1.3 points lower in high-



mobility treatments than in low-mobility treatments ( $p < 0.001$  for groups of both sizes). In essence, frustration remains stable as mobility increases but decreases when mobility reaches its peak (refer to Table 5, Models 2 and 4).

In conclusion, our results do not endorse the inverted-U hypothesis. However, they do affirm the micro assumption that losers experience greater frustration than winners and non-competitors.

BEHAVIORAL HOSTILITY

In our analysis of behavioral hostility within the joy-of-destruction game, we observed a distinct pattern. Approximately 40.7 percent of participants were willing to pay to reduce the payoff of a randomly selected group member. This figure aligns closely with the 39.4 percent reported in a previous study by Abbink and Sadrieh (2009).

When we break down this behavior by participant type, non-competitors fall below this baseline at 22.2 percent whereas losers exceed it at 51.8 percent. Interestingly, winners align closely with the overall average at 40.1 percent. Losers spend three times as much ( $M = 2.215$ ) as non-competitors ( $M = 0.753$ ), and winners spend approximately twice as much ( $M = 1.74$ ).

However, the status of the individual on the receiving end of the hostility, the “target,” appears to have minimal impact. For instance, losers invested 2.13, 2.04, and 2.48 to reduce the payoffs of non-competitors, losers, and winners, respectively.

Consequently, our discussion will primarily focus on general interpersonal hostility, defined as the average individual spending for reduction (Cronbach’s  $\alpha = .90$ ). This approach allows us to concentrate on the behavior of the instigator, which our data suggest plays a more significant role than the status of the target (refer to Table 6).

Table 6: Hostility Depending on the Status of a Focal Individual and a Target Individual

| Target’s status | Instigator’s status |        |                |
|-----------------|---------------------|--------|----------------|
|                 | Loser               | Winner | Non-competitor |
| Loser           | 2.04                | 1.65   | 0.72           |
| Winner          | 2.48                | 1.89   | 0.91           |
| Non-competitor  | 2.13                | 1.69   | 0.63           |

Note: The table represents the points invested by the focal participant to reduce the payoff of the target, contingent on the instigator’s and the target’s status. The conversion rate is 100 money points, equivalent to USD 1.

We analyzed behavioral hostility using OLS regression models (Table 7). Initially, we only tested for status effects in Models 1 and 4 for small and large groups, respectively. Next, we tested for treatment effects in Models 2 and 5.

Finally, we incorporated the participants' beliefs about the hostile behavior of other group members, which has been identified as a strong predictor of reducing others' payoffs in previous research (Prediger et al. 2014).

Table 7: Behavioral Hostility

|                               | Model 1  | Model 2  | Model 3   | Model 4  | Model 5  | Model 6   |
|-------------------------------|----------|----------|-----------|----------|----------|-----------|
| Loser                         | 1.761*** |          | 0.579**   | 1.384*** |          | 0.621***  |
|                               | (6.56)   |          | (3.06)    | (8.82)   |          | (6.19)    |
| Winner                        | 1.093*** |          | 0.0932    | 0.970*** |          | 0.282**   |
|                               | (4.86)   |          | (0.53)    | (5.53)   |          | (2.78)    |
| Intermediate mobility         |          | 0.115    |           |          | 0.0956   |           |
|                               |          | (0.34)   |           |          | (0.45)   |           |
| High mobility                 |          | -0.157   |           |          | 0.117    |           |
|                               |          | (-0.49)  |           |          | (0.47)   |           |
| Beliefs about losers          |          |          | 0.0747*   |          |          | 0.136***  |
|                               |          |          | (2.01)    |          |          | (6.38)    |
| Beliefs about non-competitors |          |          | 0.389***  |          |          | 0.348***  |
|                               |          |          | (8.48)    |          |          | (11.39)   |
| Beliefs about winners         |          |          | 0.392***  |          |          | 0.343***  |
|                               |          |          | (10.64)   |          |          | (14.59)   |
| Constant                      | 0.515*** | 1.569*** | -0.510*** | 0.814*** | 1.585*** | -0.700*** |
|                               | (4.36)   | (7.44)   | (-4.04)   | (8.55)   | (11.60)  | (-10.84)  |
| N                             | 474      | 474      | 474       | 1640     | 1640     | 1640      |

Note: OLS regression models with behavioral hostility as dependent variable. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . t-values in parentheses. Standard errors clustered at the group level. Models 1, 2, and 3 refer to groups of six participants, and models 4, 5, and 6 refer to groups of 20. Reference categories: non-competitor, low-mobility condition.

The results corroborate our initial findings. Losers consistently spend most on diminishing others' assets, followed by winners and then non-competitors. This pattern is consistent across both small and large groups (refer to Models 1 and 4 in Table 7; groups of 6: losers vs. non-competitors, 1.761,  $t = 6.56$ ,  $p < 0.001$ ; winners vs. non-competitors, 1.09,  $t = 4.86$ ,  $p < 0.001$ , losers vs. winners in a test of linear combination,  $F = 4.82$ ,  $p < 0.05$ ; groups of 20: losers vs. non-competitors, 1.38,  $t = 8.82$ ,  $p < 0.001$ , winners vs. non-competitors, .97,  $t = 5.53$ ,  $p < 0.001$ , losers vs. winners in a test of linear combination,  $F = 4.45$ ,  $p < 0.05$ ).

Treatment effects provide no support for the inverted-U hypothesis, aligning with our analysis of structural and subjective relative deprivation. No

significant differences across treatments were observed for either small or large groups (refer to Models 2 and 5 in Table 7).

In a subsequent analysis, we incorporated beliefs about the extent to which other players in the group reduced their group members' assets. This was differentiated by the categories of losers, winners, and non-competitors. The beliefs of participants in all three categories significantly predict behavioral hostility (Models 3 and 5 in Table 7). Interestingly, when accounting for beliefs, the effect of the instigator's status diminishes. In small groups, once beliefs are factored in, the disparity in hostility between winners and non-competitors almost vanishes and becomes statistically insignificant (Model 1:  $1.09, t = 4.86, p < 0.001$ , Model 3:  $0.09, t = 0.53, p = 0.596$ ). In contrast, the surplus hostility of losers remains significant and substantial when adjusting for beliefs (Model 1:  $1.76, t = 6.56, p < 0.001$ , Model 3:  $1.58, t = 3.06, p < 0.05$ ). A similar pattern is observed in large groups. When controlling for beliefs, hostility in winners aligns closely with that in non-competitors (Model 4:  $0.97, t = 5.53, p < 0.001$ , Model 6:  $0.28, t = 2.787, p < 0.01$ ), whereas the coefficient of hostility in losers remains more than double the coefficient of hostility in winners even when adjusting for beliefs (Model 4:  $1.38, t = 8.82, p < 0.01$ , Model 6:  $0.62, t = 6.19, p < 0.001$ ).

Why might winners' perceptions of others' hostility shape their own aggressive actions? This remains open to speculation. Winners might exhibit aggression because they anticipate potential threats from others: a kind of pre-emptive retaliation. Alternatively, their actions could stem from a desire to elevate their status. Conversely, the hostility displayed by losers seems to be rooted in frustration, as indicated in Table 4.

## EXPLORATORY RESULTS

Exploiting our heterogeneous and extensive sample, we conducted a series of exploratory analyses using socioeconomic background (gender, age, education), political ideology, and psychological measures (risk preferences, social value orientation) as predictors of competition entry, subjective frustration, and behavioral hostility. Most predictors were gauged with direct survey questions (see OSM2 for details). However, social value orientation was assessed with an incentive-compatible method (Crosetto et al. 2019; Höglinger and Wehrli 2017; Murphy and Ackermann 2014).

Descriptive statistics are presented in OSM3 Table SVI, and regression outcomes from combined small and large group data are in OSM3 Table SVII. Only a few variables showed significant effects. Risk-tolerant individuals ( $AME = 0.060, z = 19.35, p < 0.001$ ) and women ( $AME = 0.074, z = 4.41,$

$p < 0.05$ ) were more inclined to compete, whereas those with graduate degrees were less so than were individuals with a high school education or less ( $AME = 0.075, z = -2.24, p < 0.05$ ). Risk tolerance correlated positively with both perceived frustration ( $0.044, z = 2.23, p < 0.05$ ) and behavioral hostility ( $0.043, z = 4.75, p < 0.001$ ). The latter was also more pronounced among right-leaning individuals than among centrists ( $0.312, z = 5.96, p < 0.001$ ).

## DISCUSSION AND CONCLUSION

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The rise of populism has reignited scholarly interest in the paradox of societal advancement leading to frustration and social tension (Cutts et al. 2019; Ford and Goodwin 2014; Gidron and Hall 2017; Goodwin 2014; Meuleman et al. 2020; Rico et al. 2017; Rodrik 2018; Rydgren 2012; Smith 1995). However, our understanding of the specific macroconditions and micro-mechanisms that give rise to this phenomenon is still limited. A game-theoretical model proposed by Boudon (1982 [1977]) offers a promising approach to this issue. This model connects opportunities for upward social mobility within a social system to the prevalence of relative deprivation in that system. It predicts that the proportion of relatively deprived and frustrated losers in the competition for upward social mobility will follow an inverted U-shaped pattern as mobility increases.

To test this inverted U-shaped hypothesis, we designed an online experiment on MTurk with a large and diverse sample of US citizens ( $N = 2,114$ ). We allowed participants to choose whether to compete for high-status positions with their group members. The first treatment dimension was the number of positions available per group, which created low, intermediate, or high accessibility to upward mobility. The second treatment dimension was group size: small groups of six or large groups of 20. This treatment allowed a robustness check of the results. We employed three metrics to gauge relative deprivation. First, in line with Boudon's suggestion, we used a structural measure to determine the relative frequency of losers within a group. Second, we assessed participants' subjective frustration upon discovering the competition's outcome using a Likert-type scale. Last, we measured behavioral hostility through the joy-of-destruction game (Abbink and Sadrieh 2009), which evaluates participants' inclination to decrease their group members' payoffs at a personal cost. This measure is of specific interest as it has been linked to the intensity of competition for limited resources in everyday life (Prediger et al. 2014).

We found no inverted-U-shaped relative deprivation prevalence when mobility increased but we did find falling relative deprivation – a result that was

robust to variation in group size. The pattern of decreasing relative deprivation with increasing mobility was due to a gap between behavior expected under standard rationality assumptions and observed behavior. Our findings are qualitatively consistent in both small and large groups. We observed over-entry into competition with low mobility and under-entry with intermediate mobility, with decreasing relative deprivation as a consequence. However, over-entry in a “winner takes it all” situation ( $k = 1$ ) is particularly pronounced in the larger group with 20 actors and less noticeable in the smaller six-person group. Moreover, comparing our results with our previous experiments with six-person groups (Berger and Diekmann 2015) we did not observe over-entry in situations with one vacant position. Only in one of the three former experiments subjects invested slightly more than predicted. Hence, results are not consistent in small groups. At best, we can say that actors vastly overrate their chances of winning when the opportunity is small and the group is large.

The entire deductive sequence leading to the inverse U-type mobility-frustration relation is not confirmed by the data due to deviations from the game-theoretic predictions of the mixed equilibrium. In real-world situations, individuals typically have more time to make choices, such as completing job applications, and allocating more time to the decision-making process, potentially leading to more rational behavior.

At the same time, the discrepancy between predicted and observed entry into competition with intermediate accessibility to upward social mobility becomes even greater when actors are concerned not only with their own payoffs but also with the payoffs of others (Otten 2020; 2022). More frustration under increased chances for upward social mobility is thus unlikely to emerge under the conditions exemplified by the model. However, various factors may reinforce this phenomenon. Consider the classic finding by Stouffer et al., which suggests lower average satisfaction with promotion opportunities in those branches of the US Army offering the highest objective chances (Stouffer et al. 1950). Importantly, mid-twentieth century soldiers constitute a specific demographic group: males. It is well-established that, on average, men exhibit more competitive behavior than women, particularly when winners are selected by performance rather than by lot (Berger, Osterloh and Rost 2020; Niederle and Vesterlund 2007; 2011). Consequently, over-participation and increased frustration when mobility is relatively high may well occur in specific subpopulations with pronounced competitiveness. Behavioral contagion in networks could also encourage over-entry (Guilbeault et al. 2018; Manzo 2011).

Beyond the question of over-entry, we found an intriguing, exploratory result. Although losers exhibited higher frustration than winners and non-

competitors, as the model suggests, we found a different pattern for behavioral hostility. Not only losers but also winners showed more hostility than non-competitors. There are two potential explanations. The first is a causal effect. Winners might engage in pre-emptive retaliation due to fear of aggression from losers, or they might wish to increase their status. The second involves self-selection, suggesting that individuals drawn to competition might inherently possess more aggressive traits (Kajonius et al. 2015; Paulhus and Williams 2002; Tesi et al. 2023; Zitek and Jordan 2016). Additionally, a combined effect is plausible: Intense competition might heighten inherent tendencies toward antisocial behavior (Berger, Osterloh, Rost et al. 2020). In our study, irrespective of the exact underlying mechanisms, we can definitely exclude the idea that heightened hostility in losers stems solely from self-selection. The division into winners and losers was made at random. Nonetheless, losers consistently displayed more hostility than winners.

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In our exploratory analyses, we observed that risk-tolerant individuals and women were more inclined to engage in competition. However, individuals with higher education levels demonstrated a lower propensity for competitiveness. Those with a higher risk tolerance expressed higher feelings of frustration and exhibited more aggressive behavior. The pattern of increased hostility was also notable among individuals with right-leaning political views. Increased competitiveness in women is surprising at first glance, as typically, women are less competitive than men (Balafoutas et al. 2018; Niederle and Vesterlund 2007; 2011). At the same time, our competition game used a specific method of winner selection: the lot. Random selection has been reported to increase competitiveness in women (Berger, Osterloh, and Rost 2020).

A significant limitation of our study is participant dropout, which primarily occurred in the larger groups and the high-mobility treatment. As a result, the findings for this specific condition should be interpreted with caution. Nevertheless, we believe our main result – decreasing frustration as social mobility improves from low to intermediate – is reliable, because we experienced minimal dropout in the low- and intermediate-mobility treatments. Moreover, the results are quite consistent for both small groups, which were largely unaffected by dropout, and for large groups.

To summarize our main results: First, over-entry into competition was prevalent in low-mobility scenarios in large groups, whereas significant under-entry was noticeable in both intermediate and high-mobility situations. This trend led to a reduction in relative deprivation across all the conditions. Second, both winners and losers displayed higher antisocial tendencies than did non-competitors. Importantly, there was no corresponding rise at the aggregate

level as mobility increased, suggesting that self-selection at least partly explains nastiness in competitors.

Drawing on our findings, future studies of social structure and relative deprivation should focus on elements that intensify the competitive drive for upward social mobility when more opportunities arise. We assumed that subjects' decisions were governed by the strict rationality standards of game theory and by a utility function that excluded nonmonetary arguments. We also assumed risk neutrality for predictions made from expected monetary values. No learning processes were taken into account in any of the experiments discussed so far. Subjects had to make decisions in "one-shot" interactions. It is very likely that subjects will adapt their behavior if they have the opportunity to repeat interactions (with strangers). Further research may also consider relaxing the model assumptions or applying alternative decision principles from bounded rationality theory. Research should also investigate whether the pronounced hostility in winners stems from self-selection or has some causal elements. In parallel, it is worth examining whether societal advancement might amplify frustration due to a growing disparity between winners and losers, rather than focusing exclusively on the "losers of modernization," as suggested by Boudon's model.

Although the results of the few available experiments are to some extent inconsistent and the central prediction of the inverse U-shaped relationship between mobility and the extent of frustration is not in accordance with previous experimental findings, the model should by no means be abandoned prematurely. To put it in Boudon's own words: "It is worth noting, incidentally, that the model also provides the logical skeleton for a research project in experimental social psychology that would very probably lead to some very instructive results and would perhaps put one in a better position to understand phenomena like envy" (Boudon 1982 [1977], p. 123). As he also emphasizes, the basic model can be extended in various directions. For example, he suggests some variants that take into account heterogeneity with regard to resources or position goods, so that the value of a successful application decreases with the number of vacant positions (Boudon 1982 [1977], p. 122). Various alternative operationalizations are also conceivable when measuring the degree of frustration. The underlying strict rationality theory of mixed Nash equilibrium, which we employed in our study, makes very restrictive assumptions; bounded rationality principles and learning may come closer to observable behavior. In any case, the strength of the model is that precise hypotheses can be derived for different parameter constellations and different model variants, which can be tested on empirical data.



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BOUDON AND THE EXTRATERRESTRIALS.  
A GENERATIVE MODEL  
OF THE EMERGENCE OF A RELIGION

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The emergence of new religions remains poorly understood, partly due to the lack of detailed historical data on their earliest stages. Festinger et al.'s seminal book *When Prophecy Fails* is a counterexample. This book provides a very detailed ethnographic account of the unplanned emergence of a small UFO religion, including the formation of supernatural beliefs, rituals, and leadership structures.

This paper asks how this new religion could emerge in an unplanned way so quickly. This process includes several *astounding facts* in the sense described by Boudon (1976): First, the leaders do not set out to create a new religion, yet within just six months, a religion emerges, complete with beliefs, rituals, and norms; second, the extraterrestrial messages are often vague and unclear, yet the resulting religion develops into a relatively well-structured system; third, the group's prophecies and predictions invariably fail, yet rather than leading to the group's immediate collapse, these failures often spur further ideological development; fourth, despite the fact that the group has successfully created a religion, it does disintegrate.

The central question I address is: What social mechanisms enabled the unplanned emergence and subsequent disintegration of this small religion? I define social mechanisms as typical causal relationships that operate within one or more social games (Stolz 2023).<sup>1</sup> Furthermore, I conceptualise religions as social games of exchange with supernatural players (a definition elaborated further below).

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*I thank Gianluca Manzo, Richard Breen, Andreas Diekmann, David Voas, Lukas Spinner, and Denise Hafner Stolz for their helpful suggestions and critique. All remaining possible errors are mine.*

<sup>1</sup> For a discussion of different definitions of “mechanism”, see Hedström (2005), Manzo (2014).

To address my research question, I develop a generative model in the Boudonian tradition – namely, a dynamic model that explains the phenomenon (explanandum) using simple assumptions and mechanisms (Boudon 1979, 1981).<sup>2</sup> Unlike statistical models, such models have the form of games in which simplified players conjointly create an emergent outcome (Manzo 2007). My model takes the form of an improvisational game between leaders and followers, aimed at establishing communication with a supernatural player.<sup>3</sup> Drawing on literature from theatre improvisation, I identify the rules and techniques that inform the model. The central argument of this paper is that some religious groups employ techniques similar to those used in theatrical improvisation, albeit in a latent way.

To the best of my knowledge, this is the first theoretical attempt to model the unplanned emergence of religion in this manner. The paper makes three key contributions: first, it introduces a novel model to explain the unplanned cultural evolution of religious groups; second it proposes a mechanism, which underpins cultural improvisation more broadly; and third it advances theoretical understanding of the Brotherhood case described by Festinger et al. (2008 [1956]).

Note that I use the book *When Prophecy Fails* in an unusual way. Festinger et al. (2008 [1956]) examined the Brotherhood, a small UFO group that incorrectly predicted the end of the world, with an interest in cognitive dissonance. The large literature following this publication is concerned with testing Festinger et al.'s theory that failed prophecies will lead to increased evangelizing. This research has led to a significant number of disconfirmations of the theory (for

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- 2 Using such models, the researcher in a first step presents a simple model that is able to recreate the *astounding facts*; in a second step, it has to be shown that the supposed mechanisms actually played a role in the case to be explained. It is in part for this technique of seeking out puzzles in the social world and constructing simple game-like models that produce the puzzling phenomenon as their outcome that Boudon has become famous (1976, 1981, 1982; Hauser 1976). In a famous exchange with Hauser (1976), Boudon (1976) writes: "Given my objective, that is, to answer a number of questions of the why type, I came to the idea of building a model roughly describing the basic mechanisms responsible for educational and social inequality, to see whether it generated the 'paradoxical' outcomes some of which are listed above." For a discussion of rational models as explanatory tools, see Raub (2020).
  - 3 Such models can be seen as games that players may play, but do not necessarily have to take the form of economic game theory (Stolz 2023). For an introduction to economic game theory, see Kreps (1990), Gibbons (1992b). For game theory in sociology, see Breen (2009). For an analysis of the improvisational creation of narrative in children's play see Sawyer (2002). I thank Gianluca Manzo for pointing the Sawyer reference out to me.

overviews, see Johnson 2011; Dawson 1999; Melton 1985).<sup>4</sup> My paper, on the other hand, is concerned with the question of how new religions may appear in an unplanned way, seeing the Brotherhood as an especially well-documented test case.

Although this paper focuses on the evolution of religion, it does not engage with the literature on the long-term evolution of religion over the course of human history. Instead, it addresses how evolutionary mechanisms, such as variation and selection, can shape specific religions over relatively short periods.<sup>5</sup>

## THE CASE: THE BROTHERHOOD AND THEIR SCIENTIFIC OBSERVERS

The case of the Brotherhood, which formed around Dorothy Martin and Charles Laughead, was first described by Festinger et al. (2008 [1956]).<sup>6</sup> The group comprised approximately 20 to 30 members, excluding the scientific observers, and operated in two locations: Oak Park, Illinois, where Martin lived (called Lake City in the book), and East Lansing, Michigan, where Laughead resided (referred to as Collegeville in Festinger's account) (Jenkins 2013). The group existed for roughly 7 to 8 months.

Dorothy Martin, referred to as Marian Keech in Festinger's book, was a housewife with a substantial background in holistic practices. In the spring of 1954, she began practicing automatic writing. Initially, she claimed to receive messages from her deceased father, but she soon became convinced

4 In my view, this literature has clearly shown that Festinger et al.'s thesis must be rejected - and did not work already for the Brotherhood themselves. Cults that fail with their apocalyptic prophecy (in the sense that the world does not end) may, but most often do not, react with increased evangelizing (Dawson 1999). And the Brotherhood themselves were faced not with one but with many failed predictions - to which they reacted with a host of different strategies, evangelizing being only one of them (and not the most important) (Tumminia 2005).

5 For a discussion of different types of theories of evolution in the social sciences, see Diekmann (2004).

6 In Festinger et al.'s book (2008 [1956]), Dorothy Martin was given the pseudonym Marian Keech and the co-leader, Charles Laughead, was labelled Dr. Armstrong. In this paper, I use the real names of the people and places involved as described in Clark (2007) and Jenkins (2013). Additional information on the case, its historical background and the continuing fate of Dorothy Martin (who later called herself Sister Thedra) and the Laughead couple is given in Clark (2007). There are current spiritual entrepreneurs who work in the continuity of Dorothy Martin. See for example Alexandriah Stahr who acknowledges her indebtedness to Sister Thedra, <https://www.star-essence.org/about/lord-sananda-and-sister-thedra>, accessed on July 7, 2025.



that extraterrestrial beings were contacting her. A small group of individuals became interested in her claims and assisted her in typing the messages. Doctor Charles Laughead, a medical doctor employed at Michigan State University, led a student group focused on UFOs (referred to as The Seekers). In the spring of 1955, Charles Laughead and his wife reached out to Dorothy Martin due to their interest in her messages. The couple's close collaboration with Martin led to the emergence of the Brotherhood and the informal integration of the Seekers into the overall group.

222 Leon Festinger, Henry W. Riecken, and Stanley Schachter, all social psychologists with a keen interest in cognitive dissonance, came across an article about a group predicting the imminent end of the world. Recognizing a unique opportunity to study cognitive dissonance in action, they joined the group as covert participants and enlisted two student observers to assist them. The period of observation lasted from November 19 to December 27, 1955. However, the researchers were able to reconstruct events from the preceding months through accounts and documents provided by group members.

The group experienced not just one but a series of failed prophecies. Some of the most notable examples include: on July 23, the aliens were expected to land in a nearby field. Between December 17 and 20, the aliens were predicted to arrive on three separate occasions to collect the believers. On December 21, the cataclysm was supposed to occur, and on December 24, the aliens were anticipated to appear during a carol-singing event. However, the aliens never arrived, and the cataclysm failed to materialise. These disappointments normally led to interesting new cultural elements of the group's ideology. It is this fact that we will analyse in depth in this article.

## A GENERATIVE MODEL OF THE EMERGENCE OF A RELIGION

### RELIGIONS AS SOCIAL GAMES WITH SUPERNATURAL PLAYERS

*Social games.* For our analysis of the Brotherhood, we start out with a newly formulated general theory of social games (Stolz 2023). The theory of social games analyses social life as a multitude of interacting social games. A social game is a form of organization of the social sphere in which players engage in actions, which are shaped by resources and goals, rules and sanctions, as well as symbols and meanings. The social game creates game space, game time, and leads to game outcomes. The game takes place in a context. For example, the theory of social games analyses conversations, ping pong clubs, criminal gangs, large organizations, or countries as social games. In contrast to other theories, the theory of social games claims that every game action is always influenced



by resource-goal, rule-sanction, and representation-meaning considerations. For example, the action of playing “Rock” in Rock-Paper-Scissors is played with the goal of winning (resource-goal dimension), it is following the rule that only three actions are possible (rule-sanction dimension), and it consists of making a fist that represents a “Rock” (representation-meaning dimension). The theory claims to be more straightforward and to have a clearer link to empirical research than other grand theories. It incorporates economic game theory for the purpose of modelling the deep structure of games, but claims to be especially useful for empirical, qualitative, and quantitative research (Stolz and Lindemann 2019).<sup>7</sup>

*Religions: Social games with supernatural players.* Religions can be reconceptualised as social games that incorporate supernatural players (Figure 1). These supernatural players—referred to as Gods, spirits, angels, devils, and similar entities—are fundamentally different from regular players. They are believed to possess significant powers to influence human life. As a result, humans engage in various forms of exchange with these players, such as offering sacrifices, praying, (dis)obeying, and expecting rewards or punishments in return. Often, specialists like priests, shamans, or prophets emerge, claiming expertise in interacting with these supernatural players. From the perspective of social game theory, supernatural players are representations within the game—imagined entities that gain a social existence only to the extent that the group participates in the religious game. Note that the fact that religious games generate their own Gods, remains latent. The group believes in the independent existence of its God.<sup>8</sup>

*The Brotherhood as a religion.* In a very short time, The Brotherhood had come up with a social form that can be interpreted as a religious game as defined above. Let us look at some of its components:

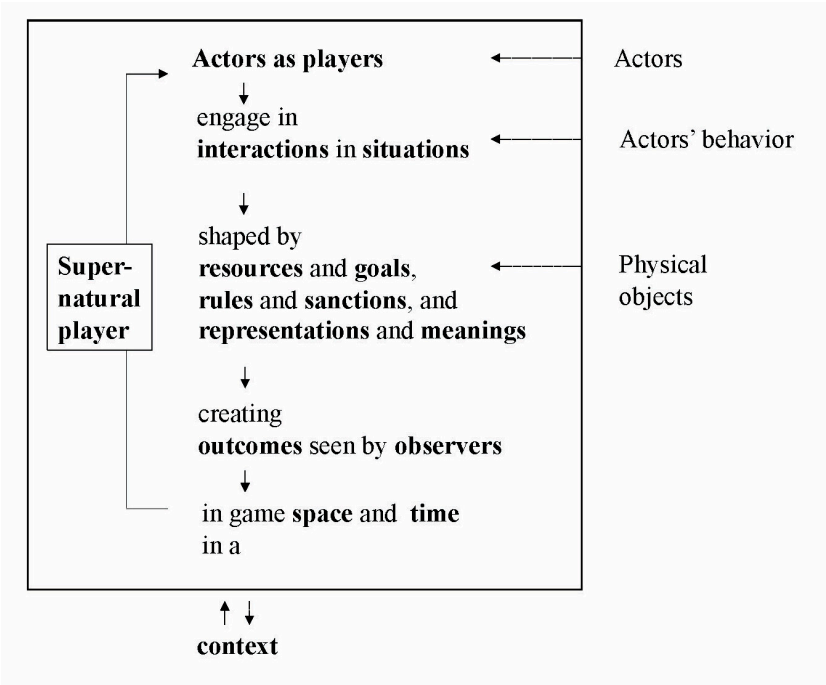
The rituals or *game-actions and -interactions* of the Brotherhood consisted of “sitting for messages,” which involved the group gathering for sessions of automatic writing to receive teachings and directives from the supernatural entities Sananda or the Creator. Members could also participate in the reading of previously received messages or individually “sit for messages” when seeking advice on personal problems.

The overarching *goals* of the Brotherhood game included spiritual growth, attaining more “light” and “inner knowing,” and achieving a “higher density”

7 I do not have space to explain the differences between social game theory and economic game theory more extensively, but plan to do this in further publications.

8 Historically, humanity took a long time to recognise this phenomenon. This insight became a central theme of the Enlightenment and its aftermath, with Ludwig Feuerbach (1983 [1843]) being a pivotal figure in exposing this latent process.

Figure 1: Religion - Social Games With Supernatural Players



Note: The arrows pointing into the rectangle signify that the game transforms actors into players, actors' behaviour into game actions and physical objects into game resources. The arrows inside the rectangle point to the fact that games are recursive.

with the guidance of extraterrestrial beings. Another significant aim was to be saved and transported aboard a spaceship before the anticipated cataclysm.

Membership in the Brotherhood required adherence to several informal *rules*. Members were expected to believe in the messages received by Dorothy Martin and avoid critical thinking or questioning. Commitment was demonstrated through regular attendance at meetings and the execution of instructions from the messages, such as spreading light, fasting, abandoning work, or relocating to live with Martin. Members are also encouraged to refrain from smoking and consuming meat.<sup>9</sup>

The group shared a set of *beliefs* or *symbolic representations* centred around supernatural players. According to their worldview, the universe contained numerous planets, including "Clarion" in the "constellation of Cerus," as well as a universal school called "the Losolo", inhabited by advanced, human-like beings

9 Page numbers in the Festinger et al.'s book are in the following given without every time citing the author and year information.

known as the “Guardians”. The leader of these beings was called “Sananda”. According to the group’s beliefs, Sananda and his allies had for a long time been engaged in a cosmic battle against “Lucifer and the scientists”, a struggle that extended to Earth. Through Dorothy Martin and the “Brotherhood”, Sananda and his allies communicate with humanity, offering warnings and guidance.

One of the group’s core beliefs was tied to an impending global catastrophe predicted to occur in December 1955. This event, involving a massive earthquake and flood, was expected to devastate the earth. However, shortly before the catastrophe, spaceships – referred to as “tola” or “avagada” – were expected to arrive to rescue the group members. The group also employed a unique *terminology*. For instance, “Beleis” meant “hello,” “scice” referred to “the one in disguise,” “lear” denoted an “earthbody,” and “inner knowing” signified “conviction guided by the aliens.” The phrase “I left my hat at home” served as a password for gaining access to the spaceship.

This fully formed religion was created in only six to seven months. Readers familiar with the literature on UFO religions from the 1950s will recognise many of the Brotherhood’s themes as recurring motifs in the broader UFO or abductee milieu.

However, Dorothy Martin and her followers have crafted a distinct new religion by weaving these familiar elements into a narrative uniquely tailored to the individuals making up the Brotherhood. To understand how this was possible, we now turn to ideas from theatre games.

#### THEATRE GAMES AND THE EMERGENCE OF NEW IMAGINARY WORLDS

Theatre improvisers use simple yet powerful techniques to swiftly create new imaginary worlds (Johnstone 1981). For spectators witnessing skilled theatre improvisation for the first time, the experience can be astonishing—how do performers spontaneously craft intricate stories with characters, plot twists, and engaging narratives? The secret lies in a set of fundamental rules that guide improvisers, encapsulated in the acronym AIJR: Accept, Improve, Justify, Reuse (Halpern, Close and Johnson 1993; Johnstone 1981; Salinsky and Frances-White 2012).

The first rule, *Accept*, is foundational in the improv world and often phrased as “saying yes” to what others contribute. Acceptance means embracing the reality implied by a fellow improviser’s offer. Accepting does not necessarily imply reacting positively within the story. For instance, if Improviser A says, “I’m the plumber, here to fix your sink,” an accepting response from Improviser B might be, “Thank goodness you’re here; we’ve been waiting for hours!” or “I hope you can do better than the last three guys. They were useless!”

In contrast, a blocking response – rejecting the reality of the offer – might be, “We don’t have a sink,” or “You’re not a plumber.” Blocking disrupts the flow of improvisation by denying the premise introduced by a fellow performer, stalling the collaborative storytelling process.

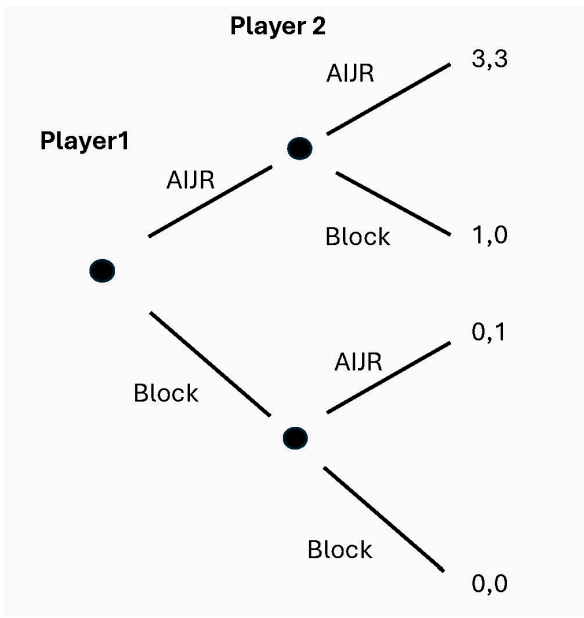
Second, *Improvise*. Improvisers must not only accept offers from their fellow players but also contribute new material spontaneously (Halpern et al. 1993). Spectators often marvel at how performers introduce unexpected, original elements that seamlessly integrate into the unfolding story. The key to this skill is the improviser’s willingness to courageously suggest new ideas without knowing what they will lead to, that is, without planning ahead. This approach is often summarised as “don’t be prepared,” and “say the first thing that comes to mind.” This spontaneity can only be successful if all other improvisers follow all other core techniques—acceptance, justification, and reuse—which collectively ensure that even surprising ideas are supported and incorporated effectively into the story.

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Third, the principle of *Justify* ensures that every element introduced into an improvisation, no matter how random, puzzling, or accidental, becomes meaningful (Halpern et al. 1993; Johnstone 1981). Justifying involves providing an explanation for enigmatic elements by connecting them to established details in the scene. For example: If Improviser A raises her arm without explanation, Improviser B might say, “Please hold on to the handle; the bus is about to turn.” If two players act as if the coffee machine was in different spots, a third might compliment them on having put their coffee machine on rollers. If A is introduced as Jack, but later claims that his name is John, one of the improvisers will explain that “this is Jack, but everybody calls him John because it’s easier to pronounce”. By using justification, mistakes are transformed into opportunities, and external disruptions – such as a phone ringing, a spectator snoring, or noise outside – are treated as purposeful and integrated into the evolving narrative. This technique ensures that no improvised element is ever seen as a mistake.

The rule of *Reuse* involves revisiting and integrating prominent elements introduced earlier in the story. For instance, if a scene begins with an elderly woman called “Jane” living alone on an island, improvisers will see to it that the elements “elderly woman called ‘Jane’”, “living alone”, and “island” will be reincorporated in the future happenings. By doing so, the story will naturally condense around these concepts and bring a story to light that could not have been anticipated. Players create a story “by remembering incidents that have been shelved and reincorporating them” (Johnstone 1981). This technique not only gives the story coherence but also provides a sense of satisfaction to the audience, as earlier threads are tied together in meaningful ways.

Figure 2: Theatre Improvisation Game



Note: Numbers are preferences of Player 1 and Player 2, respectively.

*Theatre improvisation: a symmetrical AIJR model.* Theatre improvisation between two players can be modelled with economic game theory as in Figure 2. Player 1 is influenced by an initial stimulus, either by initial suggestions by the public or the previous game. She can either use the combined rules AIJR or block. If she blocks, Player 2 can either use AIJR and rescue the scene, or she can block herself. If she uses AIJR, the scene may go on, but for simplicity, we only consider the case of two moves. The payoff matrix shows that in the theatre game, both players will try to follow the AIJR rules. Provided players have enough practice in AIJR, an unplanned improvised cultural world will emerge very quickly. The added insight of presenting the game in such a formal way is to show that the theatrical improvisation is a coordination game with one equilibrium in (AIJR, AIJR). Players prefer cooperating in any case and defect only by mistake. Note also that both players are on an equal footing (even though they may have a different status in the played scene). Both points will be different in the religious improvisational game we will analyse below.

THE AIJR MODEL APPLIED TO THE BROTHERHOOD

*Two changes to the model: latency and asymmetry.* We now seek to apply the AIJR model to our case, the Brotherhood. We find two main differences.

For one thing, in contrast to theatre improv, religious improvisation in the Brotherhood is latent. Theatre improvisers are aware that they are creating a new cultural world from scratch, the Brotherhood believes that they are discovering an existing world, consisting of extraterrestrials, spaceships, and cosmic battles.<sup>10</sup> For another thing, while theatre improv is symmetric, the Brotherhood improvisation is asymmetric. Leaders are more powerful than followers, have the monopoly on using AIJR techniques, and can sanction followers. Followers mainly have the choice of staying or leaving.

*Three examples.* In what follows, I show how the Brotherhood uses similar techniques as theatre improvisers – namely, the AIJR model. In our first example, we see how the Brotherhood learns the meaning of a new word and acquires a new mythical story.

#### Example 1: The appearance of the Scice

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The example starts with a message received by Dorothy Martin through automatic writing:

Sara and Justine were cast as the boy and the girl; to each a love of the Creator. As they came to the great city of the centre of the Earth, which is called the CITY of the self – the child, Sara, asks Justine: “Which way to the Father’s house?” To Sara, Justine said: “To be a Carter, or one who finds his way, is the great cast for which he was created”. As they journeyed to the city of the Self, in the centre of the Earth, they were overtaken by the coy little scice, which was a mink. He was in disguise of the rabbit, which was a cousin to the grouse. (p. 74.)

In this example we can see that Dorothy Martin uses AIJR techniques on the level of the individual messages. New words are *accepted*, and additional words are *improvised* by chaining them to already existing words. For example, the words “Sara and Justine” are accepted by the newly improvised words “were cast as the boy and girl; to each a love of the Creator”. The main characters Sara and Justine are given some kind of role by a new character: the Creator. The next sentence again accepts that the story is about Sara and Justine and improvises that they come to “the great city of the centre of the earth”. New words and

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<sup>10</sup> If readers doubt the existence of latent improvisations, there is a party game showing its possibility. In this game, player 1 is told that she should guess “What happened to grandma.” She can find out by making hypotheses and checking them with player 2 who can only answer with yes or no. What player 1 does not know is that player 2 answers randomly, according to a list of random zeros and 1’s given to her. This game will lead player 1 to invent a wild story out of randomness – because she believes that there is actually such a story out there.

sentences also *justify* previous elements to make them understandable. We are told that Sara and Justine come to “the great city of the centre of the Earth”. The following words explain that this city is the “city of the self”. The previous sentence is justified by giving it some symbolic meaning, in that we now understand that the characters are on a journey to increased self-understanding. Salient words are *reused* and the meaning of the message forms around these words. By reusing the words “Sara”, “Justine”, “City of the self”, and “centre of the Earth”, the story naturally assembles around these concepts. Of course, since every step also adds new elements, many questions remain. One of them is just what is meant by the enigmatic word “SCICE”.

An important point to understand is that the group does not only use AIJR at the level of the messages received by Dorothy Martin, but in the course of the entire group’s life. Thus, to continue with our example, it so happens that in another message the group is told that the extraterrestrials will land on the Lyon’s field in the near neighbourhood on a specific date. When the group stands awaiting the extraterrestrials for hours at the Lyon’s field, no aliens show up. However, a man walks along the road and Dorothy Martin briefly interacts with him. Dorothy Martin finds that the man has a mysterious allure. The group drives home, somewhat disappointed, but Dorothy Martin now receives the following message:

It was I, Sananda, who appeared on the roadside in the guise of the scice. (p. 6.)

This is a classic example of AIJR justifying. The meeting with the man (and the non-appearance of aliens) is now understandable. The man on the roadside was really the extraterrestrial they had been waiting for. He was Sananda. At the same time, the group has now learnt what the word “scice” means (“one who is in disguise”). Note the structure of the justifying technique. A previous and enigmatic element (“scice”) is made understandable by justifying it through a current element, thereby at the same time presenting the current element as intended by the previous element.

#### Example 2: The child in Collegeville

In our second, historically earlier, example, the Laughead couple (Dr. Laughead and his wife), who live in Collegeville contact Dorothy Martin by letter since they have heard from her spiritual activity and propose a meeting. Dorothy Martin is thrilled. She remembers that she has previously received a message by the extraterrestrials saying:



Go to Collegeville. There is a child there to whom I am trying to get through with light. (p. 57.)

In her view, the child from Collegeville can be no other than Mrs. Laughead. The “Getting through with light” means that the message of Dorothy Martin (the “light”) should be given to Mrs. Laughead. In this way, the previous enigmatic message is justified, made understandable and is seen to predict the fact that Mrs. Laughead and her husband now seek contact with Dorothy Martin.

### Example 3: The end of the world and the Christmas message

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Our third example is the most complex, since it involves several steps, it also includes the central failed prophecy that made the group famous. On the 15<sup>th</sup> of August, Dorothy Martin receives the following message.

When the resurrected have been resurrected or taken up - it will be as a great burst of light... the ground in the earth to a depth of thirty feet will be bright... for the earth will be purified. [...] In the midst of this it is to be recorded that a great wave rushes into the Rocky Mountains. (p. 72.)

Dorothy Martin and Dr. Laughead interpret this message as saying that there will be a great catastrophe on earth on the 21<sup>st</sup> of December (possibly referring to the winter solstice).<sup>11</sup> This interpretation is again a nice example of accepting and justifying, as this message explains why the group has not yet had direct, face-to-face contact with the extraterrestrials. The group now understands why: The extraterrestrials are waiting to save the group just before the planned catastrophe.

After the aliens fail to arrive on three separate occasions as predicted between December 17<sup>th</sup> and December 21<sup>st</sup>, and even the catastrophic event expected on December 21<sup>st</sup> does not occur, Dorothy Martin receives the following message:

Not since the beginning of time upon this Earth has there been such a force of good and light as now floods this room and that which has been loosed within this room now floods the entire Earth. As thy God has spoken through the two who sit within these walls has he manifested that which he has given thee to do. (p. 199.)

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11 I thank David Voas for this suggestion (personal communication).



This serves as a compelling example of AIJR. It can be interpreted to mean that the aliens did not arrive, nor did the catastrophe occur, because of the group's actions. The group generated so much positivity and light that a catastrophe –and the aliens' intervention – are no longer necessary. Simultaneously, the group appears to have a new mission: spreading the light ("that which he has given thee to do"). This new improvised element both justifies and explains the puzzling previous events (the absence of the aliens and the catastrophe) while reframing them as preparatory and predictive of the current element. Additionally, the Brotherhood now has the foundation for a new myth: through their collective actions of goodness and light, they averted a great catastrophe on Earth.

*How the Brotherhood used AIJR: some general points.* Having presented three examples, we can now attempt to make broader observations about how the Brotherhood employs AIJR mechanisms.

The group uses the technique of *accepting* in a remarkably consistent way. In particular, the group leaders appear willing to embrace nearly any message or sign—no matter how peculiar or questionable—as legitimate communication from extraterrestrials. Everyday skepticism seems to have been completely set aside. This principle is strictly adhered to, as both Dorothy Martin and Dr. Laughead exemplify this behaviour and actively reprimand members who challenge the validity of messages or express doubt about extraterrestrial signs. For example, on December 17, when someone claiming to be "Captain Video" calls, Mrs. Laughead is (understandably) inclined to suspect it is a prank.<sup>12</sup> However, she is criticised, and the message is upheld as genuine (p. 166). A similar incident occurs when five college students arrive, presenting themselves as extraterrestrials. Although Kurt Freund remarks, "They looked like college kids to me," he too is criticised and overruled. The group's eagerness to accept anything extends to embracing a new medium, Berta Blatsky (as named in Festinger's book), despite her messages from the "Creator" often contradicting Martin's and seemingly catering transparently to Blatsky's psychological needs (from an external perspective). This uncritical openness underscores the group's deep commitment to their belief system, prioritizing acceptance over discernment.

Overall, the group exhibits very little of the opposite of acceptance: *blocking*. Members almost never openly acknowledge that a prediction has failed, even

12 "Captain Video and His Video Rangers" is a television series being aired at the time and since the Brotherhood has received national prominence by this time, there is objectively a very high probability that a Captain Video calling by phone is a prankster.

in the most glaring cases, such as when promised spaceships or the anticipated cataclysm fail to materialise. Rarely do they express doubts or deny the validity of extraterrestrial messages or, more broadly, the existence of extraterrestrials. For instance, much to the frustration of the scientific observers, followers never discuss prophetic failures immediately after a disappointment. It is always the observers who raise questions, such as “why the saucers had not come” (p. 168). Instead of openly blocking, members often resort to latent blocking, quietly disengaging. Time and again, we hear of individuals simply disappearing from the group after being disillusioned by failed predictions. This raises an intriguing question: why is there so little overt blocking in the form of resistance to interpretations, challenges to “orders,” or outright rebellion? Why do members either comply with extreme directives—such as quitting their jobs, moving in with Mrs. Keech, or traveling long distances to await the aliens—or leave silently in the night without protest? This behaviour is typical in many new religious movements. The most compelling explanation lies in the group’s reliance on the leaders’ charisma (Palmer 1988). Discontented members likely sense that directly challenging the leader is futile. If unsuccessful, such a challenge would result in a significant loss of status, often leading to their departure. If successful, it would undermine the leader’s charisma and potentially dismantle the group. Thus, the most pragmatic choice is simply to leave.<sup>13</sup>

*Improvisation*—the creation of new elements within the religious framework—primarily occurs through Dorothy Martin’s automatic writing and Berta Blatsky’s oral channelling of the “Creator”.<sup>14</sup> A secondary source of improvisation arises from the group’s interpretation of their environment. Highly attuned to potential signs from extraterrestrials, the group often reinterprets seemingly mundane events as supernatural communications. For example, a phone call to Dorothy Martin is assumed to be from an extraterrestrial, an arriving scientific observer is seen as an alien, the prank call from “Captain Video” is accepted as genuine, and the five college students claiming to be “boys from Clarion” are equally believed to be extraterrestrials. Because of their heightened expectations, even non-events can serve as improvisational elements. For instance, the failure of the extraterrestrials to arrive is reframed as a test, while the absence of the anticipated cataclysm is

<sup>13</sup> Moreover, other members also perceive the fragile dependence of the group’s existence on the leader’s charisma and often react strongly against any challenge to their authority. They recognise that such challenges could destabilise the leader’s aura of legitimacy, upon which the group’s cohesion is built.

<sup>14</sup> We use the name given in the Festinger’s book since the real name of this person is apparently not known in the literature.

celebrated as a Christmas miracle in which the group's efforts to spread light are credited with saving the world.

*Justifying*<sup>15</sup> frequently occurs after group discussions, with Laughead or Martin often deciding which explanation prevails. A wide variety of justifications are employed, sometimes in combination. For instance, the aliens' failure to arrive is explained by: first, it being a test, requiring the group to undergo further training; second, the presence of strangers deterring the aliens; third, an error in the date; fourth, the aliens arriving invisibly; fifth, the aliens arriving in an unexpected form; or, sixth, the aliens finding no reason to come because the group had already spread sufficient light. Justifications are not limited to alien no-shows but are a general technique for explaining unexpected or surprising occurrences. They are used to interpret unknown terms in messages (e.g., "scice"), the inexplicable behaviour of supposed extraterrestrials (such as the five "spacemen" challenging Dorothy Martin's views, later rationalised as a test or a retraction of her teachings), unfortunate events (such as Dr. Laughead's dismissal, framed as freeing him for extraterrestrial work), or puzzling attitudes (like Mr. Martin's lack of conviction, explained by the possibility that he might die and be resurrected as a believer).<sup>16</sup>

The technique of *reusing* contributes to the construction of the religious world developed over several months. Concepts such as "Clarion," "Guardians," "Sananda," "inner knowing," "Beleis," "Parich," and the "Cataclysm of December 21<sup>st</sup>" originate from initial messages and are repeatedly incorporated into subsequent messages or group discussions. Through this iterative process, these elements undergo a form of cultural condensation, becoming ingrained in the minds of group members. Over time, their consistent repetition establishes a cultural reality that, in turn, shapes the beliefs and evolution of the group itself.

*Authority, power, and belief.* The Brotherhood cannot be fully understood without examining the *authority* and *power dynamics* within the group – an area surprisingly overlooked in the literature. Authority can be defined as the

15 For a similar list of justifications of non-healing among Pentecostals, see Stolz (2011).

16 The funniest story in the book, in my opinion, involves Mr. Martin, the husband of Dorothy Martin. Unlike his wife, Mr. Martin never believed in her prophecies but also never opposed them. Festinger describes him as a "man of infinite patience, gentleness, and tolerance" (Festinger et al., p. 53), enduring everything that unfolds in his home with stoic composure. When the extraterrestrials fail to appear at midnight on December 20, the group is told by the Creator that they will instead witness a miracle: the death and resurrection of Mr. Martin. At this point, however, Mr. Martin is soundly asleep in bed. Undeterred, the group checks his bedroom three times to see if he has died – each time finding him very much alive. To resolve this perplexing situation, the Creator proclaims that Mr. Martin is still alive because he has already died and been resurrected (p. 193).

ability of individuals to influence others based on perceived intellectual or moral superiority. Members grant authority to a leader when they believe that following the leader's guidance will yield benefits due to the leader's superior insight or knowledge. Power, on the other hand, is the ability to influence others using sanctioning threats – actions intended to impose negative consequences on non-compliant individuals. While authority motivates through respect and trust, power operates through fear of sanctions.<sup>17</sup>

234 Members follow Martin because of her “gift of writing,” which establishes her as a direct channel to the extraterrestrials. Laughead commands authority due to his advanced understanding of spiritual and extraterrestrial matters. However, manifest power is exercised largely by the supernatural figures communicated through Martin's messages. These extraterrestrials issue directives that members perceive as binding, such as fasting, quitting jobs, moving in with Martin, or traveling long distances to attend meetings. Many members describe themselves as being “under orders”. For instance, Dr. Laughead lost his job due to his devotion to the extraterrestrial cause and remains on “twenty-four-hour alert” for the Guardians.

Sanctions imposed by the extraterrestrial entities are both immediate and long-term. Immediately, non-cooperative members face the potential scorn of the group, loss of status, or even expulsion (though no actual case of expulsion is reported). In a longer perspective, non-compliance may risk losing one's “ticket” aboard a spaceship when the time comes. This dynamic of authority and power serves to suppress dissent, ensuring that members rarely block (voice objections to) messages or decisions from the leaders. Instead, they accept unsettling improvisational elements out of fear of sanction and the desire to retain group benefits. Note that the power of sanctioning is effective only as long as members perceive positive outcomes from their group involvement.

Note that the leaders' influence depends on followers continuing *belief* in leaders being true prophets, in extraterrestrials, the predicted catastrophe, and the benefits of group membership – such as emotional support and friendships. If these beliefs or perceived benefits weaken, the leaders' ability to control member behaviour diminishes. Below we will construct a model where these beliefs will be formalised.

#### AN ASYMMETRICAL AIJR MODEL

The improvisational dynamics within the Brotherhood can be schematically represented using economic game theory, as shown in Figure 3 (A). We set

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17 See for somewhat different definitions: Coleman (1990).

up the game as a repeated Bayesian signaling game (Gibbons 1992a).<sup>18</sup> The structure of this game differs from the simple improvisational game above in three respects. First, it is asymmetric. This means that only the leader holds the right to improvise, employing the AIJR techniques; alternatively, the leader may block, for example by admitting that a prophecy was incorrect. The follower, on the other hand, has two choices: to accept or to block. Second, this is a signaling game. By choosing between AIJR move and admitting failure, the leader signals to the follower whether she is a true prophet or a fraud. Third, this is a repeated game in which the follower updates her belief in the leader being a true prophet in every round.

More specifically, the game is set up as follows.

1. There are two players, a religious leader and a follower. From the point of view of the follower, the religious leader could be either a true prophet or a fraud.
2. The follower has a belief. She believes that the religious leader is either a true prophet (tp) or a fraud (f) with an initial belief  $p(tp) = \mu$  and  $p(f) = 1 - \mu$ .
3. Nature draws a religious leader with  $p(f) = 1$ . Somewhat pessimistically, we assume that the religious leader is in reality always a fraud, that her predictions will fail in every round, and that she always justifies. This means in practice that the leader's behaviour is exogenous.
4. The follower's initial belief about the leader is  $p(tp) = p(f) = 0.5$ . The fact, that she does not know whether the leader is a true prophet, or a fraud is represented by the dashed lines between nodes in Figure 2. In terms of the model, the follower does not know at what node she currently finds herself.
5. We assume that, in the eyes of the follower, both a true prophet and a fraud would be likely to justify their failed prophecies with AIJR, but that the true prophet would be more likely to admit her failure. In our main model we use the  $p(J|tp) = 0.8$  and  $p(J|f) = 0.99$ . The idea is that the fraud knows that she lies; she will therefore justify her failure in any case. The true prophet, however, believes in her powers and should be genuinely bewildered by her failure. She is still very likely to justify her failure by seeking explanations and signs that might make her vision come true—but she is nevertheless prepared to admit failure under at least some circumstances. Note that these two probabilities do not need to sum up to 1.

The play then proceeds through several rounds. In a first round, the religious leader chooses between AIJR (justifying the failed prophecy) and blocking (admitting an error, failing to justify the failed prophecy). As a result of the leader's move, the follower updates her belief about whether the leader is a true prophet or a fraud. She then chooses between accepting the leader's move (e.g.

<sup>18</sup> I thank Richard Breen for having suggested a model along these lines.

remaining in the group) and blocking (e.g., leaving the group). If the follower has not blocked in the previous round, the game enters a further round with an updated belief on the side of the follower. The follower updates her belief according to Bayes rule as follows:

where

$$\mu' = p(tp|J) = \frac{p(J|tp) * \mu}{p(J|tp) * \mu + p(J|f) * (1 - \mu)}$$

tp = true prophet; f = fraud; J = justification for failure

$\mu$  = prior belief (that leader is a true prophet)

$\mu'$  = updated belief (that leader is a true prophet)

$p(tp|J)$  = probability of a true prophet, if a justification has been given

$p(J|tp)$  = probability that the true prophet justifies

$p(J|f)$  = probability that the fraud justifies

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If the follower continues to accept the AIJR moves of the leader, she will find herself immersed in a wondrous evolution, a rapidly evolving cultural narrative. The game continues until the follower blocks.

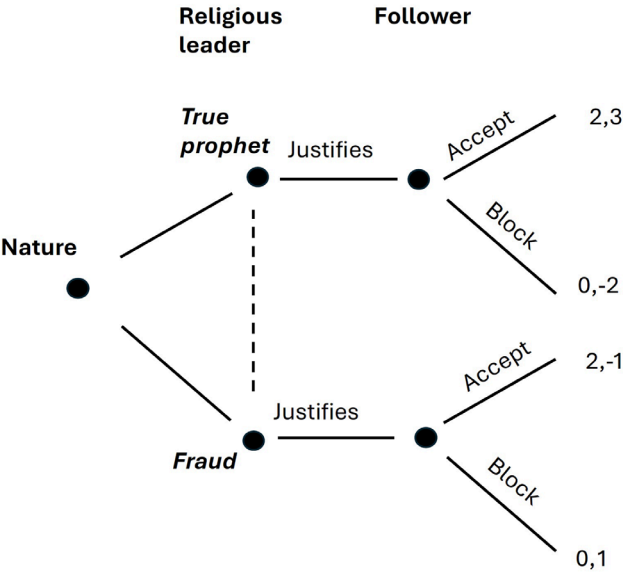
The payoffs in this model are designed to reflect the motivations of both the religious leader and the follower: For the religious leader, utility depends on whether the follower cooperates. The leader gains utility if the follower stays in the group (accepts the justification) and receives nothing if the follower leaves (blocks). This applies whether the leader is a true prophet or a fraud. For the follower, the stakes are higher and hinge on the true nature of the leader: the best possible outcome for the follower is when the leader is a true prophet and the follower accepts the justification—this outcome offers the promise of true salvation.

The worst possible outcome is when the leader is a true prophet, but the follower chooses to leave (block). In this case, the follower misses out on salvation—resulting in the lowest utility in the model. A second negative outcome, though less terrible than rejecting a true prophet, happens when the leader turns out to be a fraud and the follower stays. In this case, the follower continues to follow a false leader and becomes a misguided believer (or a dupe, a deceived follower, depending on tone—choose the wording that fits your style). The follower gains a small positive utility (1) when she correctly identifies the fraud and leaves—this reflects the relief or benefit of escaping a deceptive situation.

Since this is a repeated Bayesian game, we can analyse it in terms of belief dynamics, strategies, and long-term payoffs. Generally, we assume that the follower is choosing an optimal strategy, that is, maximizing her payoffs.

We can now look at the Bayesian updating process and calculate in what round a rational follower would block (exit the group).

Figure 3: Repeated Bayesian Signaling Improvisational Game Between a Player and a Follower



Note: Numbers are preferences of Religious Leader and Follower respectively. Dashed lines represent incomplete information of the Follower about the node she finds herself on.

Substantively, the model may be interpreted in the following way. Under the assumptions made in the model, the group may continue to operate even though multiple predictions fail, since the religious leader provides convincing justifications (with AIJR techniques). However, with every new failure and subsequent justification, the follower’s belief in the leader being a true prophet declines. At a certain threshold, the follower switches to blocking.

The threshold is the situation in which the follower is indifferent between accepting and blocking. To calculate the threshold, we get the expected utility of accepting and blocking for the follower if the leader justifies (using the payoff matrix):

$$\begin{aligned}
 EU_{\text{Accept}} &= 3\mu + (-1)(1-\mu) = 4\mu - 1 \\
 EU_{\text{Block}} &= (-2)\mu + 1(1-\mu) = -3\mu + 1
 \end{aligned}$$

The threshold is given when the follower is indifferent between accepting and blocking:



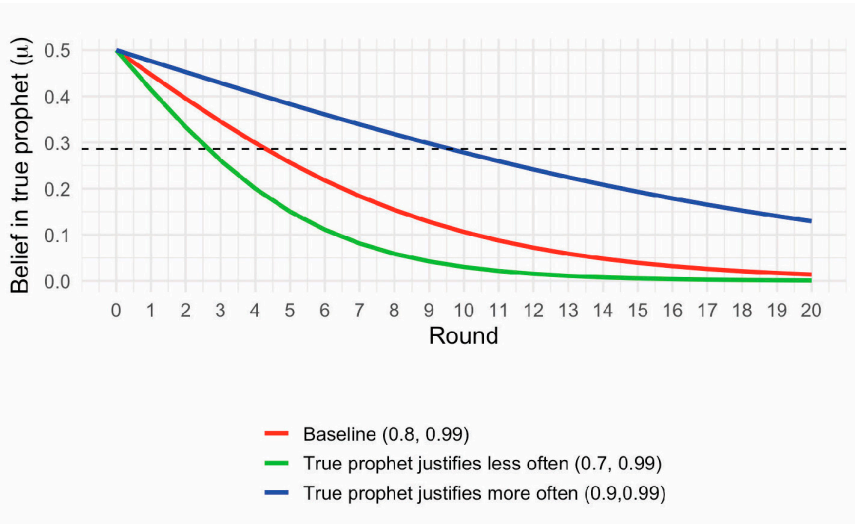
$$\begin{aligned}
 EU_{\text{Accept}} &= EU_{\text{Block}} \\
 4\mu - 1 &= -3\mu + 1 \\
 7\mu &= 2 \\
 \mu &= 0.286
 \end{aligned}$$

This means substantially that the follower is accepting justifications until there is only a 28.6 percent chance that the leader is a true prophet.

In Figure 4, we see that under our baseline model with  $(J|tp) = 0.8$  and  $p(J|f) = 0.99$ , the follower will block at the fifth failure of prophecy (red line). If the follower assumes that a true prophet would justify less often, she will block already at the third failure of prophecy (green line). On the other hand, if she assumes that a true prophet would justify more often, she will block only at the 10th failure (blue line).

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Figure 4: Repeated Bayesian Signaling Improvisational Game Between a Prophet and a Follower



Note: Every round consists of a failed prophecy and a justification by the Religious Leader. The dotted line represents the threshold below which the Follower will block.

Can this model make sense of what happened to the Brotherhood? If the assumptions of the model are feasible, it explains why the Brotherhood survived several significant failures of prophecies (around 5-6) but then disintegrated. The model explains this by the fact that the members reached the threshold value.



Our model can also explain why the Brotherhood collapsed earlier in East Lansing than in Oak Park, Illinois.<sup>19</sup> This is because the leaders in Oak Park, Illinois (Dorothy Martin and Charles Laughead) remained present and justified the failures. They showed their willingness to continue with the group and its beliefs. Accordingly, the Oak Park group could still survive for some days longer. In East Lansing, however, no leaders were present, and members were left alone with their knowledge that all predictions of the group had failed (Charles Laughead had joined Dorothy Martin in Oak Park, Illinois). This can be expressed in our model as a situation in which the leaders block, leading the followers to also block.

## OTHER EXAMPLES IN THE HISTORY OF RELIGION

One way of evaluating the strength of a model is trying to find other examples of the proposed mechanism. Do other and historically more important religious groups behave in similar ways? In my view, the answer is yes, and I only very briefly mention four examples.

The *movement of Jesus and early Christianity* provides an excellent example of the use of AIJR techniques. The historical Jesus was an itinerant Jewish preacher who likely claimed divinity and unequivocally announced the imminent end of the world (Theissen 2001). His crucifixion by the Romans presented his followers with a significant challenge—a failed prophecy. While various justifications and explanations emerged, the interpretation that ultimately prevailed was that God had willed his own Son, Jesus, to die on the cross as a sacrifice for the sins of humanity (Bermejo-Rubio 2017; Lüdemann 2002). According to this explanation, Jesus had risen from the dead, appeared to his followers, and ascended to heaven to reunite with the Father. Believers could share in this miraculous transformation by being baptised into Jesus Christ and thus lead a life in righteousness and holiness. To construct this narrative, Paul drew on established Jewish traditions of atonement through sacrifice. For instance, in Romans 4:25, he alluded to Isaiah 53:4-5: “Surely he has borne our griefs and carried our sorrows; yet we esteemed him stricken, smitten by God, and afflicted. But he was pierced for our transgressions; he was crushed for our iniquities; upon him was the chastisement that brought us peace, and with his wounds we are healed.” Through this reinterpretation, Jesus’s crucifixion was both justified and presented as a fulfilment of Isaiah’s prophecy. The story of the Son of God who died on the cross for humanity’s sins became such a compelling explanation that it became the cornerstone of

19 Already Festinger et al. had remarked on this (p. 70).

a new religion – Christianity. This faith welcomed both Jews and Gentiles, offering a universal message of redemption and salvation.

The *Jehovah's Witnesses* provide a compelling example of the application of the AIJR technique. One of the most well-known cases involves their belief concerning the year 1914 (Beckford 1975; Chryssides 2010). According to their founder, Charles T. Russell, this year marked the end of the “time of the Gentiles” and the establishment of God’s kingdom on earth (Russell 1989 [1889]). However, when 1914 passed without visible fulfilment of these expectations, the movement faced a significant challenge. Joseph F. Rutherford, Russell’s successor, reinterpreted the prophecy, asserting that God’s kingdom had indeed been established—but invisibly (Rutherford 1933). He explained that Jesus had appeared in a new form in heaven during this year. Furthermore, dramatic worldly events such as the outbreak of World War I were framed as additional signs affirming this interpretation. Since then, 1914 has become a cornerstone of the Jehovah’s Witnesses’ explanation of world history and God’s eschatological plan, signifying the year when Jesus triumphantly began his heavenly reign in a new form.

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In Islam, the so-called “satanic verses” can be seen as an example of the AIJR technique (Cook 2000; Paret 1972). According to the accounts of al-Wāqidi, Ibn Sa’d, and al-Tabarī, there was an incident in which Muhammad recited verses acknowledging the three goddesses al-Lat, al-Uzza, and Manat as legitimate deities. These accounts claim that the verses of Surah 53:19–20—“Have you thought upon al-Lat and al-Uzza, and Manat, the third, the other?”—were originally followed by: “These are the exalted cranes (intermediaries) whose intercession is to be hoped for.” This addition, however, directly contradicted the strict monotheism central to Muhammad’s message. According to these biographers, Muhammad later retracted the verses, asserting that they had been a “satanic suggestion.” If historical, this incident would represent a clear example of AIJR. The apparent momentary acceptance of polytheism—likely an attempt to ease tensions with powerful Meccan leaders—was retrospectively reframed as a grave error attributed to satanic interference. This justification was further supported by referencing an existing element of the religious worldview, namely the devil’s capacity to mislead prophets, as mentioned in Surah 22:52: “And We did not send before you any messenger or prophet except that when he spoke (or recited), Satan threw into it (some misunderstanding).”<sup>20</sup>

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20 The later Muslim tradition has mostly rejected the possibility that Muhammad ever made such satanically informed claims, on the grounds that Muhammad was perfect and therefore could not possibly have made a mistake.

A final example can be drawn from Scientology. L. Ron Hubbard's method of self-development, originally called "Dianetics" and later evolved into "Scientology," was never accepted by scientifically trained psychiatrists (Miller 1987; Wright 2013). Early in the development of Dianetics, one of Hubbard's collaborators, medical doctor Joseph Winter, submitted papers on Dianetics to the journals of the American Medical Association and the American Psychiatric Association. However, these papers were rejected due to "a lack of clinical experimentation, or indeed of any substantiation" (Atack 1990, p. 106). This marked the beginning of a series of categorical rejections by psychiatric establishments in various countries (Atack 1990). Hubbard responded to these setbacks with an AIJR technique. He justified the rejection by claiming a global conspiracy of psychiatrists to subjugate humanity. He claimed that psychiatrists sought to "harm, injure, and kill patients without restraint" (cited in Atack 1990, p. 261). According to Hubbard, their sinister motives explained their unwillingness to accept his method, which promised genuine progress for millions of individuals. Consequently, he urged Scientologists to expose the abuses and crimes of psychologists and psychiatrists. This mission led to the creation of the Citizens Commission on Human Rights, an organization "dedicated to eradicating psychiatric abuses and ensuring patient protections."<sup>21</sup>

Note that in all these examples, the justifications devised to reinterpret the enigmatic elements have themselves become integral parts of the respective religious ideologies. The expiatory death of Jesus, the possibility of satanic intervention in the Qur'an, the invisible beginning of God's kingdom in 1914, and the global conspiracy of psychiatrists—all of these have, to varying degrees, become central components of their respective religious systems. These elements emerged and solidified through acts of religious improvisation. On a cautionary note, all these examples may well be interpreted with the AIJR model in mind, but since our historical data are much weaker than in the Brotherhood case, it is much harder to *prove*, that the model applies.

## CONCLUSION

I set out to explore the social mechanisms underlying the emergence of a small religion as described in Festinger et al. (2008 [1956]) and to explain the following astounding facts associated with the case. First, the new religion emerged spontaneously within only six months. Second, the messages purportedly received from extraterrestrials were vague, yet the resulting

<sup>21</sup> See the official website of Scientology, <https://www.scientology.org/how-we-help/citizens-commission-on-human-rights/#slide9>, accessed on December 15, 2024.

religion was relatively coherent and structured. Third, despite multiple failed prophecies, the group did not collapse but instead experienced an evolution in its ideology.

To address this question, I proposed a generative model inspired by Raymond Boudon, conceptualised as an improvisational game designed to produce these astounding facts. The central premise is that the group employed techniques akin to those used by theatre improvisers, albeit in a latent and asymmetric fashion. Unlike theatre improvisers, who are fully aware they are constructing a reality, the Brotherhood believed they were uncovering an already existing reality. Furthermore, while theatre improvisers operate on equal footing, the Brotherhood's process was shaped by authority and power dynamics, granting leaders greater influence over the improvisational process than followers.

242 I have on the one hand presented the model and, on the other, provided numerous examples to demonstrate its empirical applicability to the Brotherhood. *How could the new religion emerge without planning?* This becomes possible with AIJR techniques. The group first accepts all previously improvised elements as valid. Second, it freely improvises new elements without fear of future contradictions. Third, it justifies enigmatic elements by connecting them to earlier material. Fourth, it reuses salient elements, leading to the emergence of a coherent religious ideology and group structure centred around these focal points. In this way, much like theatre improvisers, the group collectively creates a new cultural world, even though no individual can fully control its evolution.

*How could the new religion emerge so quickly?* The speed of emergence is explained by the AIJR principle of “not blocking” (i.e., “accepting”). The group is prohibited from denying the assumed reality of improvised elements. With this rule in place and a continuous stream of new contributions, a new cultural world can form rapidly.

*How could the religion become so coherent despite the often unclear and messy nature of the messages received?* The *reuse mechanism* is especially critical here: many improvised elements are forgotten in the long run, while a select few salient elements become central to the narrative. These focal points provide the framework for the emerging religious world.

Why does the group not collapse with each new failed prophecy? The Brotherhood's history is filled with such failures: extraterrestrials fail to appear, UFOs do not land, catastrophes do not occur, and messages often contradict one another. According to our AIJR model, the group remains intact despite these disappointments because it can generate one or more justifications for any failed prophecy. These successful justifications are then incorporated into the group's evolving religious culture. In this way, failed prophecies do not

necessarily remain failures; over time, they may even become central symbols of the group's belief system, much like the crucifixion of Jesus in Christianity. However, the success of these justifications often depends on the leaders' ability to enforce their acceptance. If the group remains attractive to its followers, leaders can pressure members to accept even improbable explanations in exchange for continued membership and belonging. If, on the other hand, followers' belief that the prophet is a true prophet drop below a certain threshold, the group may disintegrate.

I do not mean to suggest that religions evolve solely through improvisational mechanisms. Religions can evolve in many ways, and the mechanisms discussed here represent only one possible pathway of social evolution. For example, religious change can occur through the action of a powerful figure, such as a king or prophet, or through collective decisions, as when a synod resolves a theological question (compare to Esser 2000). Nevertheless, AIJR may apply in more instances than one may expect. Faced with new turns of events, powerful religious leaders often must adapt their ideology to changes of society. It is then often useful to justify new elements in an AIJR manner, by linking them to some older element and presenting them as somehow implied or predicted by that former element. As David Voas comments:

It's not only prophecy that can fail: doctrine can also fail. Democracy replaces divinely anointed kings. Slavery is abolished. Contraception becomes almost universal, women become managers, and same-sex relationships come to be accepted. Churches that claimed that God condemns something have to concede that God supports it. The task is now much harder than back in the more spontaneous period of AIJR, but it amounts to introducing a sharp narrative turn while still arguing that it's all part of the same story.<sup>22</sup>

The model presented here suggests that religious groups may emerge and evolve in unplanned ways, following a process of "social evolution." This raises the question of how our model relates to evolutionary models in biology. It seems that the three mechanisms identified in AIJR parallel the core mechanisms of biological evolution. The improvisational creation of new elements introduces *variation*. The reuse of some elements while discarding others resembles *selection*. Finally, the processes of acceptance and justification can be compared to *inheritance*. Thus, AIJR may be understood as functioning in a manner somewhat analogous to biological evolution—albeit at a much faster pace and involving agents with consciousness and intentionality.

22 Personal communication by David Voas from January 17, 2025.

Do our findings have implications for the theologies of different religious groups? In my view, AIJR explanations of a given religious message do not determine its theological “truth.” The message of the resurrected Christ may hold truth for a Christian regardless of how it was created, just as the interpretation of the “Satanic verses” may hold truth for a Muslim. Here, “truth” is understood not in the scientific sense but as something akin to “meaning-making value.” Nevertheless, if accepted, AIJR explanations are likely to have theological consequences, as they may rule out certain modes of argumentation – particularly those associated with fundamentalist approaches.<sup>23</sup>

This article, of course, has its limitations. First, I have focused on a single, very small religious group. While I have suggested that the AIJR mechanisms may be applicable to other religious groups, these examples have been necessarily brief and illustrative. The extent to which the proposed mechanisms can be generalised remains an open question. Second, the relationship between social game theory and economic game theory requires further clarification. Third, when analysing the case, I have relied on the written-up record of Festinger et al., not on the primary data. Fourth, the game theoretical model presented is work in progress. While this model seems to capture some of the points that interest us, it also has its drawbacks. Are the assumptions, especially the assumed starting values, reasonable? I suspect that there must be better ways than what is proposed here to model this and other cases of religious improvisation.

In future studies, the AIJR model should be applied more widely, and better game-theoretic models should be devised. When this will happen, I predict, the world as we know it will come to an end, and a bright new era of research will begin.

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23 I thank Lukas Spinner for interesting discussions on this point.



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PART V

**METHODOLOGICAL  
INDIVIDUALISM  
AND RATIONALITY**



## METHODOLOGICAL INDIVIDUALISM: KEY INSIGHTS FROM BOUDON AND A CRITICAL DISCUSSION

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### INTRODUCTION: METHODOLOGICAL INDIVIDUALISM AS A PARADIGM FOR MACROSOCIOLOGICAL RESEARCH

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PART V Methodological Individualism and Rationality

In his autobiographical reflections, Boudon (Boudon and Leroux 2003) traces his recognition of the importance of referring to the individual actions of social actors in explaining macrosociological phenomena back to a published work in the field of judicial sociology. The study aims to understand the upward trend in decisions to discontinue prosecution, alongside the increase in the number of offenses since the beginning of the nineteenth century (Boudon and Davidovitch 1964). This analysis, centered on statistics relating to individual decisions, called for interpreting them not as the mechanical consequences of macrological changes, but as the results of social mechanisms involving “the subjectivity of the magistrate, who undertakes the translation of facts into terms of law.” In a (secondary) dissertation<sup>1</sup> under the direction of Raymond Aron, *À quoi sert la notion de structure ? (The Uses of Structuralism)*, Boudon (1968) discusses the prevailing tendency among representatives of structuralism, then in vogue, to ascribe a form of metaphysical reality on the structures studied. He argues that they should be used only for what they truly are: means of identifying a set of interdependent characteristics. The significance of his methodological defense of the individualist approach is well known, although he does not explicitly refer to it as such in the context of sociology until 1979. Multiple examples of this defense appear in his subsequent publications, including: *L’Inégalité des chances (Education,*

<sup>1</sup> Boudon defended his doctoral dissertation in 1967 on *L’Analyse mathématique des faits sociaux (The Mathematical Analysis of Social Facts)*, prepared under the supervision of Jean Stoetzel.

*Opportunity and Social Inequality*) in 1973; *Effets pervers et Ordre social (The Unintended Consequences of Social Action)* in 1977; and *La Logique du social (The Logic of Social Action)* in 1979, among others.

In a chapter entitled “The Individualistic Tradition in Sociology”, part of a collective work *The Micro-Macro Link*, which compares the continental and Anglo-Saxon sociological traditions in terms of the relationship between the macrological and micrological levels of social analysis, Boudon (1987) contrasts the scientific aims of methodological individualism (MI) with those of three other traditional paradigms of macrosociological research: “observe” (the nomological paradigm which seeks macrosocial laws: If A, then B); “interpret” (the interpretive paradigm which aims to identify general social forms); or “criticize” (the critical paradigm which seeks to change society). The aim of MI, on the other hand, is to “explain” any social phenomenon – whether a regularity, singularity, or societal difference – by uncovering the individual actions that give rise to it. With this explanatory ambition, MI represents the central paradigm of macrosociological research in the social sciences. Its methodological dimension is based on three conditions: First, actions, in the Weberian sense, are bearers of meaning and, consequently, of motives; second they are ideal-typical, since their relationship to real actions takes the form of a stylized, abstract model; and, finally, individuals are social actors, and are therefore inherently embedded in social relationships:

Suppose M is the phenomenon to be explained. In the individualistic paradigm, to explain M means making it the outcome of a set of actions m. In mathematical symbols,  $M=M(m)$ ; in words, M is a function of the actions m. Then, the actions are made understandable, in the Weberian sense, by relating them to the social environment, the situation S, of the actors:  $m=m(S)$ . Finally, the situation itself has to be explained as the outcome of some macrosociological variables, or at least of variables located at a level higher than S. Let us call these higher-level variables P, so that  $S=S(P)$ . On the whole,  $M = M\{m[S(P)]\}$ . In words, M is the outcome of actions, which are the outcome of the social environment of the actors, the latter being the outcome of macrosociological variables (Boudon 1987, p. 46).<sup>2</sup>

The equation  $m=m(S)$ , mentioned above, expresses the ideal-typical relationship between actions and individual situations. This relationship, to

2 The equation, as stated by Morin (2023, p. 236) with reference to Boudon, is an effective alternative:  $S=f[a(r, C)]$ : “Each social phenomenon S is considered the collective effect f of actions a, which are driven by reasons r, within context C.”

which Boudon refers in all his works, from *L'Inégalité des chances* (*Education, Opportunity and Social Inequality*) to the posthumous *Le Rouet de Montaigne* (*Montaigne's Spinning Wheel*), via *L'Idéologie* (*The Analysis of Ideology*), *L'Art de se persuader* (*The Art of Self-Persuasion*), *Le Sens des valeurs* (*The Origin of Values*), invites us to adopt the perspective of the abstractly modeled actor and, aside from a-rational cases, to give full scope to the social actors' reasons for action. Within the framework of MI, the actors' relationship to their situation thus rests on two postulates which, as Boudon points out, are largely coextensive: the postulate of understanding and the postulate of rationality. In this regard, Boudon frequently emphasizes the organic links between Weber's or Simmel's interpretive sociology and MI.

This understanding approach (*Verstehen*), associated with the uncovering of the reasons behind the actions of social actors, stems from the social scientist's specific knowledge of their modes of action. It assumes that we can adopt the point of view of individuals and thus understand the cause of their action (understood in the Weberian sense as meaningful and oriented toward others), provided we adequately identify both the subjectively perceived external factors and the internal means of interpretation available to them. In this regard, Boudon emphasizes in various texts the role of the neo-Kantian epistemology shared by Max Weber and Georg Simmel, which involves considering the socially acquired meaning structures of individuals in order to understand their interpretive relationship to their situation (see Bulle and Morin 2024). These meaning structures help explain the motives or reasons for action, both personal and impersonal, of social actors that determine their behavior. This situation, therefore, involves both internalized structures (knowledge, beliefs, normative and conceptual systems, etc.) and external relational structures (patterns of interconnections or interdependence). The understanding perspective thus assumes that the influence of structures on action is essentially indirect, mediated by the interpretive activity of individuals. It relies on an abstract psychology that involves selecting the relevant elements from ideal-typical individual situations. Furthermore, this abstract psychology incorporates what Boudon refers to in his 1987 article as "context-bound rationality" (echoing Herbert Simon's "bounded rationality"), in contrast to universalizing conceptions of rationality. The associated principle of rationality does not pertain to the normative and often instrumental forms of rationality employed in economic models. Instead, as Boudon (1987, p. 63) writes, it assigns "a much broader meaning to this notion," a meaning that he identifies as "cognitive" in subsequent texts. Cognitive rationality assumes that the social actor chooses not only between means and ends, but also (implicitly) between different interpretations of problems, relying on beliefs or values to

address issues that cannot be resolved through purely logical or consequentialist reasoning. However, in all cases, the relationship to reality is that of a highly simplified and abstract theoretical model, which does not necessarily imply full awareness of the reasons for action on the part of social actors.

254 The individualist paradigm thus outlined is central to macrosociology, and applies to all levels of analysis – groups, organizations, societies – given the simplifications that can be made in theoretical models and explanations. On this basis, MI does not propose a general theory but focuses on uncovering the social mechanisms underlying observable phenomena. These generally present themselves as enigmas, whether historical and specific, behavioral and general, or empirical and social: “Why the French farming system was still underdeveloped when the British became modern? Why do members of a latent (i.e., unorganized) group tend to defect?”; or “Why do the expansion and democratization of education systems in advanced industrial societies not ipso facto have a noticeable effect on social mobility?”<sup>3</sup> Social phenomena particularly require sociological analysis when they represent the unintended effects of individual actions. Neglecting individual motives and focusing more on notions of collective structures and forces, on the other hand, tends to imply a form of congruence between macrological cause and effect, which assumes that individual actions are directly influenced by supra-individual structures. The individualistic method thus allows us to deepen explanations by identifying more explicit or authentic causal mechanisms. In response to the questions mentioned above, Boudon explains that in France, due to administrative centralization and the attractiveness of public offices, landlords tended to purchase these offices and abandon the direct management of their land, rather than increase agricultural productivity (Tocqueville 1952 [1856]). Moreover, Mancur Olson’s (1965) theory of collective action helps us understand the subjective situation of members of a latent group who desire the results of collective action but are unwilling to bear the costs individually. Regarding inequality of educational opportunity and social mobility, in contrast to theories that directly link social inequality and educational inequality through cultural inequality, Boudon (1973) proposed a model that illustrates how individuals’ educational choices are shaped by subjectively perceived opportunity structures. Their perceptions depend on their educational achievement and social origin, with inequality exacerbated by the cumulative effects of the choices they make throughout the schooling process. Boudon’s model also demonstrates that structural school

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3 For an overview of typical examples of MI explanations developed by Boudon in his various works, see Boudon (2023).



reforms, such as expanding access to educational levels, can mechanically reduce inequality of educational opportunity but have no significant effect on inequality of social opportunity unless accompanied by concomitant changes in the social structures.<sup>4</sup>

## DEMARCATON OF MI:

### A PROBLEMATIC SHIFT IN BOUDON'S CONCEPTION

According to the above, Boudon provides clear criteria for characterizing MI, which recur systematically in his texts until the early 2000s – that is, for nearly twenty-five years, during which this theme was omnipresent in his writings. These criteria include: the individualism of the explanatory model; the understanding that links the observer to the actor; and the rationality of the actor in the broadest sense, which he prefers to identify as “cognitive” rather than limited.<sup>5</sup> As part of a critique of the standard version of rational choice theory – which employs instrumentalist, egoistic consequentialism and utility-optimizing principles, Boudon (2002) differentiates and hierarchizes these three postulates to define MI: the P<sub>1</sub> postulate of individualism (“all social phenomena result from the combination of individual actions, beliefs or attitudes” – which I will refer to here as the postulate of causal individualism),<sup>6</sup> the P<sub>2</sub> postulate of understanding, and the P<sub>3</sub> postulate of (cognitive) rationality. The hierarchy of postulates follows a progression from the most open to the most closed conditions, with the most closed logically implying verification of the most open conditions. Indeed, on the one hand, rationality in the broadest sense implies understanding, with understanding including certain additional, “a-rational” cases.<sup>7</sup> On the other hand, both rationality and

- 4 See Bulle (2009) for an analysis which highlights the evolution of the intrinsic structure of educational opportunities in Boudon's model and Bulle (2016, 2019) for the design and implementation (applied to the French context) of a measure of intrinsic educational opportunities (“inequality within the selection process”).
- 5 See, for example, Boudon 1984, p.66; Boudon 1987, p.55; Boudon 1991, p.118; Boudon 1995, pp.253-255; Boudon 2002, p.9; Boudon and Fillieule 2002, p.25; and Morin (2024) for an overview.
- 6 Causal individualism can be defined as a methodological approach that involves analyzing a whole – here conceived as social – into units endowed with causal properties.
- 7 The possibility of a-rational but not “irrational” motives – understandable essentially through empathy – justifies the distinction between the postulates of understanding and rationality: “I regularly close my eyes without realizing it. This action responds to the needs of my organism; it is not the product of reasons formed in my mind. I am unable to pronounce a particular English word correctly: this is because my vocal cords have not been accustomed in good time to producing the

understanding imply reference to individual actions or behaviors. However, 2003 marks a shift in Boudon's presentation of MI. From that year onwards, MI is no longer characterized by the postulates P<sub>1</sub>-P<sub>3</sub>, but is instead limited to the single postulate P<sub>1</sub> of individualism. P<sub>1</sub> is then presented less as a "postulate" and more as a self-evident principle (Boudon and Leroux 2003; Boudon 2003b, 2006). The approaches defined by postulates P<sub>1</sub>-P<sub>3</sub>, previously characteristic of MI, are now distinguished from MI in the strict sense. In his 2003 texts, they are described variously as a very general variant of MI (Boudon and Leroux 2003), as effective sociological theories (Boudon 2003b), or as the paradigm that Boudon (2003a) calls "the cognitivist theory of action." In 2006, postulates P<sub>1</sub>-P<sub>3</sub> define valid explanatory approaches (Boudon 2006); in 2007, they represent a version of interpretive sociology (Boudon 2007); and in 2010, they refer to the paradigm envisioned by Boudon (Boudon 2010).

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It should also be noted that by identifying MI with P<sub>1</sub> in the 2006 and 2007 texts, Boudon links it to conceptions supposedly shared by Weber and Schumpeter – something that had not been the case previously. He had always believed that Schumpeter had carried out vacations for Weber and had likely introduced the term MI at Weber's suggestion. However, I have found no evidence of a connection between Weber and Schumpeter prior to 1910 (Swedberg 1991, p. 92). This anecdote, which Boudon believed, allowed him to attribute the very authorship of the concept to Weber. In fact, Schumpeter did not coin the expression, which appeared as early as 1904.<sup>8</sup> The key point is that, reduced to P<sub>1</sub>, MI becomes closer to Schumpeter in Boudon's view, making it easier for him to associate the economist with Weber to represent MI in this new, strict sense, now defined solely by postulate P<sub>1</sub>. Together, postulates P<sub>1</sub> and P<sub>2</sub> are said to define interpretive sociology in Weber's sense. What truly matters, however, are the postulates P<sub>1</sub>-P<sub>3</sub>, which now represent a version of Weberian interpretive sociology, specifically, the version championed by Boudon himself.

However, the distinction between three versions of Weber's methodological conceptions is artificial. Boudon derives the MI version (P<sub>1</sub>) from Weber's famous letter to the marginalist economist Robert Liefmann: "sociology, too, can only be pursued by taking as its point of departure the actions of one, or

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phonemes it includes. I'm disgusted by a dish that the Japanese consider a delicacy: This is because I haven't acquired in time the *habitus corporis* evoked by medieval Aristotelianism" (Boudon 2003b, p. 20).

8 The expression "individualist method" was used as early as the nineteenth century in the context of the *Methodenstreit* between Carl Menger and the German Historical School. The term MI can be found in a 1904 text by the French philosopher and historian Élie Halévy (see Halévy 1904, Borlandi 2020).

more (few or many) individuals, that is to say, with a strictly ‘individualistic’ method” (Weber 2012 [1920]). However, this reference to individual actions in Weber’s view inherently implies the postulates of understanding and, correlatively, rationality, since human behavior is called “action” “if and insofar as the acting individual or individuals attach a subjective meaning to it” (Weber 2024 [1922], § 1). Schumpeter himself probably did not equate MI with P1. When he wrote “when we describe certain economic processes, we must base them on the actions of individuals”, he was referring to actions endowed with intentionality, as represented in particular by the models of neoclassical economists (i.e., P1-P3 along with postulates used for modeling, which would later define the standard version of rational choice theory). Finally, Weber (2024 [1922], p. 79) defines sociology as he sees it as “a science that aims to understand social action interpretively and thus to explain its course and effects causally,” without distinguishing between an essentially interpretive version and one incorporating the principle of rationality. This is because, except in a few borderline cases, reference to the subjective meaning of action inherently involves the P3 principle of rationality in the broadest sense, applied through an ideal-typical approach. As a result, the P1-P2 definition of Weberian interpretive sociology does not fully make sense either.

Reducing MI to postulate P1 alone raises several other significant problems. First, Boudon presents P1 as a truism, which tends to deprive it of substantive content, especially since he is quick to add that effective or explanatory theories are also based on postulates P2 and P3. Consequently, MI, when reduced to P1, loses its particular methodological significance. Second, the rejection of P1, equated with the rejection of MI in the strict sense, is supposed to characterize holism (Boudon 2003b), which also trivializes the methodological problems of holism.<sup>9</sup> Third, reducing MI to P1 expands the scope of MI explanations to include individual behaviors resulting from processes that are not only unconscious – processes that P2-P3 do not reject as long as they can be linked to internalized subjective meanings – but also processes that cannot be meaningfully interpreted in this regard. This is methodologically problematic. As Popper (1994) noted, it is generally more fruitful to revise our conception of individual situations than to question the principle of rationality, and this is even more true in the case of the principle of understanding. Moreover, in explanations that retain P1 but reject P2 and P3, once individuals are deprived of subjectivity in the sense of P2 and P3, even if they are still seen as the causes of action, they become more susceptible

9 This is nevertheless consistent with Jon Elster’s approach, for whom MI “is trivially true” but who tends to emphasize the subtleties of methodological holism.

to the direct influence of environmental factors, including those encapsulated by collective concepts. In such cases, the previously established oppositions between MI and methodological holism would no longer apply. Indeed, some of the historicist theories against which MI was historically constituted<sup>10</sup> can now fall under MI when the latter is reduced to P1. MI approaches can now also include functionalist theories that rely on individual action but relate it to equilibria determined at a supra-individual level, or the sociology of Pierre Bourdieu (see e.g. Bourdieu 1985), who sought to “break out of structuralist objectivism” by reintroducing individual agency with the notion of habitus (a system of enduring, structured, structuring dispositions). This scope extends even further, as MI reduced to P1 should logically encompass approaches from depth psychology, thereby extending MI to any framework, albeit without any specific methodological focus.<sup>11</sup> However, Boudon consistently distances MI from any psychological hypothesis that portrays individuals as mere playthings of unconscious cognitive processes associated with their group membership. He cites, as examples, the psychological interpretations of Gustave Le Bon and Lucien Lévy-Bruhl (Boudon 1995). John Stuart Mill can also be included as Mill argues that, based on associationist psychology and the supposed effects of interaction with the environment, individuals adopt collective behaviors that form the basis of major sociological laws.<sup>12</sup>

Finally, the principle of rationality (in the broadest sense) is constitutive of MI in the methodological work of its founders (Carl Menger, Georg Simmel,

<sup>10</sup> See for instance Bulle (2024) on this subject.

<sup>11</sup> This is depth psychology, not just the unconscious, which only becomes problematic when it is presumed to conflict with conscious meaning. Weber considered certain exceptions to the principle of rationality, and concluded that they should simply be regarded as non-meaningful facts: “It is possible that future research will also discover uninterpretable regularities in certain meaningful behaviors, as little as has been the case so far [...] Acknowledging their causal significance would not change in the least the task of sociology (and the action sciences in general), which is to understand meaning-oriented action through interpretation. It would merely introduce, at certain points within the comprehensibly interpretable motivational contexts, non-meaningful facts of the same order as others already mentioned above” (Weber 2024 [1922], p.90).

<sup>12</sup> Popper (1966 [1945], p.303) acknowledges that Mill seems to share a key idea with MI – namely, that the actions of collectives must be explained by the actions of the individuals who comprise them. However, this does not make Mill a representative of MI, as his psychologism, since Popper points out, forces him to adopt a historicist method in which the social environment exerts a dominant influence. This leads Mill to invoke the notion of the “spirit of the people,” a concept used by certain historicist approaches to explain individual behavior: “Yet to whomsoever well considers the matter, it must appear that the laws of national (or collective) character are by far the most important class of sociological laws” (Mill 1843, ch. 9, § 4).

and Max Weber) and early proponents (Ludwig von Mises, Friedrich Hayek, and Karl Popper). Therefore, its reduction to P<sub>1</sub> is also problematic from the perspective of the historical emergence of MI.<sup>13</sup> Nevertheless it should be noted that MI is sometimes interpreted in a broad, minimalist, sense, as opposing the misuse of collective concepts, but without imposing any particular constraint in terms of rationality (see Bouvier 2011; Elster 2023). This perspective is also adopted in recent approaches in analytic sociology (see on this subject Bulle and Phan 2017; Bulle 2023a; Di Iorio and Chen 2019; Di Iorio 2023a, 2024; Manzo 2023; Opp 2024). In my view, and for the reasons outlined above, these interpretations overlook MI's logical commitment to the three postulates P<sub>1</sub>-P<sub>3</sub>.

## UNDERSTANDING BOUDON'S SHIFT

How can we explain this major shift in Boudon's conception of the scope of MI, which raises multiple problems, including the continuity of his views on the subject? To answer this question, we must consider the criticisms of MI within the scientific community since its popularization in the 1950s. These critiques have tended to interpret it first through the neopositivist lens of the dominant epistemology of the time, and later through the physicalist perspective of the analytic philosophy that succeeded it. Both lenses tend to reject subjectivism and, correlatively, to embrace the reductionist problematics widely debated under their influence. In this intellectual context, MI has tended to be interpreted as a reductionist approach that advocates a focus on individuals to the exclusion of structures (see Bouvier 2023; Bulle 2023b, 2025; Di Iorio 2023b). In this respect, Boudon (1995, p. 253) observes that MI is often misunderstood, and Boudon (1999, p. 375) describes MI without naming it. Against this unfavorable backdrop for MI in some academic circles, the Swedish sociologist Lars Udehn (2001, 2002) published a comprehensive work on the intellectual history of MI in 2001 and an article "The Changing Face of Methodological Individualism" in *Annual Review of Sociology* in 2002. Udehn had devoted his 1987 dissertation to MI, and his 2001 book represents a substantially revised and less critical version, reflecting the developments he observed (Udehn 2001, p. 24). In these texts, the sociologist adopts an integrative perspective, grouping under the banner of MI all approaches that can be linked to the postulate P<sub>1</sub>, decoupled from questions of understanding and rationality. Udehn argues that the approaches associated with P<sub>1</sub> represent multiple, more or less coherent versions of MI. These approaches,

<sup>13</sup> See Bulle (2025).

which essentially refer to individual behavior, questionably include classical economics, Mill's psychologism, and social contract theories. Moreover, Udehn tends to identify reductionist ideas in Menger, the acknowledged founder of MI in economics, and in Weber, the acknowledged founder of MI in sociology. For instance, Udehn (2001, p. 166) highlights Menger's reference to Robinson Crusoe as a method of analyzing the variable value of goods based on their utility for survival. However, in a text by Hayek on this subject cited by Udehn, Hayek explicitly emphasizes the intrinsic link between Menger's MI, the method of understanding, and the principle of rationality, that is, the postulates P<sub>2</sub> and P<sub>3</sub> as defined by Boudon which, by referring to the interpretive activity of individuals, protect against reductionism:

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The consistent use of the intelligible conduct of the individuals as the building stones from which to construct models of complex market structures is of course the essence of the method that Menger himself described as 'atomistic'<sup>14</sup> (or occasionally, in manuscript notes, as 'compositive') and that later came to be known as 'methodological individualism' [...] Unlike the physical sciences which analyse the directly observed phenomena into hypothetical elements, in the social sciences, we start with our acquaintance with the elements and use them to build models of possible configurations of the complex structures into which they can combine and which are not in the same manner accessible to direct observation as are the elements. This raises a number of important issues, on the most difficult of which I can touch only briefly. Menger believes that in observing the actions of other persons we are assisted by a capacity of understanding the meaning of such actions in a manner in which we cannot understand physical events (Hayek 1978, pp. 276-277).

Similarly, Udehn (2001, p. 191) argues that for Weber, sociology is "a science of individuals and their actions, not of society," so that society exists for him, "neither as an entity, nor as a 'level of reality'". However, for Weber, this is a methodological claim, rather than an ontological one: Any science, in his view, is defined by the perspective from which it seeks to apprehend reality, not by an ontology (see Feuerhahn 2023). This also explains why Weber argues that psychology is not a foundational science for the social sciences, because society and social actors are not considered from an ontological standpoint, but as

14 It should be noted that Menger's atomism does not refer to the atomism of the British empiricists, which focuses on the analysis of sensible impressions, but rather to the decomposition of a whole into basic units – specifically, the P<sub>1</sub> postulate of MI.

relative theoretical concepts. Moreover, while Weber, for reasons that need not be elaborated, tended to avoid collective concepts and to favor their nominalist interpretation, he developed numerous references to various types of society as such, including “traditional”, “feudal”, “commercial”, “communistic”, “mixed”, etc.

In the broad perspective he has developed, Udehn characterizes social science approaches associated with P1 based on their positioning along a scale of reductionism, indexed to the exogenous role played by social structures. He thus observes a progression from a strong, original form of MI that increasingly incorporates structures. Finally, he describes the approaches of “leading sociologists such as James Coleman and Raymond Boudon” as “best characterized as structural individualism” (Udehn 2002, p. 496) because of the importance they attach to social structures.

A few points about reduction need to be clarified here. The reference to higher levels of complexity, such as structures, does not, in itself, distinguish a non-reductionist approach from a reductionist one. Causal individualism, as associated with P1, is reductionist if, and only if, the causal properties of the basic units involved are independent of the wholes, allowing theories about wholes to be, in principle, reducible to theories about those units (their parts).<sup>15</sup> The exogenous variables essentially refer to the boundary conditions of models, meaning that reduction does not imply their absence. However, reduction logically invites regression *ad infinitum*, to a point of origin. As noted by Udehn (2002, p. 501):

It is often argued, for instance, that it is impossible to endogenize all social institutions, since the attempt to do so leads to an infinite regress [...] If this argument is correct, strong methodological individualism is not a viable position, even if ontological individualism is self-evidently true, as most methodological individualists seem to believe.

This logical regression *ad infinitum* suggested by the reductionist approach is referred to by Popper (1966 [1945], pp. 304-305) in his critique of Mill’s psychologism:

<sup>15</sup> Intertheoretical reduction was originally defined by advocates of, or influenced by, logical empiricism (see, in particular, Oppenheim and Putnam 1958; Nagel 1961). It is reworked here in a post-positivist version consistent with earlier definitions, which specifically imply the possibility of translating the laws of the reduced theory in terms of the laws of the reducing theory.



It is a desperate position because this theory of a pre-social human nature which explains the foundation of society – a psychologistic version of the “social contract” – is not only an historical myth, but also, as it was, a methodological myth.

In any case, the presence of structural variables as exogenous variables in the models does not always imply ipso facto the interdependence of the causal properties of individuals. The degree of reduction based on this presence provides, at best, an imprecise perspective on the reductionist implications of social science approaches. In MI, it is principles P2 and P3 that bring the inherently social nature of individual actions by referring to their interpretive properties.

Udehn’s approach motivated Boudon’s shift. Boudon even notes: “Udehn (2001) provides a useful survey of IM variants, but he seems not to recognize the logical importance of the psychological question that the social sciences must adopt” (Boudon 2003b, p. 66). However, one might ask, why, despite the challenges posed by abandoning the role of postulates P2 and P3 in defining MI, Boudon changed his presentation of the paradigm, apparently after reading the Swedish sociologist.

To answer this question, it is necessary to try to examine Boudon’s interpretation of the situation. When he worked with Davidovitch in 1962–1963 and began to explore the idea of an individualist approach, he was unfamiliar with the term “MI” (Boudon and Leroux 2003, p. 50). He did not use it in *L’Inégalité des chances (Education, Opportunity and Social Inequality)* in 1973. In *Effets pervers et Ordre social (The Unintended Consequences of Social Action)*, Boudon (1977, p. 248) refers to MI primarily within the framework of economics, noting that we can identify a variety of interactionist paradigms in sociology (Marxian types, Tocquevillian, Weberian, Mertonian). He writes that “economic theory as a whole rests on a paradigm to which tradition gives the name of *methodological individualism*”, a statement that leads him to question the epistemological coherence of sociology. At this stage, MI was not yet considered by Boudon as a general paradigm for the social sciences, although Philippe Perrenoud (1978) wrote a review of the work in the *Revue Française de Sociologie* entitled “Les limites de l’individualisme méthodologique. A propos des *Effets pervers et Ordre social* de R. Boudon” (“The Limits of Methodological Individualism On R. Boudon’s *Effets pervers et Ordre social*”). It was apparently when the French historian, François Furet, commissioned him to write a book introducing sociology for a collection devoted to the major disciplines of the social sciences, that Boudon decided to make MI “the common thread” of



*La Logique du social* (*The Logic of Social Action*), published in 1979. MI seemed to him “henceforth to be the common denominator of convincing analyses produced by the social sciences” (Boudon and Leroux 2003, p. 59).

Boudon thus developed MI as an epistemologically unifying project for sociology at the very end of the 1970s, at a time when MI was still little known and poorly understood. This was compounded by the fact that it had been rather clumsily defended by Karl Popper’s collaborator John Watkins (see Bouvier 2023; Bulle 2018, p. 2025), who is often cited by critics of MI. Udehn produced an important work, supported by analyses of classical texts, which was destined to become a reference on the subject. Boudon adopted MI’s minimalist approach (reduced to P1) in line with Udehn’s, especially as this change enabled him to continue defending a version semantically free of any critical charge. Udehn had positioned his work prominently among the variants of “weak” MI, and Boudon conformed to this rather than oppose Udehn on the definition of MI, manifestly believing that the semantic battle was not worth the effort. In this context, his decision may seem subjectively rational. However, as I have argued, a slightly deeper analysis reveals that reducing MI to postulate P1 alone is confusing and, ultimately, untenable.

## CONCLUSION

MI, as presented by Boudon between 1979 and 2002, represents its constitutive and coherent version. It is grounded in a methodological principle shared with the natural sciences: The analysis of a whole into basic units endowed with causal properties that enable the study of the whole in question. MI thus establishes a first postulate (P1) identifying individuals as the primary sources of action (causal individualism). For Boudon, as for the founders of MI to whom he usually refers – primarily Weber and Simmel, but also Menger – the social sciences have an advantage over the natural sciences in that they have direct knowledge of the mode of action of their causal units. This mode of action, which brings principles of understanding and rationality into play, is intrinsically tied to social structures, particularly those internalized as structures of meaning by social actors, from which they derive the subjective meaning of their actions.<sup>16</sup> This interpretive approach justifies the inclusion of postulates P2 and P3, which involve understanding and rationality, as integral

16 As Boudon also argues, the formation of this meaning implies a neo-Kantian form of approach to reason – based on the use of tools of thought, conceptual systems, and so on – that is irreducible to mechanistic associative processes (Bulle and Morin 2024).

components of the constitutive version of MI. Consequently, contrary to popular belief, MI is fundamentally opposed to reductionism.

I believe that Boudon, prompted by the negative reception of MI in the literature, shifted his conception of the methodological foundation of MI from postulates P<sub>1</sub>-P<sub>3</sub> to postulate P<sub>1</sub> alone, thus aligning with Udehn's approach to MI. This shift allowed him to further develop a version of MI that incorporated postulates P<sub>1</sub>-P<sub>3</sub> while remaining free from the prevalent criticisms. Boudon himself never deviated from his overarching aim: enriching the central paradigm of macrosociology through his work on methods, interpretive sociology, and the rationality of social actors.

In an article published in the late 2000s (Boudon 2008), Boudon observes the failure of the great theories of the social sciences, which he argues have all relied on a conception of causality modeled on the natural sciences. These approaches, he notes, operate "in congruence with the postulate of materialism," a framework that has underpinned the success of the natural sciences and assumes "the primacy of the body over the mind," presenting the human mind as "an emanation of the organism's exchanges with its environment." Whether individuals are seen as driven by *social, cultural, or biological forces*, these forces share the characteristic of escaping the individuals' "control." However, as Boudon points out, while the general explanatory principles driving the natural and social sciences are comparable, they have different access to the way their proper objects interact, so that:

Materialism is a valid postulate in the natural sciences, but not in the human sciences, for the reason that it is realistic in the first case, but not in the second. It is realistic to see the natural world as the effect of material causes, and superstitious to see it as the effect of final causes. In the human sciences, the terms of this relationship are reversed (Boudon 2008, p. 45).

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## DISSECTING THE “GOOD REASONS” AND THEIR LINK TO RATIONALITY

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Raymond Boudon has continuously highlighted in his work the importance of a reference to rationality as well as to “reasons” and “good reasons” to explain typical social behaviors and subsequent social outcomes. He has also linked this notion of reasons to new developments regarding the very meaning of the concept of rationality, stressing in particular the contrast between instrumental rationality (linked to the so-called “rational choice model”) and axiological rationality. I will try in this chapter to analyze the articulation between rationality and good reasons he has proposed: I will focus on the issue of the possibility of finding out stable common interpretive devices, linked to an idea of “common sense”, a concept inherited from Descartes, as opposed to the variety of divergent either psychological or cultural motives; and on the link between those stable motives and the various normative issues in the social life. Boudon’s aim has been an attempt to unify interpretations of behaviors by displaying motives that can be seen at the same time as stable, localizable beyond social and cultural variations, and associated with a sense of “relevance”, beyond the mere pursuit of one’s self-interest (which can be however included in this sense of relevance). It is this combination of stable and relevance-oriented motives that constitutes the sense of rationality he develops.

I will seek in this paper, following on other papers devoted to this topic (Demeulenaere 2014, 2024) to: first, identify two major contrasted orientations stemming from the common use of “reasons”; second, summarily analyze the roots and the evolution of the use of rationality in the social sciences; and third and finally describe Boudon’s ambition and contribution to this debate and express some reservations about his theorization. In doing so, I will not refer to Boudon’s particular papers or books except when specific quotations are mentioned. He has often repeated his main arguments in his many writings and modified them, step by step, sometimes in a significant manner. A history of his theorizations of the notions of rationality and reasons should be made. This is



not the aim of the present chapter, which focuses on a conceptual discussion of Boudon's theory in its final formulation, expressed, for instance, in Boudon (2009, 2011).

#### THE REASON AND THE REASONS, AND THE ISSUE OF THE SCOPE OF INTERPERSONAL RELEVANCE

It is common, in everyday life as well as in theoretical literature, to refer to "reasons" to describe the motives that are responsible for one's action: it is possible in this respect to make a difference between a strictly individual preference and a reason that implies some sort of *interpersonal* justification. Thus, a philosopher contrasts two language habits, linking reasons to rationality and justification:

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If someone says "I like coffee," he does not need to have a reason he is merely stating a fact about himself, and nothing more. There is no such thing as "rationally defending" one's like or dislike of coffee, and so there is no arguing about it. So long as he is accurately reporting his tastes, what he says must be true... On the other hand, if someone says that something is morally wrong, he does need reasons, and if his reasons are sound, other people must acknowledge their force. By the same logic, if he has no good reasons for what he says, he's just making noise and we need pay him no attention (Rachels 2003, p. 12).

In this excerpt, a link is made between rationality, reasons, and some kind of "interpersonal justification": a reason is not just the expression of a personal preference, but something that purports some sort of justification that can be vindicated on an interpersonal basis (which must be localized: it could be either a limited given community, or anyone beyond the limits of any group). In fact, this involves two issues: who is concerned by this justification, and what are the means of this justification (and in particular, whether a reference to objective facts is the only basis for such a justification).

However, we can complete this simple opposition between individual preference and reference to "reason" by noting that this interpersonal dimension can have, in the common use, three basic localizations whenever the notion of reason is involved, including simple preferences.

One is the existing link between a given motive and an action: the "reason" why I drink coffee is the fact that I like it, and this already involves somehow an interpersonal dimension, since there is a "public" or an "objective" link between the fact that I like coffee and the fact that I will tend to drink it (although this evidently does not imply that other people should like coffee or drink coffee as



well, which is another issue). This link already involves a notion of *consistency* between two events: coffee liking and coffee drinking.

The second dimension intervenes when I say that the reason why I do not drink coffee is that there is no coffee available. Here again, there is a public or objective link between the reason why I do not drink coffee and the fact that it is not available. This, similarly, does not involve any universalistic reason that people should not drink coffee. Rather, it is an objective fact, triggering the use of “reason”, that if there is no coffee, then no one can drink it. This means that reasons in common language can also refer to environmental constraints that allow some actions but exclude others. Here again, there is a consistency issue, which is that no one can drink coffee if no coffee is available.

Finally, a reason can intervene if I say that people *should* drink coffee (maybe because I believe it is good for their health, and it is commonly assumed that people should take care of their health): this example is certainly debatable, but such a stance would correspond to a reason in a stronger meaning that involves “sound” *common motives* that are supposed to be pursued and to give strength to available choices meant to realize them. Those motives, however, can concern either a given community, with its own specific norms, or, more generally, anyone. They converge or oppose, more or less. This is a central issue: do reasons ultimately rely on variable community norms, or can they be considered as extraneous to them, and as allowing precisely the interpretation of various social norms? Clearly, Boudon seeks to reach the second position: he is not an historicist nor a relativist who would believe that everything is variable, social or historical.

I will now mention classical critiques addressed to a reference to reasons (or motives) as central in explanations for social behaviors. Three are Durkheimian in substance:

1. Motives (or reasons) are not observable, only behaviors are observable, and therefore we have no access to the effective reasons of a behavior.
2. Expressed reasons are not directly responsible for behaviors since they depend themselves on other “social” factors (which people may not necessarily be aware of): the reasons one displays are not the effective explanatory factor of a given behavior, since they depend, on the same time, on general social constraints and on social norms or social trends that drive behaviors in some directions one has not set up on the basis of their own rationality, like suicide tendencies.
3. Therefore, reasons (expressed by actors or reconstructed by interpreters) do not genuinely stem from any general, basic and universalistic sense of rationality (that would be derived from the classical notion of “Reason” as an

ultimate judge of what is appropriate, the way a philosopher like Kant qualifies it), extraneous to the norms and particularities of the social world.

In this perspective, we should either abandon a reference to reasons, because they are not the relevant factors for explaining behaviors; or we could maintain them, but reasons should not be seen as the ultimate relevant explanatory factors, as based on a shared sense of rationality, since they should themselves be in turn explained in a somehow causal and indeed non-rational way.

This can lead either to a *relativistic* position stressing the ultimate plurality of reasons based on a plurality of interests, values, and social norms, corresponding to various possible motives in different communities; or to a causal stance explaining through evolutionary mechanisms (natural or social) the variety and change of such values. In this way, Durkheim had his own theory of the evolution of norms that leads them toward a universalistic unification. Psychology often describes today such causal scenarios where specific norms emerge from specific contexts (Henrich 2020).

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Boudon's theory is different from those two possible paths: on one side he refuses an ultimately relativistic reference to reasons depending on values that cannot be unified by stable interpretive devices; on the other, he refuses a causal (whether natural or social) analysis of the development and change of values, because of the importance of the "meaning" associated with reasons. He will seek thus to combine a sense of reason that is at the same time relatively stable but allows also an adaptation to various individual positions and social situations, and therefore an explanation of the various historical and social norms and behaviors.

Somehow developing one dimension of Durkheim's critique of the use of motives, stressing again their unobservability, Peter Hedström has evoked another argument dependent on it: the fact that a social action can be traced to *many possible* motives, that are not observable as such. This argument can be seen as similar to the one that is found in the philosophy of mind where the "multiple realizability" thesis contends that a single mental state can be realized by many distinct physical states (Fodor 1974). Hedström consequently departs from an analysis based on motives and proposes to replace it by one that relies only on observed *behaviors*. However, when he writes, in the same paper, that "...some individuals may become more likely to adopt the behavior in question when many others have done so, while others may shy away from behaviors that have become "too common", and it is unclear how different mixtures of such groups may affect the collective outcomes they jointly bring about" (Hedström 2021, p. 503) he inescapably refers to what can be considered as typical motives that can be linked to typical actions, although they are not directly observed.

It is clear enough that in certain recurrent circumstances typical motives are triggered and that those motives can lead to typical actions (this scenario giving birth to explanatory models), although it is also equally clear that some situations can trigger *different* possible motives (maybe dependent on other characteristics of the people involved), and that given motives can also lead to different actions. The issue is then to debate whether there is some kind of link between the notion of rationality and those motives, given the plurality of motives and of actions linked to them.

It can be argued that Boudon’s main proposition regarding the theory of action he displays in the explanation of social phenomena is the affirmation of its “rationality” of actors: but what does rationality effectively mean, given the huge controversies it is associated with? Classically and repeatedly, he refers to the opposition between *causes* and *reasons* that has been a major element of the philosophical tradition regarding the analysis of action (Collingwood 1993 [1946]). Boudon’s perspective is to challenge two orientations expressing a *determination* of action either by naturalistic/ psychological factors, or by social/ traditional habits (internalized norms): as opposed to these two orientations, he insists on the “meaning” of actions that are engaged in by actors. He also refers to a *normative* sense of rationality, beyond a mere modeling of certain features of behavior. This normative dimension, obvious in his use of the notion of *good* reasons, is linked to a notion of “relevance” (although he does not use this term), that is some sort of *correctness* of the choice, this correctness depending on norms of validity.

This sense of correctness can already be found in Weber when he mentions a dimension of *richtigkeit* (Weber 1922 [1913]) to characterize rationality in the interpretation of actions. The notion of rationality refers here to the fact that in certain circumstances some choices are *better* than others (for instance in a mathematical calculus), and the guarantee of this superiority is linked to the sense of relevance linked to rationality. Therefore, the notion of rationality corresponds to an interpersonal criterion that goes beyond either cultural norms pluralism, or *unconsciously* determined psychological behaviors (those psychological behaviors can be themselves seen as either “adapted”, notably in an evolutionary fashion, or conversely, ultimately “irrational”). Rationality is therefore a normative reflexive dimension that applies to various normative matters. It is not only reducible to “meaning”, since this notion can be linked to diverse cultural settings leading to the possibility of relativism; the challenge is therefore to find reasons that are certainly meaningful for the actor, but that are also the expression of a sense of relevance beyond a mere pluralistic and relativistic collection of possible motives.

I will contend here that any reference to “reasons” can have two different ambitions: one is minor, the other major. Boudon will seek to pursue the major one, although his examples, in my opinion, often resort to the minor one.

The minor one is to describe the motives a given action can be referred to, namely interests or values (or norms), that are convergent or divergent among people. This does not involve any normative dimension regarding the motives, since there are many observable and often conflicting interests and values that lie behind social actions. For instance, we know that Nazis were antisemitic, and that they acted *according* to this antisemitism. Social sciences do refer to this variety of motives, that can effectively be named “reasons”, only because there is a *consistent* link between the motives and the actions following them. As mentioned before, this use of the notion involves also the constraints of a given situation, that either permit or impede certain behaviors.

274 This common procedure faces usually two difficulties: one is that this reference tends to be ad hoc, that is we tend to find the appropriate reasons (or more simply the motives) that correspond to the observable data, knowing that people could often have behaved in a different way, so the explanation is limited by this ad hoc dimension.

This leads to the second consequence: when we define and model ex ante a set of motives or behaviors that are responsible for anticipated data and consequences (in fertility issues for instance), they are not necessarily stable, since behaviors can change according to a variety of motives that have not been anticipated, and so the model is usually fragile.

Beyond this, a major ambition of a reference to reasons is to try to interpret motives as *good reasons* in certain circumstances, derived from a “common sense” (Boudon 2006): that is beyond the plurality of possible interests and values, to understand some sort of relevance of the choices that are made. It is already the case in the minor situation, where the selection of means is indeed *relevant* toward ends. But the ends are kept outside the relevance scenario. On the contrary, in the major ambition, they are integrated.

I will give a simple example here that seems to me to be rather uncontroversial: firemen services are never interrupted by holidays (although individual firemen take holidays); there is a “good reason” to that, which is that firemen are highly useful in the face of the continuous risk of fires, and that this risk itself never takes holiday. Thus, because of the urgent necessity of preventing a risk that never stops, the service set up to fulfill this function also never stops. I think that here the characteristics of such a situation that allows us to speak of “good reasons” are a sense of relevance beyond the mere *coherence* between motives and action: no one disputes the fact that firemen are useful for combating fires, and no one disputes the fact that the risk never stops; consequently, it is

consistent and coherent that the service devoted to combat fire should never stop. As opposed to the previous case, where the plurality of values or interests limits the scope of explanation in terms of reason, here it is more complete since it produces a convincing analysis based on *stable, common, and uncontroversial motives* that are not dependent on the need for further explanation.

I believe that this was Boudon’s core intention: finding out good reasons, that is shared motives that go beyond the simple consistency of actions with their various ends, various interests and various values, but do include the interests, the motives and the norms themselves as part of the “relevance” picture he describes, depending on a non-cultural “common sense” faculty. This leads to two positions: one is to go beyond mere consistency between motives and actions; the other is to find out stable motives that are not reducible to local community agreements, based on particular social and cultural norms, and the following of rules in a Wittgenstein sense that would be relativistic (Lukes 2008).

This leads to the definition of rationality at two levels (Gibbard 1990): the upper level is the characterization of the norms of rationality (in particular, are they limited to a sense of *consistency* or *coherence*?), and the lower level is their application to given actions and motives, interpreted therefore as reasons and good “reasons”. I would suggest here a distinction that is not made by Boudon: reasons can correspond to motives that are kept outside the issue of rationality, whereas “good reasons” do integrate the motives themselves. It remains to be seen whether this is possible or not.

### THREE THEORETICAL STEPS IN THE CLASSICAL SOCIAL SCIENCE LITERATURE REGARDING THE SENSE OF “RELEVANCE” ASSOCIATED WITH RATIONALITY

I will argue here that the early use of the notion of rationality in social sciences has had an immediate normative dimension. It is indeed already the case in Weber’s use of the notion of rationality, in its twofold dimensions, and similarly in Pareto’s conceptualization of “logical” actions. They both have a link with Hume’s legacy (Demeulenaere 2003 [1996]) and its reinterpretation in terms of so-called “instrumental rationality”, which is at the core of the analysis of rationality (Nozick 1993). What does this correspond to? Famously, Hume introduced in the literature two related issues: the fact/value dichotomy and the restriction of the reason’s abilities to the choice of means toward ends, as opposed to the choice of ends themselves, which goes beyond its scope. Hume’s theorization, however, already inevitably involves a sense of “relevance”: beliefs can be said to be *correct* if they are *validated* by empirical evidence, and it is

because of this that what will be later labeled “instrumental” rationality is itself linked to a sense of relevance, since it is based on the fact that an empirical *validation* of the relevant choice of means regarding an end is available, whereas there is no such warrant for the selection of ends themselves. This validation has an interpersonal dimension and can be said to be “objective”. This is the basic “positivistic” affirmation (Comte did refer to similar propositions when inventing the term) that will be so much influential in the economic literature associating economic action with instrumental rationality: the contrast between fact and value (the former being linked to the possibility of correct and therefore “rational” beliefs, and the latter being separated from such an obvious validation, and the related contrast between means and ends). Of course, any such positivistic proposition involves itself normative criteria of validity which can be discussed (Putnam 2002).

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The consequences of this first step are twofold. First, there is no available notion of *utility*, as an interpersonal and substantive point of reference that would allow us to describe what is intrinsically “advantageous” to people, since it inherently corresponds to the potentially divergent valuation of various options. Utility depends ultimately on various preferences that are either linked to psychological factors, the way Pareto describes them, or to social and cultural norms, the way Durkheim does. This means that utility cannot serve as a simple unified “rational” motive for analyzing and predicting the variety of behaviors and of social outcomes. Second, the notion of instrumental rationality is independent of any kind of specific ends, since the focus is only on the choice of available means, which allows the realization of any of them. A religious zealot can thus be rationally instrumental in the pursuit of the realization of their faith. It should be added that since there are often many different ways of achieving a given end, and that they suit more or less the actor, a motive alone is not enough to trigger in a simple causal way a straightforward action: in addition to the selection of ends, the actor must indeed also decide whether they will accept to engage in the action corresponding to the means.

The so-called “rational choice model” is the second step that follows this first introduction of the theme of rationality in the social sciences literature. It is deeply ambiguous regarding its exact content: the selection of ends it involves, the localization of rationality it implies, and its normative significance. Commonly, the rational choice model is said to be linked to “instrumental rationality” and to so-called “utilitarian” self-interests, but also to the realization of one’s preferences *whatever* they are, thus beyond any substantial notion of utility, and possibly including norms and values in substance opposed to self-interests (Sen 1977). However, those three propositions are independent and possibly incompatible. Indeed, the choice of means as such does not imply

any restriction regarding the ends, nor any kind of “utilitarianism”. Utility in this perspective should not be opposed to values, since the variety of the conceptions of utility can include values and normative dimensions. If, conversely, various preferences are introduced in order to solve this problem, then they have no necessary link with either utility (in any given restricted sense), nor with rationality, and they cannot allow the building up of a general predictable model, since they can vary according to different preferences.

Despite the distinction of all those dimensions, major proponents of the model tend to mix them without paying attention to the consequent ambiguities of these various orientations. Thus Coleman explains:

The individual-level theory of action I will use in this book is the same purposive theory of action used in Weber’s study of Protestantism and capitalism. It is the theory of action used implicitly by most social theorists and by most people in the commonsense psychology that underlies their interpretation of their own and others’ actions. It is ordinarily the dominant model of action we apply when we say we understand the action of another person: We say that we understand the “reasons” why the person acted in a certain way, implying that we understand the intended goal and how the actions were seen by the actor to contribute to that goal.

For some purposes in the theory of this book, nothing more than this commonsense notion of purposive action is necessary. For much of the theory, however, a more precise notion is required. For this if I will use the conception of rationality employed in economics, the conception that forms the basis of the rational actor in economic theory. This conception is based on the notion of different actions (or in some cases different goods) having a particular utility for the actor and is accompanied by a principle of action which can be expressed by saying that the actor chooses the action which will maximize utility (Coleman 1990, pp. 14-15).

The simultaneous reference to “reasons”, to “purposive action”, to “commonsense psychology”, and to the “maximization of utility” leads to theoretical confusion: those different concepts do not easily overlap, can be sometimes contradictory, and are certainly not predictive in a simple way of certain types of behaviors based on a unified model.

Regarding the localization of rationality, it implies three possible levels. If the reference is to mere instrumental rationality, it lies in the relevant choice of means, whatever the ends are; if it purports to include a substantial notion of utility (linked to “interests” defined in a specific way as opposed to “altruistic” behaviors and assumed somehow to be rational) the theory faces two recurrent difficulties. First, it is clear enough that people sometimes follow norms and



values, that can hardly be derived from any given interests if they are specified in a narrow sense, or that can be opposed to them, it is incomplete if it wants to make sense of these attitudes (Bowles 2016). Second, if, conversely, to escape this difficulty, the model gives up any substantial notion of utility and relies only on various preferences (that possibly include values), then it has no predictive strength and tends to be tautological: people prefer what they prefer and do what they do, which defines what their interests and their utility are.

Finally, rationality is usually only related to the consistency of preferences. There is a gap regarding this between the two usual presentations of the “rational choice model”. One corresponds to the way sociologists commonly refer to it, including “instrumental” rationality, and often substantial intuitions of what “utility” is supposed to be (although utility, as we have seen, can be derived from various preferences, which are, however, considered as “self-interest” in this respect). As opposed to this presentation, the standard one in economic literature is inspired by an effort developed by Ramsey (1978) at mathematically defining preference functions: utility is only the result of an ordering of preferences, and then the issue of rationality stems from the introduction of the “consistency” assumption in this ordering procedure. There is a subsequent debate about whether this consistency hypothesis has itself a normative significance (Blackburn 1998) or not. At any rate, it is far from the traditional Weberian presentation of instrumental rationality.

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Given all these intricacies, a recurrent move in sociological literature is to try to avoid any reference to normative issues, and to insist on the importance of modeling behaviors, without introducing any reference to rationality. Thus, Homans claims that “in (his) opinion calling the principle ‘rational’ adds nothing to its meaning, provided we are only concerned with how people do in fact behave. ‘Rational’ is a normative term, used to persuade people to behave in a certain way” (Homans 1987, p. 62).

I believe that it is precisely against this possible project of abandoning any reference to rationality that the main contention of Boudon’s theory has engaged, highlighting instead its centrality: if we want to model appropriately people’s behaviors, there is a need to find out the reasons and the *good reasons* they have to adopt one course of action instead of another. He displays, therefore, two main features of behaviors: people often tend to follow their own interests, and even though he acknowledges that these interests are linked to their various preferences, he nevertheless conceptualizes this attitude as an “utilitarian” one. At the same time, he stresses the fact that people also tend to adhere to values, notably beyond their own selfish interests, and sometimes oppose them. He contends, in addition, that these attitudes are not dependent on causal psychological (unconscious, psychological, naturalistic) factors, nor



on causal social norms (that are internalized without any reflexive or critical dimension). He consequently refers to these two typical attitudes in relation to the notions of “instrumental rationality” and “axiological rationality” borrowed from Weber. Those two notions can also be traced to Kant, who had similarly opposed the sense of one’s own interests and the intervention of *reason*, developing a sense of morality. It is interesting to note that this Kantian issue had also been at the root of Durkheim’s theory of morality, which similarly opposes individual interests and a dedication to the sense of duty stemming from social norms. In Boudon’s framework, the intervention of axiological rationality corresponds to three things: There can be “objective” that is in fact universally valid values, beyond the opposition to specific interests; people will often choose to follow those values against their selfish interests in typical social circumstances; however, a certain variation of those values is due either to the various actors’ positions or to the variety of situations themselves.

Boudon has repeatedly situated his analysis in the following of the classics, stressing however that his intention was not to adopt them in a completely faithful manner, but to freely recapture some of their central important intuitions. If we ask now the question of what the norms or criteria of rationality in Weber’s dual theory are, two directions can be traced: on one side there is indeed the contrast between instrumental and axiological rationality, but on the other side those two possibilities are still unified under the label of, precisely, a common underlying reference to rationality as such. Where can it be located, and what are its norms? Weber is not explicit about that, although he refers, as mentioned before, to a sense of *richtigkeit*. Regarding *Wertrationalität*, there is one obvious familiar rationality criterion, which is *consistency* between an action and the value it depends on: the norm of rationality can be said to be simply consistency. Regarding the values themselves, it is not obvious that they can be said to be rational in Weber’s analysis, despite Boudon’s claim to the contrary. On the *Zweckrationalität* side, there is also a *consistency* dimension, which is that if someone follows an end, they should consider the adequate means, and the foreseeable consequences (to see whether they fit with the pursued ends). This is also an issue of consistency. Moreover, Weber emphasizes that there is no sharp and definitive separation between interests and values, since they might overlap. Their major difference is the existence or not of a sense of duty (but somehow it can be said that people can have a sense of duty in the realization of their interests, if they *value* their interests more than anything else; a systematic preference for one’s own interest can also be seen as the effect of a social norm stressing for instance pride based on interests). Thus, the real contrast in Weber’s theorization lies between different types of motives, different types of pressure reinforcing those motives, but the

rationality norms are not themselves so much divided, and they turn mainly around the idea of *consistency* with various motives, basically either interests or values (the two being not completely separable, and the values tending to diverge historically and socially).

Boudon equally refers to different types of motives but interprets them straightforwardly in terms of *reasons* (that is, beyond motives only, motives being interpreted in terms of rationality), resorting, however, to *different types of rationality*. The list of these tends to vary in his successive writings and is certainly broader than Weber's dichotomy between the two kinds (instrumental and axiological). I will seek to analyze the correspondence he makes between typical motives (interests and values), the "(good) reasons" one has to adopt them, and their dependence on a series of types of rationality. Boudon never locates his discussion at the level of the definition of norms of rationality, except by stressing, as we have seen, the general contrast between causes (natural or social) and reasons, which seems to be a common feature of the different types of rationality. Thus, the question is whether there is more in the use of "good" reasons than the mere correspondence and coherence between actions and various types of motives (and the limitations of these by the situation constraints).

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#### THE ISSUE OF A CONTRAST BETWEEN INSTRUMENTAL AND AXIOLOGICAL RATIONALITY

Boudon announces the necessity to go beyond "instrumental rationality," which includes, in the way it is presented by him, at the same time, the (relevant) choice of means *and* the (equally relevant?) choice of "utilitarian" interests. Therefore, there is a shift toward the ends themselves (interests) that are here integrated into the definition of instrumental rationality he displays, and this instrumental rationality is thus labeled as "utilitarian". We have seen that this move is not necessary and can indeed be criticized because it goes beyond the basic requirement necessary for the definition of instrumental rationality. What Boudon does in effect is to equate different types of motives, derived from either interests or values, to a typology of rationality itself, as ultimately responsible for these different choices. But he does not present a general discussion of whether this corresponds to different norms of rationality; and of why, when people have the choice between interests and values opposed to them (and consequently different "rationalities" themselves in his words), they will decide for one option against another. He sometimes gives examples of the prevalence of values over interests. Regarding, this, he essentially develops the Adam Smith notion of an "impartial spectator" (Boudon 2001) who can define

*and* adopt moral norms, in circumstances where one’s interests are not directly involved. But this does not solve the problem of the opposition for one person of their interests and their moral sense when they face the two possible choices stemming from two different rationalities.

I will briefly explore therefore a major situation where the contrast between the two attitudes simultaneously intervenes: the social dilemmas, where people have at the same time an interest (specifically defined) in following a norm, and in not following it. I will seek to understand whether these two attitudes can be associated with a difference between two types of rationality (depending on different norms of rationality). It is a type of situation, interestingly, that is not discussed by Weber when he refers to the two types of rationality, nor Pareto, when he contrasts logical action and the adoption of values, although it was already present in Hume, Rousseau, and Kant’s theorizations. In such situations, where, if everyone follows their own interest (specifically defined), the result is bad for everyone, there is a tendency to the emergence (Coleman 1990) of a cooperation “value” that would solve the dilemma. It is commonly labeled in terms of “justice”, as opposed to non-cooperative free riders who are seen as behaving in an “unjust” manner, because they harm others -they impose negative externalities on them by not cooperating. People have, consequently, typically and recurrently, the choice between following their immediate interests or respecting the cooperation value that would enhance everyone’s interests. They can either choose one option or the other. There are in addition social pressures and social sanctions that are devoted to strengthening peoples’ respect for cooperation norms.

Although there is no systematic discussion of the emergence of norms in social dilemmas in Boudon’s work, he does present *examples* of them as a core case for the opposition between interest and values. It is notably the case in his treatment of the paradox of voting (1998). In such situations, people have at the same time an interest in cooperation (voting) and an interest in not cooperating, because one vote does not make any difference in a large-scale election: but if no one votes, the benefits of democracy (which are assumed to be desired) are lost. There is a subsequent demand for norms in favor of voting, which would solve the issue, although everyone has at the same time an interest in not voting, because one vote does not make any difference. I think this is the central case where “axiological rationality” clearly intervenes for Boudon *against* “instrumental rationality”. People decide to vote on the basis of a dedication to democratic values, against the interest they have in not voting.

But does this difference of choices between clearly defined but opposed interests and values imply also different types of rationality, that is, different norms of rationality? There are several things that are indisputable regarding

this: there are social dilemmas, there are cooperative norms, and they lead to typical conflicts between cooperative and non-cooperative strategies in such situations (Demeulenaere 2021):

- They correspond to a common sense of utility shared by all the participants: there is an available interpersonal comparison of utility, which stems here from the fact that, by hypothesis, in such a social dilemma, people have the same ranking of their preferences.
- However, in such situations, there are different possible strategies, and a dominant one, which, when generalized, leads to a general loss for all participants. This is the dilemma.
- Consequently, there is the objective interpersonal definition of a norm that would solve the problem: everyone has an interest in adopting the norm, but also an interest in not adopting the norm if others follow it.
- People thus decide to follow the norm or not: in both situations, there is a “reason”, either to follow one’s immediate interest, or to follow the norm against one’s immediate interest.

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It is interesting to note that in experimental devices that are set up in order to check how people do effectively behave in such public games, they in effect tend to behave *differently* (either on cultural lines or on individual ones, although there are some general tendencies that can be traced) (Henrich et al. 2001). Thus, some people tend to follow the norm, and some do not (it is likely that their behavior depends also on the level of sanctions and of social pressure, and also on the general features of a given society).

However, I do not think that we can deduce from that recurrent situation and those two typical opposed choices, based on the emergence of norms, different “types of rationality” as such in the sense that this would resort to *different norms* of rationality. People act *according* to either their immediate interests or to the norm that solves the dilemma, the norm being also rationally (instrumentally) set up in favor of their interests. These are different possible attitudes, one that can be labeled “selfish”, and one “moral”. But both involve the same criterion of rationality, that is *consistency* toward ends that are pursued but tend to be conflicting. The fact that one acts on behalf of a moral attitude, or conversely on behalf of a selfish one, does not imply that one acts because of a specific “axiological rationality” (except that it is intended toward a moral behavior, because instrumental rationality does not exclude such moral behavior, except if it is arbitrarily defined as excluding it). We can add that the setting of the norm itself clearly obeys instrumental rationality, since its aim is to escape the dilemma and to favor a public interest that is commonly pursued. It can be said moreover that the respect of the norm is consistent with the

pursuit of this public interest aim, that is itself consistent with the realization of one's interest, but eventually conflicts with it. All this had been seen by Hume, Rousseau and Kant.

Although Boudon develops the central example of voting, he does not discuss the general extent of the norms that stem from such social dilemma situations, nor the contrast and possible differences with *other* types of norms. He clearly does not limit to the emergence of norms in social dilemma situations, nor does he make a specific case out of them: he purports to describe a general sense of axiological rationality in situations that do not correspond to the properties of social dilemmas where the contrast between interests and cooperation norms is clearly defined and designed. He seeks to develop a general theory of adherence to norms, not only beyond social dilemmas, but also beyond the Rawlsian limitation to a basic framework where only some limited norms are derived from the ability of reason to select them, whereas all that ambition to reach "conceptions of the good" is seen as going beyond the limits of such rationality (Rawls 1971). This Rawlsian framework tends to define only a limited set of norms, mostly egalitarian and universalistic, that can be vindicated on rationality grounds. Many philosophers have argued that this path is too narrow, and that many other norms can be debated on the basis of *reasons* (Scanlon 1998).

Similarly to that contention, I believe that Boudon's ambition is to interpret, in addition to those basic egalitarian and universalistic norms, the meaning *and* therefore the rationality of *most* conceptions of the good (that is, values), their evolution, and the adherence to them. This leads him to an attempt at interpreting the change of norms and to analyze their evolution in a way that also purports to avoid any relativism. His theory of norms is somehow symmetrical to that of Durkheim, who similarly unifies all social norms as basically dependent on one fundamental social constraint, that of solidarity, analyzing how it applies to different social situations: but instead of those social constraints, what is found in Boudon's theory are the "good reasons" people have to follow such specific norms in different situations. Boudon does make and does accept a difference between cultural variable norms, that do not stem from rationality, and norms that depend on rationality that are adapted to different contexts. But he seeks to maximize the identification of the ones that are linked to good reasons, in the sense of something that escapes the variation of cultural norms but can be interpreted as adaptations of rational attitudes to particular situations with their own limitations. One good example is his theory of magics (2007): a basic similar cognitive ability is adapted to circumstances where the modern notion of natural law is not available, and therefore the contrast between magics and scientific inquiry does not hold.

In order to stabilize the interpretation of those various particular situations and norms, he introduces three major invariable elements, that are seen as pre-cultural, and corresponding to the “common sense” ultimate characterization of behavior:

1. A stable foundational normative reference: the sense of human “dignity”, which is borrowed from Kant.

2. A stable cognitive ability to correctly assess facts. He thus defends the idea of common features of rationality against the idea promoted by Levy-Bruhl of a “prelogical mentality”.

3. A reference to a stable non-cultural “conventional rational psychology”, that differs however from an unconscious psychology (of the Kahneman type) and which is referred to Simmel’s “conventional psychology”. Those two pre-cultural common-sense features are thus summarized:

“To conclude with a definition of the notions of decentration and sociocentrism, we can say that an explanation escapes sociocentrism when it is composed exclusively of two types of propositions: factual propositions subject to empirical verification and psychological propositions belonging to the register of ordinary psychology: that which is also called ‘rational’. It consists of attributing to the subject only immediately understandable motivations and reasons” (Boudon 2006, p. 123, our translation).

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The combination of those stable foundational competences with the variation of individual positions and of social historical situations is made through selection mechanisms that are such that: first, new ideas are invented; second, some of them are seen as “better” than the previous ones; and third, are consequently mostly adopted. This involves at the same time a theory of adaptation and indeed a theory of progress.

I believe, however, that there are four basic difficulties in this scenario: First, the human dignity being considered as a stable value, it is not obvious to interpret the various historical norms that clearly go against it (like norms in favor of slavery). Second, even if we refer to stable psychological aptitudes, many possible outcomes based on them are indeed available, which do not necessarily unify in commonly shared stable norms. They can lead to conflicting norms. It is not necessary that there is *one* solution to any dispute. Boudon’s stance is similar to “the formula offered by David Wiggins (which) is that over a potentially disputed issue, one side can gain the high ground, justifiably talking of knowledge and truth, by showing that there is ‘nothing else to think.’” (Wiggins 1990, quoted in Blackburn 1998, p. 301). But very often, such definitive conclusions are not reached in social life. It is also possible that a common psychology leads to irrational behaviors (Elster 2010).

Third, it is not obvious that the interpretations that are proposed go beyond existing opposed cultural norms. For instance, Boudon, following Weber, describes the interest of Roman Empire state officials in Mithraism, because of their common emphasis on hierarchical bonds. The “good reasons” here clearly correspond to local cultural norms that in no way can be seen as a rational choice in a more ambitious way corresponding to an idea of common sense, making the choice of Mithraism a “good” solution to the choice of a religion. Many examples of such good reasons given by Boudon are thus reframing of cultural norms, and their interior fabric, and this does not help combating relativism because no real sense of progress or adaptation can be localized in such situations. This involves the difference between “meaning”, which can be cultural, and a stronger sense of rationality aimed at finding out ultimately good solutions beyond the plurality of cultures, and making sense of them.

Finally, if there are clearly mechanisms of adaptation, it is not obvious to assert that new ideas are always responsible, on a sole rational basis, for the diffusion of them. Some better (from the point of view of some normative instance) ideas or norms can be available without being adopted. Boudon (1988) has been interested in these phenomena, but he considers optimistically that, in the long run, good norms tend to prevail. We must then wait for the long run to see if this proves to be true.

## CONCLUSION

In this chapter, I have tried to assess Boudon’s theory of rationality. I have expressed three main reservations. First, the constant reference to reasons and good reasons does not lead us to a clear break with the variety of social and cultural norms, since the very notion of reasons can include them. Second, the introduction by Boudon of different types of rationality is not clearly articulated in a discussion of the norms of rationality, and its relation to various types of motives. Those two points limit the scope of his great ambition. Finally, the attempt to develop the genesis of values and their evolution in terms of rationality as the result of selection mechanisms is not really supported by empirical evidence.

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## BOUDON ON TOCQUEVILLE

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Alexis de Tocqueville is one of the most discussed, most elusive thinkers in the history of social science and political theory. This is not because his writing is elusive or inaccessible, although he has been charged with an excessive concern with style, but rather because so many interpretations have been imposed on it, and from so many points of view. Raymond Boudon's book on Tocqueville, *Tocqueville aujourd'hui* (2005; 2006 English translation cited hereafter), concentrating on the second, "sociological," volume of *Democracy in America*, takes a particular, distinctive approach. It is a text, fundamentally, about explanatory form: about the types of explanations found in the text. Its aim was to "reconstruct its methodological principles from the analyses of the second *Démocratie* – based on a primary principle: that of axiological neutrality" (2006, p. 29). But there was another, which will be our primary concern here: "A further basic principle of Tocqueville's is his preference for explanation" (2006, p. 29). The "reconstruction" is also explicitly presentist, as the title makes clear. As he puts it, at one point, "We do not come across the word 'value' used in its modern sense in the work of Tocqueville or that of Durkheim. But if we want to have an idea of the significance of their thinking, it is helpful to retranslate it into a language that has become more familiar to us" (2006, p. 25). Similarly for "explanation": Boudon wants to translate into a familiar language unlike Tocqueville's own. Boudon is not only interested in understanding these explanations in light of present concerns about explanation, but also about the similarities to others in the "classical" sociological tradition with present resonance, notably Weber and to some extent Durkheim, though primarily with what can be thought of as the present rational-choice or analytical sociology paradigm, broadly construed. This then is a self-conscious reconstruction of Tocqueville, for a particular purpose – getting an idea of the significance of their thinking – and a particular audience – "us" – meaning present day sociologists.

The term “value,” I hope to show, is part of a family of problematic terms that reveal a gap between Boudon and Tocqueville that goes beyond historical changes in terminology. But it is a gap that is both difficult to understand and crucial for present concerns, and not just in sociology. To understand the issues requires a good deal of background. The claims of *Tocqueville aujourd’hui*, together with other writings of Boudon, provide a way into these tangled issues, which involve not only such anachronistic terms as “value”, but the question of the limits and applicability of ordinary psychology and rational choice to matters of belief, the nature of belief itself, the meaning and limits of “understanding” in explanation, the role of the tacit and the problems of characterizing it, as well as the meaning of Tocqueville’s own explanations and characterizations of the differences between the democratic and aristocratic modes of existence.

# RECONSTRUCTIONS, TRANSLATIONS, AND LACUNAE

The topic, and Boudon’s approach to it, falls within the general category of “history and philosophy of science,” which is the way I will treat it here. So it is perhaps useful to think about what a reconstruction does, and about the various kinds of reconstructions. Understanding what he is attempting, and then gauging this, thus, requires a brief excursion into the methodology of interpretation itself. The kind of “rational reconstruction” envisaged by Imre Lakatos (Lakatos 1970) for the history of scientific theories was different from Boudon’s. For Lakatos, the task of the historian was to reconstruct the problem-solving of the scientist. To do this required understanding the problem as it appeared to the scientist and employing a notion of scientific rationality to explain how they solved it. The “reconstructive” aspect is a matter of displaying the rationality of the response: showing why it was a rational response, despite whatever distractions appear in the historical record about the motives, religious beliefs, and so forth of the scientist that might have been part of the story. The point is historical. However, it uses our notions of rationality and applies them to enable our construction, or translation, of the problem situation: to make it intelligible, which is necessary because it is no longer our problem situation. The effect is to reduce scientific advance to situated problem solving.

Tocqueville set up a highly specific and constrained problem situation. His repeatedly announced aim was to understand the effects of democracy, as well as its sources. The source and cause was this: “The democratic social order in America springs naturally from some of their laws and conceptions of public morality” (Tocqueville 2006 [1835], p. 417). The aim of the book was

“only... to demonstrate how equality has modified” both “our inclinations” and “our ideas”: 2006 [1835], p. 417). He frankly acknowledges the existence of powerful causal elements, influencing “opinions, instincts and feelings due to circumstances strange,” including “the nature of the country, the origin of the colonists, the religion of their founding fathers, the enlightenment which they acquired, and their former habits, all things unconnected to democracy.” Similar factors operated in Europe “different from those operative in America but equally untouched by the fact of equality” (2006 [1835], p. 417). This provided the basis for a comparative analysis dealing with the sole cause of democracy and the sole effect of distinctive mores and ideas. But Tocqueville disavows any attempt to account for either the causes or consequences of these other things, save where they relate to his main theme: they are, so to speak, confounders whose possible influence must be separated from the main one. So, this is already a causal problem with a specific structure, involving the category of democracy and the categories of non-democracy. For him, democracy as an egalitarian form of society was a historical novelty, which produced a novel human type with novel social relations, novel habits of the heart, and novel receptivity to particular kinds of ideas (2006 [1835], pp. 417-418). Democracy was always contrasted to a society of ranks, and specifically to the two forms of aristocracy to which American society was most closely related, the French and the English. His empirical evidence is mostly directed at the contrast between these societies, as Tocqueville constructs them. This construction he substantiates “empirically” in a particular way – by citing his own observations of the normal practices and attitudes of the different societies. Tocqueville, it should be noted, was an exceptional observer, so the evidence consists in telling details that reveal the differences he is seeking.

The Lakatos version of rational reconstruction is emphatically *not* Boudon’s. Boudon is concerned neither with historical reconstruction nor with explaining Tocqueville in terms of his intellectual context and interlocutors, nor with the grand issues in political theory and history he engages with elsewhere, which provide insight into Tocqueville’s motivations. Nor does Boudon engage historically with the methodological issues of Tocqueville’s own time, particularly his relation to J. S. Mill, to Auguste Comte, to François Guizot, and to the ideas about social scientific laws that they were engaged in constructing. In Mill’s case, the ideas he was constructing were, arguably, a response to Tocqueville’s work, which he praised in reviews and in his correspondence with Tocqueville as a friend and ally (see Suh 2016). One of Mill’s constructions fits Boudon’s interpretation of dependent casual laws very closely, indeed more closely than anything in Tocqueville’s own self-explanations.

But Boudon's strategy is limited in another way that will concern me. The particular classical figures Boudon identifies Tocquevillian arguments with, Weber and Durkheim, share a common feature, one that becomes obvious when they are compared to such contemporaries as Franklin Giddings, William Sumner, Gabriel Tarde in France, and his admirers in American social psychology in the US, such as Charles Ellwood and Edward A. Ross. These contemporaries were focused on ideas like "consciousness of kind", mores, interaction and interstimulation, sympathy and empathy, and imitation, or, to put it more broadly, with what Ellwood called the psychological foundations of society. They trafficked in notions like instinct, had a concern with evolution and the relation of social life to its evolutionary biological origins, and to issues that would now fall under the category of cognitive science. Like Mill, and indeed arguably like Tocqueville himself, they believed that there were basic psychological laws that were the ultimate determinants of sociological phenomena, modified in their effects by local circumstances. In a sense, Boudon agrees with this. But his view of these psychological laws is different. For him, the relevant laws are those of folk psychology and rational choice, together, as we will see, with "understanding".

This is a large gap, and it raises a question about Tocqueville himself: can he be assimilated to Boudon's psychology? Edling and Hedström in their article on Boudon, "Tocqueville and Analytical Sociology" (2009), defend the forgetting of earlier thinkers. Leaving out these older figures and their concerns makes a certain kind of sense. They have dropped out of the current discourse in sociology. They did not survive the period, dominated by Talcott Parsons and Niklas Luhmann, which pointedly ignored them until they were themselves superseded by rational-choice; a process in which Boudon played a great role (Turner 1993). The problem situation of these older figures was different as well. They were all, in some respect, concerned with the problem of Darwinism as it was reduced to the slogan "survival of the fittest," and were attempting to identify the pro-social psychological forces that explained or underlay social life. The flaw in their use of these concepts was that they tried to do too much with them. This led to reductive accounts of society, and many similar attempts at reduction, including rational choice. In any case, they were effaced within sociology as it professionalized into national traditions, especially when "social psychology" turned into the study of attitudes and the quantitative rejection of null hypotheses as the standard of proof (Danziger 1990; Greenwood 2003). But they were also omitted from the line of succession cited by Boudon, which included and stressed Weber and Durkheim. They were omitted in Weber because of his self-imposed limitation of sociology to subjectively meaningful action; in Durkheim because of the Renouvier-derived concept of the idea

of autonomous laws of sociology, and of the collective consciousness and the implied dualistic psychology that replaced it.

But there is more to the story, both with respect to Tocqueville and Boudon, and it is a sufficiently confusing and consequential one to try to untangle. Boudon ridicules “depth psychology” in the form of “mimetic desire” to explain conformism, one of Tocqueville’s important explananda in his discussion of democracy (2006, pp. 86–87). In the case of conformism, Boudon’s response is to reduce the issue to his own terms, with the comment that “Benthamite utilitarianism is sufficient” (2006, p. 87). But for many other things, and perhaps conformism itself, Benthamite utilitarianism is not sufficient. Tocqueville himself spends a great deal of time on “natural propensities of the human mind” (2006 [1835], p. 447), instinct, and unconscious effects. These concerns do reappear in Boudon, but indirectly, in the form of conditions of understanding. He comments that

Tocqueville, Weber and Durkheim did not lose their way by concocting theories that deny the existence of human nature and which make man the integral product of his environment, such as those of the Marxists and culturalists. If the idea that the human being is entirely conditioned by his environment is taken literally, how would it be possible to understand the behaviour of individuals belonging to cultures very different to our own? The very concept of “understanding” supposes that there are cognitive processes and affective mechanisms that transcend “cultures”. (Boudon 2006, p. 102.)

This may seem like an arcane issue, but it can be clearly stated: if we accept that there are “cognitive processes and affective mechanisms” that transcend culture, are we not back in the world of the post-Darwinians looking for the psychological foundations of society? Why is this not a kind of depth psychology? Is this not in conflict with, or at least an alternative to, even an extended version of rational-choice? Can things like *mores* be accounted for in this model? Or do they operate in terms of the kinds of explanations – mimesis, for example – that Boudon avoids?

These questions point to a tension over cognitive and affective processes that recurs in various forms, both in Boudon’s writings and in his uses of Tocqueville for polemical purposes. It will be my concern in what follows, for “presentist” reasons that are parallel to Boudon’s own to ask: what might, in a future “sociology,” be the role of cognitive processes and affective mechanisms not accounted for by rational choice broadly construed, including “understanding”. Although the concepts of these earlier thinkers dropped from the standard lexicons of sociologists, the phenomena they pointed to did not disappear, and

live on as lacunae in sociological accounts. Some of them have been revived in contemporary cognitive science. Tocqueville was concerned with many of these lacunae, a point to which I will return at the end. But the lacunae haunt Boudon as well. Reconstructions leave something out: part of the job of understanding Boudon as well as Tocqueville is to understand what was left out, how it was left out, and to ask whether it matters, and why.

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Boudon shows that the lens he chooses for his reconstruction in order to identify arguments and forms of reasoning in Tocqueville's most "sociological" work is in fact a powerful one, and that at least a few of Tocqueville's arguments can be assimilated to it or interpreted in terms of his idea of rational action. But the basis for identifying methodological commitments in Tocqueville's own writings is thin. For Boudon, Tocqueville's significance as a methodological innovator rests on his having "founded the sociology of ideas, of beliefs and of values" (Boudon 2006, p. 11), and on his rejection of "both those who see only chance in history and those who see only necessity," as well as "those who see history as merely a combination of chance and necessity" (Boudon 2006, p. 8), those who see history as determined by individual will and those who see it as the product of social forces, because they neglect the crucial role of ideas in historical development. A "basic principle," affirmed by Tocqueville,

is that social processes are always a result of the combined effects of chance and necessity. Necessity, to the extent that they are always the result of a basic cause that is part of human nature. Chance, to the extent that the opportunities that allow a group or individual to improve their situation are far from being always due to necessity. (Boudon 2006, p. 101.)

Chance and necessity stand in for a variety of other polarities, around which Boudon organizes his interpretation.

The upshot of these affirmations is negative: they exclude reductive accounts which appeal to culture or laws of history, or to the acts of leaders. But the significance is positive: to implicitly affirm the crucial role of ideas, or rather people with their ideas, in historical development, but in conjunction with social forces, mores or culture, and individual wills. There are two major steps in this reasoning: the first is about individual rational action, the second about the long-term institutional and collective processes that they can be used to explain. As Edling and Hedström note in their comment on Boudon (2009), the logical structure here is from individual action to institutions or collective



phenomena produced by individual actions which persist and then influence future individual actions: what came to be known as “Coleman’s boat”. This is then applied to, or found in, Tocqueville’s own reasoning, especially in the example I will discuss below: his accounts of the spread of Christianity and also of the revolutionary ideals of the Enlightenment. The point of these accounts is to explain, in terms of individual action, what the rejected alternatives purport to explain: long-term trends that look like “laws” supervening on individual action and differences in culture of the kind cultural determinisms focus on.

Action is the normal focus of rational choice. The difficulties arise when this form of explanation is extended to belief. Boudon’s own views on cause and the explanation of belief can be found in the entry on belief in the Boudon-Bourricaoud *Critical Dictionary of Sociology* (2015 [1990]). The focus of the entry is to refute or complicate the claims made by Marxism of class determination of belief, and also ideas about culture as a determinant of belief<sup>1</sup>. But much of the entry is engaged with the same issues Boudon later discussed in relation to Tocqueville. The Marxist and culturalist accounts are replaced with the idea that “beliefs must be understood and analyzed as responses to interactive situations” (Boudon 2006, p. 47). This points them to examples where the expected class determination of belief is falsified and the actual causes take the form of adaptations to situations and their meaning to the subject (Boudon 2006, p. 46). The systemic nature of belief is crucial to meaning to the subject. Thus, the adherence of many Jewish intellectuals to communism in France is “less because of the universalism of the Judaic tradition than because ancient practices tended to distance them from the university establishment, which in the main tends to the right” (Boudon 2006, p. 47). But we are warned that it would be excessive to treat beliefs in all cases “as dependent variables”. In the case of the Protestant ethic, for example, “from it comes the idea that beliefs can play the role of independent variables, that is to say, appear as a cause rather than an effect” (Boudon 2006, p. 48).

The reasoning here requires a good deal of unpacking. But there is a key to it that bears on everything else that follows. A form of epistemic voluntarism is part of the argument. “Responses to interactive situations” are not cases

1 The basic thoughts of the sociological tradition, they comment: “can be gathered under several principal titles: the sensitivity of *beliefs* compared with reality; the more or less systematic character of beliefs; the role and function of beliefs in the determination: 1) of the objectives of individual action and social action; 2) of the most appropriate means for the realization of these objectives; the relation between social structures and beliefs; the role of interests in the determination of beliefs – in other words the full significance of the utilitarian theory of beliefs.” (Boudon and Bourricaoud 2015 [1990], p.42.)

of mechanical “determination”. The term “adaptation” is crucial: this is a term covering the whole range of responses to the “interactive situations” in question. Moreover, the responses have meaning to the subject. The meaning, as is suggested by the case of the Jewish intellectuals, can derive from “ancient practices” as well as the immediate interactive situation. Adaptation in this broader sense might be summarized by the notion of “convenient to believe”. What is convenient to believe is the result not merely of one’s interests, one’s immediate objectives, the encompassing social structure, comparison with reality, or the place of the belief in the more or less coherent belief system of the agent, which makes some beliefs harder or easier to accept – more or less convenient to believe in the broader sense of convenient in the face of these multiple situational constraints or inconveniences. A simple example of this would be the beliefs involved in the self-justification of actions to others.<sup>2</sup> The Jewish intellectual might well find it to be more convenient, given the interactional situations he is routinely faced with, to adhere to the beliefs underlying communism and to justify himself more readily to his co-religionists and peers than to rebel against them and adhere to the prejudices of the more rightwing establishment, of which he is not a part and with whom he does not interact. This is a paradigm case, and it does have parallels in Tocqueville. But it is also a complex case, which the use of the notion of “ancient practices” shows: assimilating them to the model of rational choice is possible, for example, through such means as showing the rationality of conformism. The idea that we must choose to believe is sometimes called epistemic voluntarism: what someone believes is a matter of acceptance. But the question of the nature of what is being conformed to raises its own questions: are they “ideas” in the sense of epistemic voluntarism, or something that does not conform to the rational choice model of choice of beliefs?

Boudon’s primary concern was not to defend rational choice as a psychological model of belief formation and acceptance. His concerns are rather with the sociological issues: how do the dominant ideas change? But the topic of epistemic voluntarism bears on both. The Victorian temptation was to say that rationalization was the long-term process that produced change. In short, we just got smarter, less superstitious, and so forth (Lecky 1919 [1865]).

Tocqueville’s achievement, for Boudon, was that he brought people back in with their ideas, in an explanatory rather than evaluative way – one of

2 Sperber and Mercier have made what I think is an important point of distinguishing practices of justification and explanations of action (2011, 2017). I have suggested elsewhere that one can assimilate justification to action explanation by way of the Andy Clark’s concept of predictive processing (2018, pp.62-63, 105, 107-109). But I will not pursue this point here.

which is objective, or for which we can have evidence. The result was a model of explanation that accounts for ideas and also for their social consequences, such as their diffusion and competition with other ideas, causally, rather than through dependence on an ideological account of the truth of the beliefs. How did Tocqueville manage this? As Boudon suggests,

...Tocqueville explains beliefs, changes in beliefs, the rhythm of the process of diffusion of beliefs, and the outcome of the conflict between competing religious belief systems, by the action of causes. These can be identified on the basis of evidence, *and they reside in the motivations experienced by individuals situated in a given context that encourage them to embrace one or other of the belief systems available in the market.* (Boudon 2006, p. 18; emphasis added).

For Boudon, this was Tocqueville's problem and also his achievement. But it also reveals a deeper problem.

The "quest for objectivity" and the idea that the causal effects of ideas "can be identified on the basis of evidence" are difficult to put into practice. Like Weber, Boudon says, "Tocqueville wants to see the new science seek an objective route into the subjective" (Boudon 2006, p. 13). The last phrase is central to what follows. The reality that is sought is the subjectivity of the other, his beliefs, or the values that consciously motivate him: this is the force of "motivations experienced by the individual." "Experienced by" with respect to motivations implies consciousness, which in turn implies the person who is being explained and understood has subjective access to these beliefs: they are the kinds of beliefs he or she would affirm explicitly. So what is the objective route into the subjective? As we will see, this depends on a related question, which is more basic and even more problematic: how does "the objective" causally influence or produce "the subjective"? For Boudon, this necessarily becomes a question about the rational basis of belief. But it is important to see why this is the case. It depends on assimilating belief explanation to action explanation.

The causes of actions for Boudon are "motivations" understood as beliefs and values: "motivations," understood as a combination of beliefs and values, conforms to the "belief-desire" model of action explanation, in which beliefs and desires taken together are causes (Bittner 2001; Davidson 1963; Turner 2017). "Experienced by individuals situated in a given context" is an important qualification, as is "available in the market". But the result is familiar from the problem of historical explanation generally. It is one of reconstructing the situation, the beliefs and values that directly cause actions. The problems begin with beliefs and values themselves. It is one thing to attribute them and treat

them as parts of the causes of action. It is another to account for them, and also changes in beliefs, within the framework of the belief-desire model. Boudon's use of the term "people's ideas" is telling: the aim is "to understand why individuals accept or reject them". To say that is to say the acquisition of a belief is being treated as an action, within the explanatory framework of situated rational choice. To believe is to choose to believe something. The translation to "values" enables this: what were understood as involuntary tacit acquisitions of customs and mores, such as "ancient practices," is assimilated to the model of value-choice in the face of the utilitarian need to conform – adaptation or what is convenient to believe, and thus cases of epistemic voluntarism. But because we are changing terms, this is a rational reconstruction into our language: Caesar didn't have "values," in his own subjective terms, but we use these terms to reconstruct his subjective situation.

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There is no place in Tocqueville that he affirms this "everyday" or "ordinary" psychology model of explanation of belief: it is Boudon's own reconstruction. But he explicitly attributes it to Tocqueville's explanatory practice:

The next question will thus be to determine the type of psychology that is appropriate. Ordinary psychology or depth psychology? Here again the analysis of Tocqueville's work brings a clear response; all that is needed is ordinary psychology, the same that we use in everyday life. It is the only one that can legitimately deliver both conviction and consensus. Following the work of the American sociologist Robert Nisbet (1966), this approach has sometimes been described as "rational" psychology. But it is preferable to speak of "ordinary" psychology, since the causes of behaviour reside not only in reasons but also in motivations. (Boudon 2006, p. 109.)

For Boudon this meant that motivations could be understood largely in terms of utilitarianism. As he says of Tocqueville, "He paid a glowing tribute to the utilitarian tradition. It is 'of all the philosophical theories, the most appropriate to men of our time' and 'it contains a large number of truths that are so evident that all it takes is to enlighten men as to their existence for them to see them' (DAIL, p. 173)" (Boudon 2006, p. 129). But Boudon also identifies a tension: "At the same time he knew that 'beyond his material concerns, man still has ideas and feelings' (DAIL, p. 173) and that it is essential to take account of this important fact if we want to explain social phenomena in a satisfactory manner". And for Boudon this implied that "[Tocqueville] appreciated why it was so important not to replace the model of *homo oeconomicus* with a model in which man is conceived to be fundamentally irrational, as if he was

driven by cultural, social, psychological or biological forces". Tocqueville was in the middle:

In advance of his time, he refused, as did the Nobel Prize-winning economist Amartya Sen (1977), to make man into "a rational idiot". But he also rejected the idea of making him into an "irrational idiot" subjected to forces over which he has no control (Boudon 2006, p. 129).

As we will see, Tocqueville used language that is difficult to interpret in these terms. But the reconstruction enables Boudon to give an account of collective phenomena. As he puts it in a discussion of Root (1994), these considerations allow for an explanation of national differences in patterns of protest:

Like Tocqueville, Root sees collective phenomena as the out-comes of understandable and individual motivations and reasons. The average Londoner readily admits that a member of parliament elected in the provinces is hardly likely to be impressed by his protest, while the average Parisian knows that, even today, demonstrating in the rue de Varenne or the rue de Grenelle, outside the offices of the Prime Minister or the Minister for National Education, may well be effective. The Parisian and the Londoner have the same psychological make-up, but their behaviour takes account of the institutional factors characterising their two different contexts. (Boudon 2006, p. 38.)

The difference, in short, is not a matter of "cultural determinism", or cultural difference, or even of the psychological makeup that results from different social experiences – the Parisian and Londoner have the same psychological make-up – but a result of more or less utilitarian ordinary rational selection in different contexts that produces consequences at the level of collective phenomena.

The existence of a variety of opinions or ideas allows for a "market" of choices, and therefore a market-like mechanism of selection, with collective results. The fact that people conform to the selections of others, to the dominant opinion, as an adaptive mechanism, together with market selection, produces a climate of opinion. But the "choice" model also allows for intellectual novelty and invention, and for ideas in this way to be explanatory:

The irrefutable existence of this mechanism of rational selection of ideas contains within itself, let us recall, a refutation of all "culturalism". It is accepted that certain values derive from adaptive mechanisms and may in consequence

be different from one culture to another. But it may not be affirmed that values can be introduced only through the operation of adaptive processes. (Boudon 2006, pp. 70-71.)

“Introduced” is the key term here. For most people, the mechanism is adaptation to the values that were already present to be conformed to. But some people invent the value ideas that others use to adapt to new situations.

## SUBJECTIVIZATION AS A PROBLEM

Boudon gives the example of the spread of Christianity in Rome and the subsequent spread of rationalism out of Christianity after Luther as models of this kind of explanation. He comments that Tocqueville argues that

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the Roman Empire was a favourable terrain for the expansion of Christianity. Why? Because a single God is a symbolically appropriate representation of the Emperor, but also because the status of the subject recalls the image of a central authority, whilst the obligation that all have to be subject to the Emperor evokes the submission to God. (Boudon 2006, p. 14)

This is also an explanation that requires some unpacking. Boudon calls it a “theory,” and comments that “This theory can be compared with that of Weber, who was also concerned with why Christianity so easily entered the Roman Empire” (Boudon 2006, p. 14). As Boudon reconstructs him,

Weber put forward the idea that monotheistic cults, initially that of Mithra and then Christianity, were attractive in particular to the functionaries and soldiers because they reminded them in a symbolic manner of the organisation of the Roman Empire. As soon as Eastern monotheistic cults appeared in the religious ideas market, Roman soldiers and functionaries were easily converted. (Boudon 2006, p. 15)

Christianity was a winner in a newly created marketplace of ideas, with buyers, so to speak, in a novel condition, which made a particular idea attractive to them because it “reminded” them – an “ordinary” cognitive mechanism – in a “symbolic manner” – perhaps a bit more mysterious mechanism – of an organizational fact, which led them to being “easily converted” – also a somewhat less ordinary cognitive process. Tocqueville does not say this, but only that there is a certain similarity in ideas of a single God, which is a

symbolically appropriate representation of the Emperor, that recalls the image of a central authority, and because the political notion of submission “evokes” the theological one (Boudon 2006, p. 14).

For Boudon, what is of interest here is the social conditions, not the psychology, but the psychology has an important effect: the mixture of peoples in Rome and their subservience to a single God-like Emperor unified them in a universalistic way. He takes from Tocqueville that “the ‘social state’ of the Roman Empire had introduced a certain degree of equality, according to Tocqueville, in the form of the equality of all under the Tutelage of the Emperor” (Boudon 2006, p. 16). This equality was, in a sense, external: it was a legal status.

What kind of explanation is this? Epistemic voluntarism is at the core: it is a choice. The social situation of the agents, in this case, the functionaries and soldiers, was that they were alike in being subjects to a central authority: the epistemic situation was that there was a marketplace of ideas with a particular set of intellectual goods. But “social state”, in this case, actual Tocquevillian terminology,<sup>3</sup> implies something more, perhaps involving a subjective condition in response to an actual state of affairs. This turns out to be an important difference.

The mere fact of subservience to the single emperor and mixing of peoples are external or objective “causes”, to the extent that we can speak of “cause” in an unproblematic way in relation to the “causes” of beliefs and values.<sup>4</sup> But these facts are external: the idea itself is abstract, and also external to the individual, but becomes subjective. How does it become subjective? How is the problem of the relation of objective to subjective content solved? This is a problem Boudon flags for us with his comment that “Tocqueville wants to see the new science seek an objective route into the subjective. It is still doubted, even today, that this is possible” (Boudon 2006, p. 13). The term “evokes” (2006, p. 14) is at least a start on this problem: what is evoked is a subjective response. And we get similar language in other contexts. Declarations of the rights of man

spread so readily because they made abstractions of any particular national or cultural context. Such declarations were comparable to religious texts to the

3 Though probably taken from François Guizot (Guizot 1972, p. 153; see Richter 2004).

4 Obviously this is not Tocqueville’s or Boudon’s problem alone. Elster’s article on Tocqueville’s account of the coming of the French Revolution captures the issue in its title: “Preconditions, Precipitants, and Triggers” (2006). Each of these terms is “causal,” and the preconditions included the “values and beliefs” of the Enlightenment, which are part of the subjective. But to explain the subjective, to be “the sociology of ideas, of beliefs and of values” that Boudon claims Tocqueville founded, needs to be something else.



extent that they expressed general ideas on the rights and duties of men towards each other that were considered to be applicable to any particular context. “The French Revolution worked in the same way as the religious revolutions [...] it considered the citizen in an abstract way, outside of any particular society, in the same way as the religions considered man in general” (Tocqueville 2004, p. 62). As a result the religion of the rights of man spread through the same mechanisms as the great traditional religions (Boudon 2006, pp. 17-18).

“Were considered” is the term that points to subjectivization. And it is one that can be, like evoked, supported by evidence: we can show what people said when they considered the term applicable to any context. The fact of abstraction facilitated general acceptance. Later, he notes the role of criticism, especially exemplified by Luther, which led to its extension from one previously uncriticized sphere to another, and of the equality of men. These were also subjectivized by virtue of being “considered,” which is something for which we have evidence. Similarly, we have something like the force of ideas, which “encourages”

... the causes of Christianity’s success are also those of its decline. It insisted on the equality of men, but equality encouraged criticism. By encouraging criticism, equality also encourages disbelief (Tocqueville 2004, p. 178). Earlier than others, and in particular before Durkheim and Weber, Tocqueville had realised that Christianity was the religion of the end of religion. (Boudon 2006, p. 19)

This gets us a causal sequence, or at least a genealogy, from Christianity to equality, to criticism, to disbelief. It is more or less an exemplary explanation of a collective phenomenon. And it has the elements of Coleman’s boat. But we can ask some basic questions about it, including two crucial ones. Is this a good model for explaining these cases? And was it Tocqueville’s explanation? The last question, as it happens, provides a path to answering the first.

## CONDITIONAL LAWS, ORDINARY PSYCHOLOGY

Boudon’s general methodological commitments with respect to explanatory form are clear. He attributes them to Tocqueville, whom he places in a familiar line of intellectual successors.

In their writings on the methodology of the social sciences, Weber (1922), Popper (1986 [1957]), and Hayek (1953) have, each in his own terms, developed the idea that one of the essential objectives of the social sciences



is to establish conditional laws, and have made clear that a law of this sort is only plausible from the point at which it can be considered to be the fruit of understandable psychological motivations and reasons on the part of the individuals concerned. (Boudon 2006, p. 39.)

This is a model explanatory form. And it is also the one Boudon wishes to reconstruct Tocqueville in terms of. There are two distinct parts of it: the idea of conditional laws and the model of action explanation. And there is a vague corollary, to the effect that understanding is linked to non-material features of human nature. The idea that equal conditions lead to the acceptance of general ideas, exemplified by Christianity in Rome and the rights of man in Europe, is a model conditional law. So we may suppose that it gives us a clue to the problem of relating the objective to the subjective.

The idea of conditional laws is more puzzling than it appears, though less puzzling in principle than in relation to Tocqueville's own practice, which plays with the idea in subtle ways. As noted, the core idea is found in Mill, and indeed represents its own historical puzzle, because while this discussion, in the context of the inverse deductive method, is most clearly applicable to Tocqueville's practice, Mill ascribes the method to Comte (Jones 1999; Suh 2016):

If, therefore, the series of the effects themselves did not, when examined as a whole, manifest any regularity, we should in vain attempt to construct a general science of society. We must in that case have contented ourselves with that subordinate order of sociological speculation formerly noticed, namely, with endeavouring to ascertain what would be the effect of the introduction of any new cause, in a state of society supposed to be fixed; a knowledge sufficient for the more common exigencies of daily political practice, but liable to fail in all cases in which the progressive movement of society is one of the influencing elements; and therefore more precarious in proportion as the case is more important. (Mill 1982, Book VI ch. 10, § 4.)

One important point needs to be made about this. Tocqueville's literary practice was to play with paradox: to identify what might be expected and to then show the surprising alterations or combinations that were actually produced. One can think of his analyses as identifying a condition of a law, and showing why, because of this condition, the law does not hold in particular cases or in particular respects. Tocqueville does not use this (Comtean and Millian) language, though he does speak of causes.

There is a sense in which a general law that is not “conditional” simply needs to be taken as given: there is nothing additional to be said. As Mill says, explanation is the substitution of one mystery for another. But conditional laws imply conditions, or at least claims about the absence of nullifying conditions, for the application of the general law, as well as conditions for the exceptions to it, which are not mysteries. The law-like statement in Tocqueville is one we have already seen noted by Boudon (2006, p. 19): “By encouraging criticism, equality also encourages disbelief” (Tocqueville 2004, p. 178). The case of *Democracy in America* turns out to be one where the law does not simply apply. America is not simply an application of a general law, but an exception to the very process the law describes: it does not lead to religious skepticism, but the opposite.

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The idea that there was a natural succession toward first universalized beliefs, then skepticism from dogmatic local religious attachments, such as those of the people absorbed into the Roman Empire or Europeans in the progression from the Reformation to the Enlightenment, is an example of a conditional law. It was not a general law or universal truth. It was contradicted by the fact of American religiosity and religious diversity. The intervening cause was a local historical one: “It was religion that gave birth to the English colonies in America. One must never forget that. In the United States religion is mingled with all the national customs and all those feelings which the fatherland evokes. For that reason it has peculiar power” (Tocqueville 2006 [1835], p. 432). But the power had another explanation, which is relevant to the problem of belief acceptance:

In this way Christianity has kept a strong hold over the minds of Americans, and – this is the point I wish to emphasize – its power is not just that of a philosophy which has been examined and accepted, but that of *a religion believed in without discussion*.

And further,

In the United States there are an infinite variety of ceaselessly changing Christian sects. But Christianity itself is an established and irresistible fact which no one seeks to attack or to defend.

And this had derivative effect on American morals.

Since the Americans have accepted the main dogmas of the Christian religion *without examination, they are bound to receive in like manner a great number of moral truths derived therefrom and attached thereto*. This puts strict limits on the field of action left open to individual analysis and keeps out of this field

many of the most important subjects about which men can have opinions.  
(Tocqueville 2006 [1835], p. 432; emphasis added)

Two things are notable about these comments: the dogmas and the moral truths that follow from them are not a product of examination or discussion, nor are they open to analysis, and perhaps more importantly, they are not even a subject on which men can *have* opinions. It is questionable whether there is anything like an act of acceptance of the kind epistemic voluntarism envisages, or in the sense envisioned by the model of rational action. These dogmas are not a matter of choice. The adherence to Christianity was not a case of epistemic voluntarism, much less a choice in a market. It is dogma without authority or speculation, which is to say, without conscious adoption, conversion, or decision.

Tocqueville's thought here is a complex one. On the one hand, Americans have a philosophy, which amounts to an epistemology.

... it is noticeable that the people of the United States almost all have a uniform method and rules for the conduct of intellectual inquiries. So, though they have not taken the trouble to define the rules, they have a philosophical method shared by all. ...to seek by themselves and in themselves for the only reason for things, looking to results without getting entangled in the means toward them and looking through forms to the basis of things—such are the principal characteristics of what I would call the American philosophical method. The Americans never read Descartes' works because their state of society distracts them from speculative inquiries, and they follow his precepts because this same state of society naturally leads them to adopt them. (Tocqueville 2006 [1835], p. 429)

The point about this “philosophy” was that, although it was sometimes articulated, it was not an abstract or even explicit doctrine. It was fundamentally tacit: no one has taken the trouble to define the rules. These were precepts that were followed, shared by all, but not articulated as a doctrine. If it were, and propounded authoritatively, or arrived at by “speculative inquiries”, it would contradict the basic feature of the “philosophy,” that individuals “seek by themselves and in themselves the only reason for things”. This is what makes them naturally, meaning unreflectively, Cartesians.

Paradoxically, however, this kind of self-reliance makes them slaves to opinion. Social opinion and common patterns of behavior had a special role in this society.

Not only is public opinion the only guide left to aid private judgment, but its power is infinitely greater in democracies than elsewhere. In times of equality men, being so like each other, have no confidence in others, but this same likeness leads them to place almost unlimited confidence in the judgment of the public. (Tocqueville 2006 [1835], p. 435)

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Adaptation to this community and conformity were important, and as Tocqueville saw it, somewhat frightening – the rise of mass society was the theme of J.-P. Mayer’s early interpretation of Tocqueville’s work (Mayer 1939, 1940). But what was also striking to Tocqueville was the absence of this kind of pressure in the aristocratic society of France, in which the aristocrats simply ignored the opinions, and even the humanity, of others. In that context, the kind of social learning that characterized the American setting didn’t exist: universalism as a philosophy was simply an abstract idea, not a tacit understanding of the world rooted in daily experience. The “state of society” is in this sense not a determinant in the sense of Marx or culturalism, but a social learning environment that “naturally leads” to the kind of non-explicit “philosophy” in which individuals are self-reliant. This is an explanation in terms of a social state, but the relevance of the social state is in terms of experiences and learning from them.

If we make another distinction, we might account for this anomalous result. But how did Americans get that way? Boudon cites a “law” that might be taken to explain it:

*Another Law.* Human nature is singular, but the psychology of the human being varies with social context. In particular, equality changes its sensitivity. *In their most illustrious period, the Romans cut the throats of enemy generals after they had been dragged in triumphant procession behind a chariot, and fed their prisoners to wild animals for the amusement of the people. Cicero, who greatly bemoaned the idea of a citizen being crucified, had nothing to say about such atrocious abuses of victory. It is clear that to his eyes a foreigner was not at all the same sort of human being as a Roman* (DAII, 542). (Boudon 2006, p. 48; italics in original.)

And there is an application of this law to France:

Very much the same was still true of the eighteenth-century France where Madame de Sévigné could write to her daughter that “hanging seemed (to her) such a refreshment”, because in her time, as Tocqueville points out, “it was not clearly understood what suffering was if the person was not a gentleman” (DAII, 541).

“Democratic” societies are by contrast differentiated by the fact that “the severity of people is softened” (DAII, 541). For example, “when the ranks are more or less equal, all men think and feel in much the same way, and anyone can at any moment imagine what the all the others would feel [...] There is no woe whose pain could not be appreciated” (DAII, 541) (Boudon 2006, pp. 48-49).

When Boudon uses the term “psychology” here and claims it varies with social context, he is consistent with Tocqueville, to be sure. This was the basic problem that the second volume of *Democracy in America* was devoted to: the psychological effects of democracy. Whether this account can be re-interpreted in terms of “ordinary psychology” is an open question, but even Boudon does not try to do so. Instead, he relies on the more elastic notion of understanding: we can “understand” why the severity of the people is softened, even if we cannot explain it.

It may be noted that in *L’Ancien Régime* (1955 [1856]) Tocqueville noted the obverse of the softening of this law in France: where inequality, together with mutual isolation, led to not regarding inferiors as fully human, yet sympathizing in the abstract, and the persistence of both attitudes even after the revolution. The explanation for this was that “It was no easy task making fellow citizens” out of people “who had for many centuries lived aloof from, or even hostile to, each other and teaching them to co-operate in the management of their affairs” (Tocqueville 1955 [1856], p. 107).

The peasants’ upbringing and way of living gave him an outlook on the world at large peculiar to himself, incomprehensible to others. And whenever the poor and rich come to have hardly any common interests, common activities, common grievances, the barriers between their respective mentalities become insuperable, they are sealed books to one another, even if they live their lives side by side. (Tocqueville 1955 [1856], p. 135)

And he makes a telling observation:

We are reminded of the conduct of Mme Duchâtelet, as reported by Voltaire’s secretary: this good lady, it seems, had no scruples about undressing in the presence of her manservants, being unable to convince herself that these lackeys were flesh and blood men! (Tocqueville 1955 [1856], p. 183)

Tocqueville makes other comments about the incommensurability of world views or mentalities – social context dependent psychology, in Boudon’s own terms. Tocqueville notes that “The genuine love of freedom, that lofty

aspiration which (I confess) defies analysis...is something one must *feel*, and logic has no part in it" (Tocqueville 1955 [1856], p. 169; emphasis in original). And such comments, which are ubiquitous in Tocqueville, point to a number of problems for any interpretation, and specifically for reconciling Boudon's basic methodological premises with his own practice. In what follows, I will focus on one issue: the apparent gap between any version of rational choice or ordinary psychology and the kind of explanation needed to account for radically divergent world views or mores, or what Tocqueville calls "habits of the heart" (Tocqueville 2006 [1835], p. 287).

## HIDDEN FORCES AND CULTURALISM AGAINST RATIONAL CHOICE

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Is there a genuine explanatory gap between rational choice or ordinary psychology explanations (supplemented perhaps by a rich notion of "understanding") and the facts of cultural difference? Or does Boudon have a way, consistent with his methodological commitments, of eliminating this apparent gap? And if not, does Tocqueville at least point to an alternative solution to the apparent gap? These questions take us deep into the wilds of methodology, but they are unanswerable otherwise.

Boudon's comments on culturalism and its Marxist-influenced variants, presumably of the Bourdieu variety, are explicit, and negative:

God knows well enough that the contemporary human sciences readily assign the processes they want to explain to hidden forces, under the persistent influence of Marx or Freud, and also of a variety of intellectual movements, such as culturalism, structuralism or sociobiology (Boudon 2004, 2005). By making human behaviour the result of causes operating without the knowledge of the subject, all of these movements turn their backs on the notion that human behaviour should be considered "in principle" to be understandable in the Weberian sense. (Boudon 2006, p. 42.)

This is a more radical "principle" than it appears. It is not Weber's, who considered human action – not behavior – to be his sole topic, and took the criteria for being action, that it was subjectively meaningful, to be less than an explanation even of action (Turner and Factor 1994, pp. 29-44; Weber 2019 [1922], p. 81, 93-94). For him, the subjective meaning was a veneer over a more complex set of causes, some of which were unknown or even not "understandable" to the agent in the sense of being subjectively meaningful to him. Subjectively meaningful action, as distinct from behavior, which might be instinctual, purely emotional, or habitual, happened to be the thing that the

sociologist was concerned with, not the whole explanation of behavior or even of “action.” Boudon goes much farther: “According to this principle it is the reasons and motivations of the subject, as far as the sociologist can reconstruct them, that should be considered as the sole causes of his behaviour” (Boudon 2006, p. 42)

Weber would have rejected “sole causes”. This “principle” is a radical methodological claim. Is it Tocqueville’s? Boudon wishes to claim it is: “It is because he believes in a methodology that sees the understanding of human behaviour as an essential element of any form of analysis, that Tocqueville so vehemently rejects the mechanical theories of philosophers of history, of historians and of the social theorists of his time” (Boudon 2006, p. 41)

On the basis of his critique of hidden causes and generalisations, Tocqueville adopts a methodology centered on the idea that the beliefs and behaviour of individuals are driven by understandable reasons and motivations rather than social, cultural, psychological or biological forces. This methodology allowed him to put forward an impressive number of conditional laws in the second *Démocratie* and *L’Ancien Régime*, that still appear even today to be solid and convincing. It is readily noted that Tocqueville is greatly concerned to ensure the credibility of these laws by showing how they follow on from “understandable” motivations and reasons on the part of individuals in respect of their own environment – in the wider sense of that term. (Boudon 2006, p. 44.)

But Boudon’s own précis of Tocqueville’s methodology is less radical than the “principle” and is stated as a negative: “By refusing to give weight to the intentions, reasons and motivations of the human being, the intellectual movements I have just referred to are examples of the theories that Tocqueville so detested because they ‘exclude [...] men from the history of mankind’” (Boudon 2006, p. 42). This is a different claim than the rejection of hidden forces and the insistence that human behavior should be understandable in the Weberian sense. It merely excludes those doctrines that refuse to “give weight” to conscious motivations.

Boudon is going beyond, at least on the surface, both Tocqueville and Weber: Tocqueville’s position seems to be instrumental and concerned with establishing and not ignoring understandable motivations; Boudon’s with asserting their explanatory sufficiency. He attributes the idea that understandable motivations are sufficient for explanation to an identifiable tradition that not only includes Weber, but can be extended to account for Durkheim’s explanation of the relation of crises to the suicide rate.



The approach recommended by Weber, Popper and Hayek assumes that the analyst can reconstruct the motivations and the reasons that are the causes of the actions, beliefs or attitudes of individuals. The theory of understanding that would later be developed by Weber is based on the assumption that it is in principle possible to reconstruct the reasons and the motivations of any given social actor, whatever his cultural distance from the observer, once care has been taken to collect the necessary data. (Boudon 2006, p. 39.)

He argues that the fact of understandability itself requires us to acknowledge the universality of basic cognitive and affective mechanisms:

If the idea that the human being is entirely conditioned by his environment is taken literally, how would it be possible to understand the behaviour of individuals belonging to cultures very different to our own? The very concept of “understanding” supposes that there are cognitive processes and affective mechanisms that transcend “cultures”. (Boudon 2006, p. 102.)

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And this suggests, though he does not say it directly, that the universal cognitive processes and affective mechanisms in question equate to “ordinary psychology” as supplemented by “understanding.”

The apparent gap between this kind of explanation and the differences in culture that motivate culturalism thus disappears in principle: it is filled by “understanding”. It can also be made, sometimes at least, to disappear in practice. Commenting on Durkheim’s account of suicide, Boudon reinterprets Durkheim’s observation that “In all cases, the greater the intensity of the crisis, the lower the rate of suicide, and as the crisis calms down, the higher the rate of suicide mounts”. Boudon explains this in individualistic terms consistent with ordinary psychology, or at least an ordinary understandable response: it “is because during a period of crisis those most likely to commit suicide have a greater incentive to forget their personal problems for a while” (Boudon 2006, p. 40). To apply Boudon’s methodological strictures fully, one would need to reinterpret all of the apparent culturalist and hidden causes explanations in a similar universalistic way, or dismiss them. And indeed Boudon supplies examples of how this might be done.

But Boudon also qualifies this methodological argument in a way that returns to Tocqueville’s instrumental view

Let us clarify matters. If a theory concerning the reasons and motivations that inspire the behaviour of an individual seems to be incompatible with certain data, it would be advantageous to stay as long as possible within the framework



of the rational, and to attribute the actor's behaviour to reasons and motivations that are readily "understandable". (Boudon 2006, p. 45.)

It is "advantageous" to stay inside the framework as long as possible. But in this passage at least this is only a prudential rule. It can be further explained by our preference for hypotheses that can be assessed for their credibility by an observer.

Although it seems implausible that the wood-chopper should want to burn logs in his hearth, it is possible that he wants to make a wooden object, a piece of furniture for instance. The observer can easily test the credibility of this second hypothesis. It is only when he has assessed all of the "understandable" motivations that the observer might envisage that he could venture an "irrational" interpretation and assume that the wood-chopper has a compulsive need to cut wood. (Boudon 2006, p. 45)

Weber explains the example differently: he finds that credibility is added to an interpretation by considering connected actions, such as taking the wood to a market. Boudon's point is about the preference for non-hidden causes:

In short, irrational explanations of behaviour should be considered as having a residual nature. As they introduce hidden causes and as they are in consequence not testable, they can only begin to be objectively confirmed if we are convinced that all possible "rational" explanations have been exhausted, that is to say all explanations in terms of understandable reasons and motivations. (Boudon 2006, p. 45)

He argues that these are principles "Tocqueville always follows in his analyses. He never uses an irrational interpretation of the behaviour that he examines for the reasons and motivations which lie behind its existence". Boudon claims that "Weber and Durkheim have no hesitation in treating rain dances as rational" (Boudon 2006, p. 45).

The apparent equation of rational and understandable – alien to Weber for whom affective responses were also understandable – goes both ways. It redefines "rational" in terms of what is understandable, and also implies that what is understandable is "rational" in an ordinary or quasi-ordinary sense. Tocqueville thus treats the cruelty that is a characteristic of "aristocratic" societies as rational – as understandable.

## THE TACIT AND SOCIAL LEARNING: THE UNRESOLVED PUZZLE

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The issues here are difficult to explain, much less resolve, for a number of reasons. But we can nevertheless gain clarity about them. The basic problem is one of language. We do not, and in principle cannot, adequately characterize the tacit in terms of the non-tacit, that is to say such explicit things as claims, beliefs, values, dogmas, assumptions, and so forth. To do so is to do violence to the tacit elements themselves, which characteristically are inexpressible: in Michael Polanyi's famous formulation of the concept of tacit knowledge, "we know more than we can say." What is tacit is at least partly inaccessible to us. It is embodied, at a cognitive level (such as pattern recognition) that is beyond our conscious control or involuntary, individual or personal in nature (hence the title of Polanyi's magnum opus *Personal Knowledge* (1962 [1958]), and only partly shareable with others, for example, by those who recognize overlapping patterns (Turner 2023). But we can deploy an impressive but problematic array of analogical terms to describe that which is tacit: mentalities, culture, presuppositions, and so forth, as well as the terms listed earlier, like values, which are employed analogically. But we also have Tocqueville's own term, "habits of the heart" (Tocqueville 2006 [1835], p. 287), and Hume's treatment of causality in terms of habit or custom understood as habit.

The nature of this analogizing is important to understand, especially in relation to the concept of epistemic voluntarism. The overt meaning of value is associated with value-choice, and with an overt action or affirmation. It is voluntary and conscious, rather than tacit. The tacit analogue is neither. It is attributed because it is as if someone were making that choice or affirmation. This is a deeper problem than it appears: in many languages, there is no semantic difference between affirming or being committed to and knowing. This has been a longstanding issue with Bible translators (Needham 1972, pp. 33, 36-37). But there is a problem with our own reflections and access to our tacit background. We can "reflect" and express our "assumptions", in accordance with the dictum "state your assumptions", but one can do this only analogically. Euclid could state assumptions. We can only, in effect, theorize about what we are "assuming". And our reflective theorization is itself limited by our language and the scope of comparisons we can make. A later thinker might find us to be unconsciously racist or sexist, but we would not have been cognitively or theoretically equipped to identify our own implicit biases. And even the notion of bias is being used analogically here.

But the confusion of knowing and commitment is telling. The habits of the heart are bound up with language, and acquired with language, but they are not the same. The mother who tells her infatuated teenage daughter "you

don't know what love is" is not making a solely semantic or linguistic point. She is alluding to an experience which is simultaneously embodied, emotional, customary, and irreducibly private or personal, learned with experience and feedback, yet at the same time partly recognizable and "understandable" in others. The word cannot exhaust or adequately portray this thing. And it is this kind of inexpressible habit of the heart that Tocqueville is alluding to when he speaks of freedom as a "lofty aspiration which (I confess) defies analysis...is something one must feel, and logic has no part in it" (Tocqueville 1955 [1856], p. 169; emphasis in original)

Boudon places great emphasis on the fact of symbolic similarities in accounting for the acceptance of beliefs: the similarity between the Christian God and the Roman Emperor, for example. And he notes Tocqueville's own appeal to symbols as "tools of moral teaching that are, if they are not irreplaceable, at least 'practical,' to use the qualification Tocqueville did not hesitate to employ in this respect" (DAII, 527) (Boudon 2006, p. 20). This has the effect of turning what is not understandable into something understandable, because it is overt or explicit. But this conversion to the explicit has the same limitations as reducing the mother's response to the semantics of "love." It does not capture the realm of feeling that goes with the symbols. When Tocqueville speaks of Americans unreflective devotion to Christian dogma and therefore to the "moral truths derived therefrom and attached thereto" (Tocqueville 2006 [1835], p. 432) he is, similarly, not talking about explicit truths or derivations. He is talking about a regime of feeling together with reason, which is irreducible to either, but also tacit rather than explicit or overt, as symbols and their similarities are.

Whether this can be fit into Boudon's capacious category of understanding is an open question. But it is interesting that when he comments on these tacit differences, he appeals to something explicit: not the practical, but images. Boudon contrasts Tocqueville favorably to Guizot, who contrasts the "*génie*" (genius or spirit) of England and France with the comment that "anyone looking closely at the English genius would be struck by [...] the lack of both general ideas and of a haughty approach to theoretical questions". He commends Tocqueville for recognizing "the existence of these differences but rather than explain them by hidden forces such as 'génie' or 'principle' that Guizot employs, he explains them by the fact that the enduringly aristocratic nature of English society produces different images in the minds of individuals to those of their French counterparts" (Boudon 2006, p. 41). Can "images" do the work of filling the gap?

Boudon tends to reduce that which cannot be assimilated to ordinary psychology and understanding to the irrational and "hidden causes", which he rejects. This is a way of filling the gap. Tocqueville is open to filling the gap

in a different way: not by a theory, like culturalism, or by an account of the tacit. But he does supply something telling and vivid when he describes the social experiences that support the habits of the heart: both the experiences of democratic interaction, which support the “Cartesian” self-reliance of the American, and the separateness of people living side by side but in different class worlds of the aristocratic order. These tell at least part of a story about what we might call social learning: about the experiences that are the basis of the regime of feeling captured by the term “habits of the heart”. Tocqueville typically characterizes this in contrast to the ideas: “If, in the course of this work, I have not succeeded in making the reader feel the importance that I attribute to the practical – in a word, to their mores – in the maintenance of their laws, I have missed the principal goal that I proposed for myself in writing it” (Tocqueville 2006 [1835], p. 295). This realm of the practical does not fit into the category of the irrational. Far from it: the habits formed from practical experience are habits that result from feedback, the success and failure of practical efforts, and the social feedback that accompanies experience. But the diet of experience differs, as does the result. And this points to a kind of explanation of such things as the American dogma and the taste for freedom that is absent from Boudon.

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PART VI

**TRAINING  
THE NEW GENERATION**





## COMPLEXITY FROM CHAOS: THEORIZING SOCIAL CHANGE

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In addition to and in part because of his work on education and social mobility, Raymond Boudon was an important theorist of social dynamics. He made his contributions and approach to the important subject explicit in his work *La Place du désordre* (1984), translated into English as *Theories of Social Change: A Critical Appraisal* (1986). Boudon asks a question fundamental to the philosophy of social science in this work. Is a theory of social change possible? As I will argue, he ultimately concludes – somewhat surprisingly – that theories of social change are not possible, thus casting into doubt the entire project of comparative historical sociology. However, his pessimism is eased slightly by a sentiment that, despite the impossibility of theories of social change, a scientific study of social change is possible.

This precarious and perhaps slightly contradictory stance begs the question of what a scientific study of social change would look like. Boudon gives us a sense that it would have something to do with the study of unintended consequences, but does not delve into the details of the best methods to anticipate those consequences. And this is unfortunate because if he were more willing to commit to a style of analysis for analyzing unintended consequences, he might also have been more optimistic about the potential for theories of social change.

Boudon presented unintended consequences as a vast and eternally unmanageable sea of contingency and chaos, responsible for social outcomes: the place of disorder he refers to in the title of the original volume. Individuals are like sailors setting out for a specific port but cast on unknown shores by unpredictable tides and currents. He assumes that the sea of social contingency is so chaotic that no theory can reliably predict which port will be reached, for instance, which social outcome will be achieved. This perception drives his sense that systematic theories of social change are impossible.

However, he was writing before many tools and approaches we now rely on had reached full maturity. Today, several methods have been developed to systematically analyze complex situations – which can resemble chaos if the underlying patterns are not detected, including computational modeling, network analysis, complexity sciences, and natural language processing (for examples, see Manzo 2014; Hedström and Bearman 2009).

These methods – and the theories upon which they are built – allow us to reconceptualize a sea of chaos as an area of vast complexity that nevertheless can be explored and even analyzed, although perhaps with great difficulty. If it is possible to detect complex patterns within the chaos, then there is also a potential for theorization. And despite Boudon's deep skepticism about a full accounting of social complexity and unintended consequences, I argue that his work suggests a specific and promising path forward.

## REASONS FOR PESSIMISM

Boudon presents a clear argument in *La Place du désordre* (1984) by laying out in detail the reasons that he has such significant doubts about the possibility of theories of history. He begins his critique in chapter 1 by arguing that many, if not in fact all, existing theories of social change are wrong. They are, however, wrong for different reasons – and he proceeded to give each of these reasons its own chapter.

In my opinion, chapter 2 begins to set out a positive agenda and therefore should have gone at the end of the critique. I, therefore, delay my summary of that chapter.

Chapter 3 takes as its topic nomological theories of social change, which is to say laws of social change that we could expect to hold constant across circumstances in much the same way that we expect the law of gravity to govern the movements of celestial bodies as well as any object with mass. Boudon argues that the social laws, such as the law of supply and demand, depend on an understanding of why individuals act a certain way, and that circumstances throughout history vary so greatly that one cannot reliably understand or predict why someone would act a particular way in a particular point in time. Thus, any law-like theory of social change that has an *if A, then B* logic will only ever be right in a particular context – and therefore will be wrong the majority of the time. To make matters worse, it is difficult to know in advance whether any given context is one in which the law will be right or wrong. Casualties of this criticism include Parsons, Rostow, and Popper (which is remarkable given Popper's similarly deep suspicion of the possibility for historical explanation), as well as theories of collective action, development, and modernization.

In chapter 4, Boudon takes on structural theories of social change. One of his central examples is Margaret Mead's *Cultural Patterns and Technological Change* (1953), which posits that traditional communities will be resistant to change because of the complex interdependencies that link various cultural practices into a resilient web of interlocking systems. Boudon argues that this structural theory has been proven insufficient by Trude Epstein. Epstein's work shows that technological change, in the form of modern irrigation, is able to transform traditional villages in India. Further, the change is incomplete and affects different traditions in different villages. From this and other examples, Boudon argues that structural theories of social change, in fact, always depend on something that is not structural. As a result, structural theories of social change are at most only partially right. And that anyone who believes the structure alone causes the outcome is, in fact, wrong.

Chapter 5 addresses theories that identify one fundamental cause of social change. Here, Boudon singles out Marx and theories of conflict as inadequate, even with Marx's own writings. In *The Poverty of Philosophy* (1900), Marx identifies class struggle as the motor driving the transition into capitalism, a position that notably gained many adherents in sociology. In the same work, however, Marx explicitly argues that the discovery of gold in the Americas was essential to the disruption of the feudal system – a factor self-evidently unrelated to class struggle. Further, the ongoing argument between views that material conditions always drive culture or culture always drives material conditions is unhelpful. Indeed, according to Boudon, theories attempting to identify one fundamental cause are not merely wrong – they are so entirely wrong that they are not even proper scientific theories. They are instead suspect and grandiose metaphysical claims.

Chapter 6 closes out the roster of theories with a focus on the impossibility of deterministic theories of social change. With a range of different examples, Boudon argues that theories that seem to be right only work because they are explaining closed systems. And those closed systems will always eventually become open – through exogenous shocks and chance events – at which point the existing deterministic theories of social change will also be wrong.

By the close of chapter 6, the prospects for theory are bleak. Boudon has made a strong case that the existing arsenal of theories, which in this case would be mid-twentieth-century theories of social change, are too grand, too ambitious, and have no sense of an appropriate scope for their application. Further, they do not recognize that contingency and chance will always be a large factor in determining the path of social change. He appears to see these deficiencies as insurmountable, however, I believe that he does in fact lay out

a positive path forward for constructing better theories in chapter 2 – though I cannot say for certain whether he did so intentionally.

## AGGREGATION AND CONTINGENCY

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If we return to chapter 2, which could have served as the penultimate chapter of the book, he begins the process of mapping out a new direction for sociological research. This chapter focuses on aggregation effects and temporal contingency. Aggregate effects are emergent effects that change depending on the number of people involved. Classic examples include Karl Popper's seekers of solitude (Popper 2006 [1957], p. 158) and Jean-Paul Sartre's farmers of Sichuan (2004). In Popper's example, if one person goes to the mountains to be alone, they will enjoy perfect solitude, but if everyone goes to the mountains, no one will find solitude. Sartre's example, which he used to illustrate a dialectical relation between persons and nature as well as a metaphysical relation between creation and destruction, is more complicated. The peasants in Sichuan desire more arable land for cultivation. To create more land, they cut down the trees that stand on the land. The individual strategy is adopted by all Sichuan people. Sartre called this a unity of purpose and action. The collective nature of this undertaking transforms the action into a destructive force: The systematic demolition of the forests by all the people calls up a counterimpulse in nature. The trees had in fact protected the farmers from natural flooding, which is unleashed to devastating effect by the deforestation. The farmer's attempt to reform nature destroys their newly cultivated land.

These two examples present instances of perverse outcomes, where the actions of actors produce the opposite of what was intended when everyone does them to negative effect. Good outcomes can also follow from perverse effects, examples of which fill Bernard Mandeville's *Fable of the Bees* (1993 [1714]), where vanity promotes industry and pride provokes generosity. The most famous example however is Adam Smith's argument in *The Wealth of Nations* (1994 [1776]) that selfish actions can produce national prosperity, thereby improving the material circumstance of the impoverished (an argument that was strongly influenced by Mandeville's work).

The next chapter, "Giving Disorder its Due," which is the last substantive chapter, explores temporal contingency. Boudon focuses here on the idea that a particular conjuncture of circumstances will always play a role in social change. In this chapter, he concludes that theories of social change must be specific to particular places and times: "It is only possible to construct theories (in Popper's rigorous sense of the word) of social change about partial and local social processes firmly situated in time and particular circumstances"

(Boudon 1986, p. 207).<sup>1</sup> This criticism poses a serious challenge to the idea of a ‘real theory’ of social change, given that social change is necessarily about the transition between a set of particular circumstances unique to one time into a different set of particular circumstances that define a new time. So, either social change is a continuous process that resides in all moments and social processes – and therefore a special category of social theory devoted to “change” does not make sense – or the causal effect is residing outside of local circumstance somehow, a proposition that Boudon explicitly rejects.

There is, of course, also the added issue that theory is usually considered to consist of generalizable abstractions portable across time and place, though not necessarily all times and places. And when theory is not generalizable or portable, one may ask whether it is really a theory or, in fact, an untested hypothesis. Boudon does not, however, push his criticism this far.

Additionally, a historical setting is inevitably going to include at least two independent causal sequences, and following Augustin Cournot’s definition, the intersection of two causal sequences will be random. So, pushing someone out of a window cannot account for the likelihood that someone will walk under the window with a mattress and save the individual from injury. Following this model, a conjunction of circumstances will always be random.

These two issues then set the stage for Boudon’s understanding of social change. In his definition, social change is the product of “emergent effects from the aggregation of the behavior of individuals in conditions which were changing under the influence of a particular conjuncture of circumstances” (Boudon 1986, p. 130). This definition makes sense if we consider that social and historical change takes place at the aggregate level in a historical moment, which will also necessarily encompass several (if not billions) of causal chains. Social changes are changes to the whole of society, and history is not the story of one person but the story of civilizations, nations, and empires. The story of nations, peoples, and lands is, of course, made up of the stories of individual persons. However, if aggregate effects exist and outcomes vary based on the number of people involved, it follows that large-scale historical transformations will unfold differently than they would under the same circumstances for one individual alone. Further, the inclusion of more than one causal chain and different circumstances will make things even more difficult to predict, if not inherently random.

It follows from this that understanding the intentions and motivations of individual actors is insufficient for theorizing about social change. In Boudon’s

<sup>1</sup> There is evidence that Boudon revised his opinion on this issue in later works, such as *The Poverty of Relativism* (2005).

perspective, explanation at the level of individual action is good and necessary. Still, it is not adequate to explain the aggregate consequences of individual behavior, which may be quite unmoored from the intentions of actors. Therefore, if one believes that social theories must be based on individual motivations alone, theory cannot address social change.

The issue is not merely one of alignment between motivations and outcomes, for instance, good intentions can produce bad things, and bad intentions can produce good things. If good motives are consistently aligned with bad outcomes, that may also lend itself to theory, prediction, and explanation. But if effects are inherently unpredictable because they are the result of aggregate actions and contingent conjunctures, then the task is perhaps impossible.

324 Thus, Boudon portrays unintended consequences as unknowable – or more precisely, the subset of action-effect links that are unpredictable and arbitrary. Boudon states “the way in which aggregation effects of the type  $M = M(m)$  shape things is thus not always straightforward, and a more or less lengthy training is necessary if we are to understand it. It is no more ‘natural’ to the human mind than handling the differential calculus and, like that discipline, has to be learned” (Boudon 1986, pp. 57–58). Now we might think that that this could be the purpose of graduate training in the social sciences, but Boudon instead suggests the training should consist of reading “authors like the Scottish moralists, the German dialecticians and certain modern economists, political scientists and sociologists who are aware of the basic notion [of aggregation effects]” (Boudon 1986, p. 58). And when he presents a list of eleven different aggregation effects as a demonstration of their ubiquity, which he characterizes as a small number of examples of what is an “indefinite number” that is both ‘difficult and pointless to classify.’

## REASONS FOR OPTIMISM

In summary, Boudon presents a typology of four types of theories of social change, all of which are insufficient for two main reasons. One is that social change is composed of aggregate effects, and a second is that contingency always plays an important role. The question then is whether to treat these elements as limits to inquiry or the most promising direction for exploration and research. If we treat them as the latter, then Boudon has essentially laid out a path for future research.

Boudon argues that social theories are not really theories but instead ‘models’ because they require input – in the form of chance and contingency – to serve as explanations or tools of prediction, for instance to function the way we would want a theory to function. But elements of social change

that Boudon would have written off as chance and contingency can now be explained through theory.

Take, for example, the division of labor. The division of labor is a fundamental social process that has been at the heart of social science inquiry since its origin, appearing as a central concept in the work of Adam Smith (1994) and Emile Durkheim (1996), among others. Understanding the rise and spread of the division of labor can be understood as a central component of understanding the rise of commercial society and, by extension, capitalism. Thus, it is central to theories of social change.

When people engage in a division of labor, they separate out tasks that collectively achieve a common goal. Each person must accomplish their portion of the larger task for the goal to be achieved. For Adam Smith, the larger goal seems to have been to increase the prosperity and wealth of the larger population. For Durkheim, the larger goal seems to have been to provide for the needs of the population. But the division of labor is also applied daily to smaller, more discrete tasks by corporations, universities, government bureaucracies, and even sports teams that assign different roles to different players.

In this sense, it is possible to treat the division of labor as a social coordination problem that can be reduced to a more abstract model or game. In the graph coloring game, which has its roots in cartography, a network is composed of nodes and edges. The object of the game is to color each node a different color from the nodes to which it is immediately connected. This represents the problem faced by mapmakers that wanted to color countries differently from their contiguous neighbors without requiring an infinite number of colors.

The graph coloring game has been shown to support generalization to a large set of coloring games (Dong et al. 2005). A division of labor game needs to capture a slightly different goal, where nodes are not necessarily colored differently from neighbors, but are immediately connected to nodes of the subset of colors that represent the different tasks necessary to achieve a common goal (Erikson and Shirado 2021). So, for example, if a task is split into green, yellow, and blue segments, a green node must be connected to both a yellow and blue node for the task to be accomplished.

This game can then serve as the basis for an agent-based model (Macy and Willer 2002), such as a computer simulation, that captures how the division of labor might emerge and spread within a population. The nodes in the network represent agents, edges represent possible exchange relations, and colors represent task specializations. The agents are incentivized to cooperate when possible, and simulations explore the role of various structural parameters (such as size, density, etc.) in inhibiting successful specializations in the larger population.



The reason why I introduce this example is because it turns out that the number of solutions to the coloring node game can be difficult to predict and is affected by small properties, such as whether there are an even or odd number of nodes in the network or the number of closed cycles and the length of the shortest cycle (Fengming et al. 2011). Changes in these properties, which are likely to seem like minimal adjustments to most people, can therefore have a large impact on the spread of specialization and, additionally, the rise of the division of labor – which again, let me emphasize, is a central social process that has driven industrialization and modernization throughout history.

If we don't know how much network structure matters, then we might interpret the difference in outcomes across the two settings as depending on chance factors. However, it is in fact dependent on a structural condition in a way that is entirely predictable but just happens to be difficult to observe. So that people unaware of the importance of network structure in solving this particular puzzle would very likely chalk up variation in results to unexplained factors of fate and contingency.

If we return to the traditional village example raised by Boudon in chapter 4, it is possible that the pattern of relations in the various villages studied by Epstein (1967) affected the ease with which certain actors in those villages could successfully find exchange partners supplying their full set of needs, allowing then to transition to a more efficient, specialized role within a larger division of labor. If this were the case – and I am at this point only saying that it is valuable to entertain the possibility that it might be the case – then the contingent circumstances that appear to be the result of a very specific and chance configuration of institutions and traditions are actually the result of an invariant structural condition that does have predictable results across times and places. Those conditions, however, were not theorized, measured, or observed at the time that Mead or Epstein published their research.

This example is particular to network structure, but the category of aggregate effects is now much better understood than it was earlier in the last century – although certainly there is still much to learn.

## IMPLICATIONS FOR FUTURE RESEARCH

If we accept this more positive interpretation of Boudon's argument, implications follow for how we should pursue research into social change. Following Boudon, social change results from the unintended emergent consequences of aggregate processes. And secondly, society is a large, complex system with many different interacting and interdependent components that unfold sequentially over and within historical time. The interaction of



these components, as well as the order and timing of those interactions, can independently affect macro-social outcomes (Ermakoff 2015). Effects such as these, which occur outside the level of individual actions, can be challenging to observe, measure, and analyze for individuals. And if they are unintended, those consequences are by definition difficult to predict, as they are the outcomes that individuals do not expect or consider to be ancillary.

Since these emergent and temporal effects are harder to anticipate and observe than other types of cause-and-effect sequences, it makes sense that we need an academic field devoted to understanding and analyzing them. The area of sociological inquiry best suited to studies these effects is, arguably, comparative historical research. Comparative historical research already has a strong legacy of analyzing unintended aggregate effects that unfold within historical sequences. The canonical work being Max Weber's *The Protestant Ethic and the Spirit of Capitalism* (2002), which makes the point that it is often the unintended, second-order consequences unfolding at the scale of the population level that had one of the greatest impacts yet experienced on the course of world history.

Comparative historical research is one of the few ways in which we can understand the chain of actions and consequences that produce large-scale changes in the messy, complex reality of social life as it occurs on the ground. But, along with most of sociology, large theoretical frames are largely eschewed for middle range topics, such as state formation, collective action, and empire. It might be helpful if these meso-level areas of theoretical inquiry were conceived of within a project of emergent aggregate and temporal effects, like unintended consequences.

Certain tools also offer an advantage in analyzing emergent processes, both temporal and aggregate. These tools include but are not limited to computational models, network analysis, large language models (LLMs) for processing historical and archival data, computational models for complex social processes, and a truly global comparative approach to questions of macro-history.

Since the middle of the twentieth century, computational models have been central to understanding and demonstrating the existence of aggregate effects. Tom Schelling's neighborhood model (1971), Mark Granovetter's threshold model of collective action (1978), Watt's small worlds (1999) are all examples of essential formalizations that have led to a deeper understanding of how individual actions are related to larger outcomes such as segregation, revolution, and the diffusion of information. Computational models are the most essential tool there is for understanding how complex interactive social processes unfold over time.

The advantage of network analysis is that networks are crucial to understanding the emergent properties of aggregate outcomes. Social networks are always implicated in large-scale social processes because their structure directly and independently affects the diffusion of ideas, knowledge, information, and resources. The informality and fluidity of networks make them powerful potential agents of change, though they are almost always a secondary consequence of some intended action that has an independent causal effect (Erikson and Occhiuto 2017).

The problem, however, with historical network analysis has always been obtaining systematic network data. That data does exist in the archives, but in incredibly varied formats, like early modern typeface, ancient scripts, handwritten bank notes, or ships' logs. In the past, these records had to be painstakingly translated into a text-readable format. But now, LLMs are showing an amazing capacity to translate and organize these sources into datasets. They can extract and code archival data that records the activities of people in the past in a systematic way (Rolan et al. 2019). This is a great gift for understanding how social change has proceeded throughout human history.

Where these three methods are underrepresented in comparative historical inquiry, global comparative research has been expanding at a faster rate. This expansion is also extremely important to the progress of the field. As Boudon notes, many theories have been wrong because they have treated concepts like modernization and development as more real than material reality. Another way of seeing this, however, is not as a realist trap but as a perspective problem. An example would be the common and erroneous belief in twentieth-century social science that patterns of social change in Europe would automatically set the course of history for the rest of the world. I think it is fair to say that a single-mindedly Eurocentric perspective is going to fail at understanding general principles of social change. But this does not mean that social change cannot be theorized, as per Boudon. Rather, this strongly suggests that that global comparative work is extremely important to identifying what parts of extant social theory relate to specific contexts and which are more general.

## CONCLUSION

In conclusion, Boudon was skeptical of the possibility of developing true theories of social change. Boudon's pessimism was based in his understanding of unintended consequences, which he thought of as a residual category of the unexplainable. If we reframe our understanding of unintended consequences to refer to – or at least include – emergent aggregate and temporal effects like contingency, there is less need for pessimism and more space for progress.

His line of reasoning brings us to a point at which the logical path forward is clearly indicated: systematic analysis of the causes and consequences of unintended outcomes.

Understood in this way, Boudon's book lays a strong case that the tools that help us understand the unanticipated consequences of action will be central to inquiry into social change. Computational models, network analysis, LLMs, and global comparative historical methods are likely to help accomplish this goal. Thus turning some portion of what we have experienced as chaotic into a slightly more tractable area of complexity – and probably some chaos.

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## TEACHING SOCIOLOGY AND THE HISTORY OF SOCIOLOGY

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General sociology and the history of sociology – sometimes called sociological theory – are two subjects that are useful as an introduction to the discipline for both new students and curious minds. In the context of this paper, the term *general sociology* refers to the fundamental concepts, methodologies, and approaches that define the discipline as a whole, without specifically touching on any particular thematic area. Nevertheless, it is inevitable that any such discussion will touch upon a range of themes, whether explicitly referenced in Boudon's work or not. The term *history of sociology* encompasses both the works of authors who preceded and were contemporaneous with the institutionalisation of the discipline, as well as the ongoing evolution of their theories.

Raymond Boudon made significant contributions to both areas, explicitly and implicitly. In the case of general sociology, Boudon's approach challenged the deterministic paradigms that dominated mid-twentieth-century sociology. He emphasised the significance of the perceptions, decisions, and rationalities of individual actors, arguing that these micro-level phenomena could explain macro-social patterns and structures. This perspective diverged from the more structuralist and collectivist orientations of his contemporaries, providing a unique viewpoint from which to examine social dynamics. His contributions

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are key to understanding the use of individualistic proposals in sociology, ranging from perspectives linked to Rational Choice Theory (RCT) to Analytical Sociology (AS). Moreover, he was one of the most prominent theorists on generative social processes. Today, generative explanation theory in sociology is widespread, particularly in connection with computational social sciences. Although Boudon did not use such methods in his writing, relying more on mathematical models, the conceptual development of the idea of generative explanation is found throughout his work.

As far as the history of sociology is concerned, Boudon's studies on classical authors were not so much historical in nature, but rather recognised good practices comparable to those of contemporary sociology. Boudon often revisited empirical studies and theoretical proposals by classic authors such as Alexis de Tocqueville and Max Weber to exemplify how a social phenomenon should be described. As will be seen later, he did not approach these studies to produce a history of sociology but as a way to exemplify explanatory correctness. In fact, references to the classics abound in Boudon's texts, which focus on methodological, epistemological, and theoretical issues.

The chapter is divided into two sections that highlight Boudon's contributions to both fields. The first section focuses on the teaching of general sociology, whereas the second section discusses the teaching of the history of sociology.

## TEACHING SOCIOLOGY

In sociology, there are a variety of approaches to the discipline, including introductory texts, manuals, dictionaries, and treatises. In the initial and intermediate stages of his career, Boudon contributed to the publication of a methodology text in three volumes entitled *Methods of Sociology* (Boudon and Lazarsfeld 1966, 1971; Boudon, Lazarsfeld, and Chazel 1970), *Critical Dictionary of Sociology* (Boudon and Bourricaud 1989), *Traité de sociologie (Treatise on Sociology)* (Boudon 1992), and numerous works on conceptual analysis, including an introductory text (Boudon 1979) and a critical assessment of the state of the art of the discipline (Boudon 1971). Some of Boudon's texts remain valid in the context of teaching sociology. Conversely, others have become somewhat outdated, particularly in light of developments in the field over recent decades. For instance, Boudon's text on the notion of structure (Boudon 1968) was highly pertinent in the discussions about French sociology in the 1960s, but is now a much less-used term.

An examination of the evolution of sociological concepts reveals the value of analysing the *Critical Dictionary*, published with François Bourricaud. The production of such materials requires a standpoint that is not necessarily shared by the entire sociology community. Thus, a certain bias is noticeable in favour of themes such as beliefs and ideologies, political power, rationalist epistemology, and classical authors, which are recurrent in the work of Boudon and Bourricaud. Comprehensively updating the concepts would require a more substantial reference to the vocabulary of areas such as social networks, mechanisms, and contemporary causal analysis. It is remarkable that the way of expressing relationships already resembles current developments in network theory, although there is a notable absence of counterfactual thinking. In the domain of social networks, the entry on diffusion predates the development of models of social contagion and the impact of reticular structures on diffusion processes.

It is worth noting that, in the English edition published seven years later, the publisher removed several of the original concepts. The justification is that some of the concepts had already become obsolete, while others were addressed in greater detail in other sections of the text. Additionally, some terms were omitted due to discretionary decisions, such as the exclusion of the term “models”. This was apparently due to excessive mathematical rigor deemed incompatible with the requirements of a conceptual introduction. Nevertheless, the practice of modelling in sociology has become a hallmark of rigorous approaches and is an integral part of the daily work of social scientists from all disciplines. Indeed, the development of models represents a key aspect of scientific knowledge production.

Regarding the significance of the *Critical Dictionary* as an introductory text to the sociology of the twenty-first century, it can be argued that sociology has undergone significant changes over the past four decades. Thus, concepts such as *structure*, *functionalism*, or *teleology*, which were fundamental in an initial introduction to the discipline forty years ago, are now not so important.

Another conceptual approach in Boudon’s work is the *Traité*, which brings together the contributions of several authors who analyse a series of notions central to sociological knowledge, including *action*, *conflict*, *power*, and *social mobility*. The texts that comprise the work provide concise historical overviews of eleven key concepts. Nevertheless, the publication date is 1992, which makes it more suitable for the study of the recent history of the discipline than for a contemporary introduction to it. A similar phenomenon can be observed in the case of the three volumes on social science methodology (Boudon



and Lazarsfeld 1966, 1971; Boudon Lazarsfeld and Chazel 1970). These volumes bring together contributions by leading researchers in fields such as the construction of indicators and indices, the application of quantitative methods, and the analysis of causality. They also include texts that are now considered classics, such as the study by Coleman and Katz on innovation in medicine. Once more, the update of methodologies locates these volumes within the domain of historical rather than contemporary introductory works.

#### LA LOGIQUE DU SOCIAL AS AN INTRODUCTION TO CONTEMPORARY RIGOROUS SOCIOLOGY

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The text that is perhaps most interesting as a contemporary example of sociological teaching is *La Logique du social* (Boudon 1979). This text presents “*the principles, postulates and objectives of sociological analysis*” from an individualist and rationalist perspective. Consequently, it opens with the rejection of sociology as a science of deterministic and irrational behaviour, instead presenting it as the study of the social phenomena that emerge from human systems of interaction. This definition aligns with the current proposals embraced under the label Sociological Science (see, in particular, Gërzhani et al. [2022] *Handbook of Sociological Science: Contributions to Rigorous Sociology*, hereafter HBSS), where the primary objective is to elucidate the way human actions and interactions lead to aggregate phenomena.

However, the most interesting point of *La Logique du social* is its review of the different systems of interaction and their potential effects, illustrated with classic and modern examples. It confirms and exemplifies the objective of the discipline presented in the introduction. The non-expert reader of sociological literature will find in the text a detailed account of how a sociologist might proceed from the observation of a social phenomenon to its elucidation. Lizón (2007, p. 307) identified this workflow as a core tenet of sociological practice. The text makes extensive use of generative models, which are designed to capture the rational processes of social actors, their decision-making, actions, and interaction with the broader environment – which ultimately leads to the explanation of social facts.

A review of the research programmes presented in the updated HBSS reveals that Boudon’s approaches in *La Logique du social* can be seamlessly integrated into some of these programmes. Specifically, the book is an ideal point of departure to introduce sociological knowledge from any of these perspectives. The programmes in question are detailed in Table 1.

It could be argued that the proposal of Stochastic Network Actor-Oriented models (SAOM) (Sneijders 1996) is least related to *La Logique du social*’s



approach, particularly given that it is based on a network-centric approach, a field in which Boudon did not work. Initially proposed by Snijders, stochastic actor-oriented models are a family of models that aim to elucidate the patterns of evolution of a reticular structure by resorting to dyadic-level processes. Boudon did not use Agent-Based Computational Modeling (ABCM)<sup>1</sup> models, and *La Logique du social* precedes its popularisation within the social sciences. However, the book itself references Schelling’s model of residential segregation as an example of the amplification effect (Boudon 1979, pp. 126-127), and some of Boudon’s models have since been translated into ABCMs (Manzo 2009, 2011; Linares 2014). This is also the case for computational social sciences concerned with the collection of data via online sources. However, both SAOM and ABCM techniques share a fundamental objective with *La Logique du social*, namely, generative explanation (Linares 2014, p. 555).

Table 1: Scientific Programs and the Authors of the Chapters Describing Them in HBSS

| Programme                 | Author/s                                         |
|---------------------------|--------------------------------------------------|
| Population science        | Michelle Jackson                                 |
| AS                        | Gianluca Manzo                                   |
| Rational choice sociology | Andreas Diekmann                                 |
| ABCM                      | Andreas Flache, Michael Mäs and Marijn A Keijzer |
| SAOM                      | Christian E. G. Steglich and Tom A. B. Snijders  |

Boudon postulates in *La Logique du social* that the sociology of social change is dedicated to explaining an emerging phenomenon situated at the level of a system of interaction or interdependence, resulting from the behaviour of agents – but not based on their will, despite the fact that this exists – whose representation depends on a complex theory of action (Boudon 1981, p. 91). The logic underlying SAOM and ABCM is precisely that of a system of interdependence, whereby a series of rules applied to connected agents generate an aggregate result.

RCT, as exemplified by *La Logique du social*, seeks to elucidate macro-level phenomena through the analysis of the aggregation of purpose-oriented behaviour. It is acknowledged that Boudon was critical of RCT as a general theory, proposing an “ordinary rationality theory” which subsumes it. In *La Logique du social*, he presents concrete models that include the presuppositions of RCT, including the relative frustration model, which was itself mentioned by

1     ABCM aims to identify whether, and if so, how and under which conditions precisely, the theoretical assumptions a researcher makes about the interactions between interdependent individuals allow one to generate a social outcome (Epstein 2006).

Diekmann (2022). The elements of interest that connect *La Logique du social* with rational choice sociology are as follows: first, that rational decisions are dependent on the context of interaction; second, that the relative frustration model demonstrates this; and third, that the results of rational actions do not always coincide with the will of the actors. In a second case, an example from *La Logique du social* links the activity of American trade unionism with increased productivity in companies (Boudon 1981, p. 65). Given the bargaining conditions between companies and unions in the United States, unions tend to focus their activities on the most dynamic companies. From the perspective of these companies, the only rational strategy is to accept wage increases. The objective of the union is then to target the least dynamic companies. To achieve this, the unions must implement processes to improve the companies' performance. This results in greater productivity and enables the companies to remain competitive. It can be observed that an increase in union activity is correlated with an increase in productivity, despite the fact that the objective of this activity is wage increases.

In his eponymous work, John Goldthorpe (2017) popularised the concept of "sociology as a population science". This text contains numerous references to Boudon, particularly in the context of justifying methodological individualism as a research strategy. In the corresponding chapter of the HBSS, it is established that the three fundamental principles of sociology as a population science are its commitment to scientific rigour, its attention to regularities at the population level (macro-level phenomena), and the great significance placed on the descriptions of phenomena prior to their explanation, which is the main objective. As with the other programmes presented, sociology, as a population science, employs mechanisms at the micro level to explain macro-social regularities, thereby sharing the objective set out almost 40 years before in *La Logique du social*. Furthermore, Boudon's general work serves as a source of inspiration for sociology as a population science, as Goldthorpe argued in his recent *Pioneers of Sociological Science* (Goldthorpe 2021).

Finally, the connections between AS and Boudon's work are perhaps the most pronounced, as evidenced by the author's contribution to its foundational text, *Social Mechanisms* (Hedström and Swedberg 1998), and his subsequent recognition as a precursor to the programme in other works (Hedström 2005). *La Logique du social* reflects and exemplifies the principles of AS in its pre-computational stage, with an extensive use of micro-social models to explain macro-social phenomena.

This concise review showed the clear alignment between Boudon's sociological approach, as presented in *La Logique du social* (Boudon 1981), and various contemporary forms of scientific and rigorous sociology. The book

serves as an introductory text for those teaching this subject, as it also contains pertinent references to seminal works in sociology. In *La Logique du social*, the novice student will encounter the foundational principles of a scientific sociology with cumulative ambitions, as well as the primary goal of sociologists as constructors of models and explanatory theories. These foundations have served in recent years to configure a series of programmes that, despite their specificities, share both a common objective and epistemological language. This unifying language is explicitly present in *La Logique du social*, thereby justifying its relevance today as an introductory text to the field of sociology.

## TEACHING THE HISTORY OF SOCIOLOGY

Boudon's entire body of work is full of references to the foundational texts of sociology and other social sciences. Quotations from Adam Smith, Tocqueville, and Weber are frequently cited by Boudon in his defence of the individualistic programme and cognitive sociology (for instance, Boudon 1998b). Indeed, Boudon uses the acronym TWD (for Tocqueville, Weber, Durkheim) to designate his theoretical framework as *the sociology that really matters* (Boudon, 2002). However, his most significant contribution to the study of the classics is possibly the *Études sur les sociologues classiques* compendium (Boudon 1998a, 2000). This two-volume work is interesting for two reasons. First, the fourteen studies dedicated to nine classical sociologists could form the basis of a course on classical sociological theory. Second, the two introductory essays to the volumes and the concluding essay provide an excellent reflection on the different ways to present the history of the discipline.

Boudon's presentation of the history of sociology is rooted in a rationalist perspective, as he explicitly states (Boudon 1998a). A comparison with other celebrated works of classical sociology reveals a distinct divergence in approach. In contrast to the approaches used by Aron (1967) or Randall Collins (1994), for example, Boudon rejects the doxographic and unifying perspective of social science. For Boudon, the doxographic method is flawed because it prioritizes understanding what authors really thought over evaluating whether their claims are true or false (Boudon 2000, p. 64).<sup>2</sup> As a result, doxography tends to treat authors as particular cases within established schools of thought or intellectual systems.

2 For a detailed discussion on the use of the term *doxography*, see Mansfeld and Runia (2004) in *The Stanford Encyclopedia of Philosophy*: <https://plato.stanford.edu/entries/doxography-ancient/>, accessed on July 7, 2025.

Despite its limitations, the doxographic method offers certain advantages, including the ability to highlight the distinctive characteristics of a tradition or the discipline as a whole, to propose a certain evolution in the sciences, and to establish a canon. In sociology, there is a canon of established works, including those of Aron, Collins, and others, such as Ritzer's (1992). This canon includes several *pioneers*,<sup>3</sup> typically French and British figures from the Enlightenment, including Condorcet, Montesquieu, Ferguson, and Smith. In certain instances, however, the canon extends back to figures such as Ibn Khaldun or Machiavelli. The *founders* of the discipline are post-Enlightenment figures, including: Comte, widely regarded as the inventor of the term sociology; Tocqueville, whom Elster regards as the first social scientist (Elster 2009); and Marx, who is recognised as a versatile figure and can also be included in the following generation. The most well-known among the *institutionalisers* are Weber and Durkheim, who are typically accompanied by Pareto, Tönnies, Simmel, and Mead (if American sociology is mentioned). If we consider traditions instead of generations, we will probably find the following: The *positivist-functional*, the *conflictivist-dialectical*, the *rational-utilitarian*, and the *interactionist/micro*.

However, the decision to adopt a doxographic approach causes certain difficulties, particularly regarding the need to include all canonical sociologists in some of the generations and traditions. This is highlighted by Boudon (1998a, pp. 7-16), who adopts a Popperian viewpoint. His intention is to identify the enduring aspects of the classics when subjected to theoretical and empirical scrutiny, although in a less rigorous manner than that employed in the natural sciences. This allows for the creation of a catalogue of classics, focusing on the parts of his work that remain relevant, rather than his entire body of work. In any case, it is not reasonable to view the *Études* list as a definitive or exhaustive account of the sociological classics, given that it only encompasses a select set of fields and themes. Moreover, it does not claim to represent the overall scope of sociological knowledge, either in the past or in the present.

The most significant aspect of the *Boudonian* approach is the methodology used to convey the historical evolution of the discipline to the reader. A historical reassessment, similar to that conducted by Boudon in *The Crisis of Sociology* (1971), reveals that the current state of the discipline is not significantly different from its condition at that time. It also exhibits that there is still a set of scientific programmes that appear to be in a state of mutual

3 The distinction between pioneers, founders and institutionalisers is present in Lamo de Espinosa (2001) which is something that neither the pioneers of the 18th century nor the great creators of the 19th century (from Comte to Spencer, without forgetting Tocqueville or Marx).

incomprehension. From this position, Boudon's decision to adopt a rationalist perspective remained consistent over time. In line with this perspective, our goal is to demonstrate that an effective approach to teaching the history of a discipline involves selecting established pieces of knowledge and tracing their genealogy within a historical context. This approach largely informed Boudon's work in *Études* and other publications.

#### AN EVALUATION OF *ÉTUDES*

A close examination of the fourteen<sup>4</sup> texts that comprise the two volumes of *Études* reveals several points of particular interest. The initial observation is a tendency towards the prevailing themes in Boudon's body of work, namely the sociology of beliefs and values. Eight of the fourteen texts address beliefs as a central theme, specifically 1.6, 2.2, 2.3 and 2.4, while the remaining texts also touch upon beliefs as a recurring issue. Similarly, values are a recurring theme in the texts, with references to them in 1.2, 2.2, 2.5 and 2.8. Likewise, *Études* includes a substantial number of chapters dedicated to methodological and epistemological approaches, with up to five chapters (1.3, 1.4, 1.5, 1.7 and 2.6). The history of sociology is full of disputes and clarifications between and within traditions. An exemplary case is that of methodological individualism, which Boudon extensively defended, and which remains a subject of significant debate in the field today. Tocqueville's examination of social power, at the beginning of the book, does not exclusively focus on the aforementioned themes but rather revolves around the concept of collective beliefs and opinions.

Regarding the authors present in the work, a recurring element in the book is the Durkheim-Weber binomial, which appears both separately and in a chapter that appears to compare the two. One of Boudon's obsessions in working with the classics was to emphasise the relevance of both authors. In the case of Durkheim, Boudon highlighted the explanatory power of his empirical studies – even though Durkheim's theoretical and methodological guidelines did not always align with the way he conducted his own research (Boudon 1998a, pp. 93–136). Smith and Tocqueville appear as authors with great intuition, although they wrote at a time before the institutionalisation of the social sciences. The other authors, with the exception of Lazarsfeld, wrote between the end of the nineteenth and the twentieth century; the appearance of Tarde and Scheler is of note because they do not usually appear in the canonical histories of classical sociology.

4 Omitting the introductions and conclusions, and the appendix with the inaugural speech of Émile Durkheim Street in Paris.

Table 2: Index of Chapters in *Études: Volumes I and II*

| Volume 1      |                                                                                                           | Volume 2                                                    |                                                                                   |
|---------------|-----------------------------------------------------------------------------------------------------------|-------------------------------------------------------------|-----------------------------------------------------------------------------------|
| Avertissement |                                                                                                           | Introduction. Convergences entre les sociologues classiques |                                                                                   |
| 1             | Le pouvoir social : variations sur un thème de Tocqueville                                                | 1                                                           | Adam Smith : Le « spectateur impartial » et l'acteur partial                      |
| 2             | <i>L'Éthique protestante</i> de Max Weber : le bilan de la discussion                                     | 2                                                           | Émile Durkheim : L'explication des croyances religieuses                          |
| 3             | Durkheim et Weber : convergences de méthode                                                               | 3                                                           | Georg Simmel : Facteurs sociaux de la connaissance                                |
| 4             | <i>Should one still read Durkheim's Rules after one hundred years?</i>                                    | 4                                                           | Vilfredo Pareto : Rationalité ou irrationalité des croyances ?                    |
| 5             | <i>Les problèmes de la philosophie de l'histoire de Simmel</i> : l'explication dans les sciences sociales | 5                                                           | Max Weber : La « rationalité axiologique » et la rationalisation de la vie morale |
| 6             | Le phénomène idéologique : en marge d'une lecture de Pareto                                               | 6                                                           | Gabriel Tarde : La connexion micro-macro                                          |
| 7             | « L'analyse empirique de l'action » de Lazarsfeld et la tradition de la sociologie compréhensive          | 7                                                           | Max Scheler : Contextualité et universalité des valeurs                           |
| 8             | Appendice : Discours à l'occasion de l'inauguration de la rue Durkheim à Paris, 7 décembre 1996           | 8                                                           | Comment écrire l'histoire des sciences sociales ?                                 |

The case of Lazarsfeld deserves special attention. First, it should be remembered that he was one of the masters and co-authors in the first stages of Boudon's career. At the same time, he was involved in significant publications with James Coleman and Robert K. Merton. However, Lazarsfeld is usually absent from the sociological history canon. This is partly explained by his lack of a *system*, an element that prevails in the doxographic approach to the history of sociology. Nevertheless, as Boudon points out, he published significant reflections on epistemological issues (see e.g. Lazarsfeld 1966). A popularised view of Lazarsfeld as a defender of atheoretical empiricism – first propagated by some *Frankfurtians*, then by Wright-Mills, and later by authors such as Bourdieu – partly clouds Lazarsfeld's contributions and his consideration as a classic author in sociology.

The list of authors and themes present in *Études* is far from exhaustive. Some notable absences are Comte, Marx, and Spencer. The reason given by Boudon (1998a, pp. 7-16) is that their work is characterised by production with totalising pretensions, a theory capable of explaining all phenomena, in which the concern for internal coherence ends up burdening some of the interpretations and explanations provided. In contrast, authors such as Weber and Durkheim based their empirical work on the description of specific phenomena, sometimes even overriding the rules they had previously developed in theoretical and methodological texts. Indeed, in terms of generations, Marx

and Comte are closer to Tocqueville and Smith than to the institutionalists of the late nineteenth century.

AN EXAMPLE OF THE GENEALOGICAL APPROACH  
IN BOUDON'S WORK: ON RELATIVE FRUSTRATION

The relationship between frustration and opportunity was analysed by Tocqueville (2011 [1856]) in his study of the Ancien Régime. *Tocqueville's paradox* applies to the situation in which an environment of growing opportunities tends to correlate – counterintuitively – with higher rates of frustration in the population. What Tocqueville proposed as a prolegomenon to the French Revolution has become one of the best-founded theories of sociological knowledge in the form of Boudon's model of relative frustration. De Tocqueville's (2012 [1840]) original description referred to the phenomenon where an increase in the probability of social advancement and enrichment correlated with higher levels of general dissatisfaction.<sup>5</sup> Durkheim followed a similar process in his theory of anomie, but it was not until the publication of *The American Soldier* (Stouffer 1949) that the structure of frustration was more rigorously contrasted using quantitative data.

The finding that soldiers in US Army units with fewer opportunities for promotion had higher rates of satisfaction than those in units with greater opportunities raised a sociological question of the first order. One of the first answers to the question of frustration was given in connection with the idea of the “reference group” (Lazarsfeld 1949; Runciman 1961). The basic connection between the two is expressed through the idea that frustration is not absolute, but is limited to the fact that the possession of a good  $x$  by a member of  $A$  produces certain feelings in a subject  $i$  because he belongs to  $A$ . Something that would not happen if  $i$  belonged to  $B$  or if the owner of  $x$  belonged to  $B$ .<sup>6</sup>

But it was Boudon who completed this argument, first by arguing that the phenomenon described by Tocqueville, although following a similar pattern to that of *The American Soldier*, occurred in a context where the “reference

5 “No inequality, however great, offends the eye when all conditions are unequal; while the smallest dissimilarity seems shocking amid general uniformity; the sight of it becomes more unbearable as uniformity is more complete. So, it is natural that love of equality grows constantly with equality itself; by satisfying it, you develop it” (Tocqueville 2012 [1840], p. 1203).

6 “The notion of relative deprivation implies that people do not suffer in an ‘absolute’ way; they compare their lot with that of other people of their kind” (Lazarsfeld 1949, p. 388).



groups” were diffuse or as large as a social class. Second, by developing an insightful model in which the structure of competition is more decisive than the effect of the group (Boudon 1981, pp. 116-127). The model in question, in its simplest version, takes the form of a lottery where there are  $n$  prizes less than the number  $N$  of group members, in which one can participate at a cost  $c$  or not participate. If each prize has a value  $b$  ( $>c$ ), then the expected utility of not participating is 0, and the expected utility of participating is defined as follows:

$$U(\text{participate IF } n < x) = \frac{n(b-c) + (x-n)(-c)}{x}$$

$$U(\text{participate IF } n \geq x) = b - c$$

Where  $x$  is the number of participants. All other factors being equal, the value of  $x$  represents the variable relative to opportunities, and the expected utility of participation grows with it. So, why does a factor that increases expectations of improvement as it grows correlate with an increase in frustration? The key is how the increase in opportunities leads to an increase in individual expected utility. The greater the number of prizes, the greater the chances of obtaining  $b-c$ , which leads to an increase in participation because of a higher expected benefit.

Once the lottery is over, the level of frustration depends on the relationship between the *winners* (those who bet and got  $B-C$ ) and the *losers* (those who bet and got  $-c$ ). For the losers, the winners become members of their reference group to the extent that they have made the same investment. The perception of injustice is palpable, because in a lottery it is luck that determines who occupies each position, so seeing oneself as a loser leads to a state of frustration. Since the specification of Boudon’s relative frustration model, its basic hypothesis has been experimentally tested in its canonical form (Berger and Diekmann 2015; Otten 2023; Berger, Diekmann and Wehrli 2024) and formalized into simulation models (Manzo 2009, 2011). Additionally, the model has been formalized into simulation frameworks (Manzo 2009, 2011), reflecting the present-day relevance of the topic and Boudon’s model.

Analysing the path taken by relative frustration theory from de Tocqueville’s initial approach to the present, the process has progressed through the following stages: description; intuitive explanation; modelling; and finally, successive empirical verification. The first stage is common to both the phenomenon of frustration described by Tocqueville and that described by Stouffer: a puzzling social phenomenon is discovered, and its explanation is presented as mysterious. Later, tentative explanations are proposed for the phenomenon in question, drawing on existing theories or generating new ones.



At the same time, other similar phenomena are discovered, and their study follows a similar pattern. Subsequently, several phenomena are found to share a similar process, despite differences in context. At this point, an attempt is made to unify the explanation for all of them by pointing to common mechanisms or by generating a model that allows us to understand several phenomena with a similar causal pattern. Once such a model has been specified, it is tested in other situations to check its explanatory potential and to specify its components. This scientific procedure frequently occurs in Boudon's texts on methodology and epistemology, as well as in his evaluations of the classics as inspiring useful contemporary theories.

Thus, from a rationalist perspective, the genealogical approach is the most suitable one for teaching the history of sociology. On the one hand, it complies with the maxim of presenting the accumulation of knowledge through the explanation of enigmatic phenomena as the objective of the discipline. On the other hand, it fits into the mechanistic approach to explanation. Within this approach, the generation of middle-range theories helps to produce new explanations, and also helps to unify a causal language under which the efforts of researchers can be combined. It also highlights the contributions of the classics as precursors of both contemporary theories and a style of theorisation based on the principles of cognitive sociology (Boudon 2002).

Added value is provided through the fact that sociology, or at least part of it, is presented as a science unified by objectives and a common language. It is also in dialogue with other related disciplines, such as cognitive science, economics, political science, and demography. Researchers from these and other sciences work under the premises of scientific rationality, methodological rigor, and the accumulation of knowledge, thus awarding meaning to the historical development of the social sciences from a rationalist and genealogical position.

#### WHAT ABOUT EPISTEMOLOGICAL AND METHODOLOGICAL DISPUTES?

The history of sociology is not just a history of key findings. This is mainly due to two facts. On the one hand, many activities carried out under the label of "sociology" are not motivated by scientific goals. On the other hand, disputes over methods, objects, and approaches have occupied pages and pages of sociological heritage. Boudon's rationalist proposal – and the genealogical proposal – also includes the teaching of certain practices that have made sociology what it is today. In the *Études* themselves, we find analyses such as Simmel's philosophy of history or Lazarsfeld's theory of action, which do not have an empirical aspect, although they have contributed in various ways to guiding empirical work. In this second example, the influence of Lazarsfeld's

concept of “action” is manifest in his own work, as it is in the Columbia School, in which methodological individualism and empirical analysis of action were signs of its identity.

Boudon’s characterisation of sociology in *The Crisis of Sociology* is that of a discipline without a general agreement on key issues: from the absence of a common language, to the inability to determine whether the discipline’s aim is to discover truths, to produce descriptions, or to serve as a political tool. Years later, when he outlined his ideal type of sociology in the *Études*, little seemed to have changed. Despite the fact that sociology is a multi-paradigmatic discipline, some of its formulations have come close to the rationalist goal of creating a common language and, above all, of generating established and useful knowledge.

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To illustrate this, we can consider the explanatory syntax proposed by AS (e.g. Hedström 2005, Manzo 2014, León-Medina 2017). Scholars agree that the process begins with identifying a pattern at the population level, designated the “explanandum”. This pattern must then be elucidated based on the entities, relationships, and activities that constitute it at a microsocial level through the utilisation of a generative model. A multitude of assumptions are placed within this concise delineation, many of which have been the subject of considerable debate at an epistemological level. These include the notions of causality, methodological individualism, explanation by generative mechanisms, and micro-macro transition.

In this case, we may choose to follow a genealogical strategy to delineate the historical path that has constituted one of these elements as a fundamental element of the analytical approach, for example, explanation using mechanistic models. We may begin with the classics, since it has been demonstrated that authors such as Tocqueville and Weber employed mechanistic explanatory models in their empirical research. They did so despite the fact that this was not an explicit methodological principle. Subsequently, Robert K. Merton employed analogous concepts in his delineation between medium-range theories and his empirical studies. In doing so, he anticipated what Fararo (1969) and Boudon (1979) would later formalise at a theoretical level. Between the 1970s and 1990s, the term *explanatory mechanisms* was employed in a variety of fields within the social sciences, as well as in the fields of biology and the philosophy of science.

The fundamental work prior to the widespread integration of the theoretical concept into empirical research is the publication of the compendium of essays *Social Mechanisms* (Hedström and Swedberg 1998). Discussions about the relevance of mechanism-based explanations in sociology have taken place in the 25 years since its publication, but a contemporary assessment shows

that their application has been successful (Manzo 2021 is useful. As in the previous examples, learning about the history of sociology is marked more by contemporary practice than by the doxographic interest that the discussions may have had when they first occurred.

## CONCLUSION

Boudon, one of the most influential sociologists of the twentieth century, has left a profound impact on both general sociology and its historical evaluation. His individualistic approach and contributions to generative explanations have been crucial in understanding the logic of the social and the enduring relevance of classical sociological studies. Rather than focusing on social structures or their functions, Boudon places an emphasis on individuals and their actions. From his perspective, understanding social phenomena necessitates an analysis of individual decisions and behaviours, which aggregate to produce a broader social impact.

This approach has facilitated a more detailed and nuanced understanding of phenomena such as social mobility, inequality, and collective beliefs. One of the most innovative aspects of Boudon's work is his emphasis on generative explanations. Unlike traditional causal explanations that seek to identify direct determining factors, generative explanations focus on the processes through which social phenomena are generated. This type of explanation enables a comprehension of how individual actions can lead to complex and emergent social patterns.

*La Logique du social* is one of the most significant works in this regard, where Boudon articulates his individualistic and generative approach in a comprehensive manner. This work has been fundamental to contemporary sociology, as it offers a robust theoretical framework for analysing how individual micro-processes translate into macro-social outcomes. *La Logique du social* not only provides detailed and convincing explanations of various phenomena, but also challenges sociologists to reconsider their methods and approaches, promoting a more rigorous and detailed analysis of individual action. Notably, we suggest that *La Logique du social* be used as an appropriate starting point for engaging with contemporary proposals such as those expressed in Historical Social Science.

In addition to his theoretical contributions, Boudon has made significant contributions to the history of sociology. His studies on classical sociologists, such as Max Weber, Émile Durkheim, and Alexis de Tocqueville, are not confined to a historical analysis of their works, but aim to highlight the ongoing relevance of their approaches and theories. Boudon argues that many of these

thinkers' ideas remain pertinent and useful for understanding contemporary social phenomena. Boudon does not aim to historicise the works of these sociologists; instead, he seeks to demonstrate how their approaches can be applied and adapted to contemporary contexts. This perspective has been crucial in keeping the sociological tradition alive, bringing together historical analysis and theoretical insight, and demonstrating the continuity and evolution of sociological thought.

Boudon's oeuvre continues to be an indispensable point of reference for contemporary sociologists, offering theoretical and methodological tools to analyse the complexity of social life. His work exemplifies the incorporation of rigorous individual-level analysis into broader social theory, thus providing a comprehensive framework that remains highly relevant in the field of sociology today.

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## BOUDON'S LEGACY FROM A TEACHING PERSPECTIVE

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From the perspective of sociology of education, university teaching remains, in industrialized contemporary societies, the privileged means of transferring the knowledge regarded by a given generation of scholars as the most valuable and advanced for the cognitive and practical training of the next generation (Brint 2017, chs. 1, 2). One way to address the question of one author's legacy thus is to ask whether they should be included in the syllabus of a university class, and, if so, what aspects of their work should be presented to students. I will follow this approach to reflect upon Boudon's legacy in this chapter.

In particular, I imagined an institutional setting in which I was given the opportunity to design a two-semester introductory course in sociology for first-year Master's students that had to meet the three following constraints: first, the course's main goal must be to provide practical guidelines on how to design sociological research; second, the course's secondary goal must be to help students to think about the current state of sociology as a discipline; and third, at least two-thirds of the course's reading assignments must rely on Boudon's oeuvre. Such an imaginary setting put me in a moral-dilemma-like situation regarding my sense of responsibility as a teacher. This responsibility compels me to select the best pedagogical resources for students, but may collide with my sense of loyalty to the authors of the past generations that I admire, given that the desire to show respect is a possible bias in one's capacity to honestly judge the relevance of those authors. Therefore, the question I had to solve was: Did I really believe in the possibility of finding enough material in Boudon's scientific production that was still worthy to be presented to a new generation of students in sociology, or would I have to conclude that Boudon's works seemed to me too outdated by recent developments in contemporary sociology to include in such a course?

After recursively examining Boudon's earliest works and his latest writings, I convinced myself that a selection of them can still support the design of a thought-provoking syllabus for a Master's-level introductory course to

sociology that I believe worthy to be taught. In particular, the course would be composed of three sets of lectures, which I will call *modules* hereafter, and that could be respectively titled “Research puzzles”, “Research heuristics”, and “Research quality”. In the following three sections of this chapter, I briefly explain the goal and the content of each module; an overview of the corresponding potential syllabus is provided in Appendix 1.

352 As a final preliminary remark, I would like to draw the reader’s attention to a caveat. While I will justify the selection of Boudon’s writings assigned within each course’s module, I do not claim that the choices that I made are the only possible ones. In particular, based on the observation that many students today are reluctant to read extensively, I prioritized short over long reading assignments, thus selecting Boudon’s articles and book chapters rather than entire books. Moreover, given the space limitation, my imaginary setting allowed me to design only a single, two-semester course with specific goals. I have therefore excluded Boudon’s pieces of work on classics (for the possible teaching value of which, see Sanantonio and Miguel’s chapters in this book). Therefore, my only claim is that the proposed syllabus seems a reasonable and defensible starting point for the design of an introductory course to sociology that would still benefit a new generation of students. Variations and modifications of it are certainly possible and would be welcome.

## RESEARCH PUZZLES

According to the first requirement of the imaginary setting I have described in the introduction, the course to be delivered had to equip students with the capacity to design sociological research. The module “Research puzzles” proposes to meet this requirement by explaining to students how Boudon replied to well-defined counterintuitive *why*-questions concerning specific substantive phenomena, i.e., what we may call “puzzles” (see Gambetta 1995). This module comes first because dissecting the details of specific pieces of empirically-oriented research that one regards as successful from both a substantive and methodological point of view seems to be an effective way to introduce students to sociology. In a posthumously published article, Boudon (2014, p. 43) appeared himself to share this pedagogic principle:

Every scientific puzzle is unique, so that it requires scientific imagination to solve it. A practical consequence of this is that the best way to teach the complexity of the micro-macro link problem is to expose sociology students to examples where the problem has been successfully solved. They will learn then that the question as to “What is context?” has actually no general answer,

but answers specifically adapted to the challenging macroscopic puzzles the sociologist wants to disentangle.

Among the number of *puzzles*, both at the micro- and macro-level, that populated Boudon's writings, the six *why*-questions that follow received answers characterized by a particular degree of systematicity and elaboration:

1. *Why* did the frequency of judges deciding to discontinue a case before them in court ("affaires classées sans suite") increase in France between 1831 and 1950 (Davidovitch and Boudon 1964)?
2. *Why* do actors with high social background tend to make more ambitious educational choices compared to actors with low social background, even when they have similar grades (Boudon 1973, ch. 4)?
3. *Why* might an increase in the number of highly-educated individuals not lead to a proportional increase in the rate of absolute intergenerational social mobility (Boudon 1973, ch. 8)?
4. *Why* may the fraction of unhappy actors initially increase, despite the fact that the number of available places that provide access to certain goods expands (Boudon 1977, ch. 5)?
5. *Why* were French students, compared to students in other countries, so massively in favor of protesting in May and June 1968 (Boudon 1971a)?
6. *Why* do highly-educated citizens tend to have markedly different levels of tolerance to moral and behavioral diversity compared to low-educated ones (Boudon 2002a)?

Students may benefit from being exposed to the machinery of how Boudon replied to these six research questions for three reasons. First, as suggested by the abundant secondary literature on questions 2 and 3 (see, for instance, Breen's chapter in this book), they raise the problem of whether or not the explanandum was properly established by Boudon, and, if so, whether or not the puzzling character Boudon assigned to a given explanandum is really or not. Therefore, in this respect, the heuristic value of the selected research examples is to make students think about what "establishing a phenomenon" (Merton 1987) means, and what a research question worthy of interest is (Martin 2017, ch. 2).

Second, the six research examples selected allow us to illustrate a variety of ways to test hypotheses. In particular, questions 1 to 3 refer to data-oriented and formalized explanations. The answers provided by Boudon rely on hypotheses formalized through mathematics or algorithms (for more details, see Sage's

chapter in this book), and these tools are then used to derive hypotheses' consequences and connect these consequences to specific datasets. In this sense, the proposed hypotheses were verified with a clear methodological apparatus that can be inspected (see Breen's and Birkelund's chapter in this book). The explanation for question 4 refers instead to stylized-fact-oriented but formalized explanations. The answer provided by Boudon still relies on hypotheses that are formalized through mathematics, namely game theory (see Raub's chapter in this book), so that checks and replications are still possible. However, the hypotheses are developed to account for a class of empirical patterns rather than a specific dataset. Boudon (1996, p. 63, 65) classifies his answer to question 4 as a "model", which he defined, with respect to this specific piece of work, as a theory explaining a set of "heteroclite", his own word, phenomena rather than one set of particular empirical observations. Finally, the answers Boudon provides to questions 5 and 6 refer to data-oriented, informal explanations: they have the ambition to connect hypotheses to specific data but the hypotheses are only formulated as qualitative narratives. In this sense, Boudon admits himself that these explanations are more "hypothetical causes" (Boudon 1971a, p. 148) or "conjectures" (Boudon 2002a, p. 43). As a consequence, the heuristic value of the selected research examples is to force students to reflect upon the status of a given explanation depending on how the connection between hypotheses and empirical data is implemented.

Finally, but related to the previous point, the third learning benefit that I see in dissecting the six selected research examples is that they allow one to raise the more general question of what a "good" explanation is. For instance, as the secondary literature on the answer Boudon provided to question 4 suggests (see Berger et al.'s chapter in this book), it can indeed be argued that these explanations need to be revised. At the same time, the need for revision suggests that there is *something* to revise. This *something* may well be a new mechanism nobody has thought about before. Again, this was the case for question 4 as Gambetta (1998, Table 5.1, and p. 117) correctly noted. In other words, the explanations at hand pinpoint something new. They had the capacity to lead the observer to see the social world otherwise, meaning by thinking about a possible social mechanism that we did not see before we considered the proposed explanation. The capacity to trigger an observer's curiosity, thus leading them to further investigate the proposed mechanism, seems to be a property of good explanations. The six selected research examples thus also have the pedagogical virtue of forcing students to reflect upon what makes an explanation worthy of its name.

## RESEARCH HEURISTICS

Teaching by research examples means examining the details of how things are done in practice, rather than discussing the general principles behind the practice. However, at least at the earlier stages of a sociological training, and in particular given the number of philosophy-minded students that enter our Master's programs in sociology, providing a systematic discussion of general principles for designing sociological research also seems an important step in their training. Thus, while the course's first module on "Research puzzles" focuses on the substantive phenomena to be explained *and* on the substantive content of the explanations proposed by Boudon to questions 1 to 6 (see section 1 above), the "Research heuristics" module is about the *modus operandi* of the six pieces of research associated to these questions. Although to a different extent, these pieces indeed share a common set of working principles. I recognize five of them, plus a sixth insight whose heuristic value deserves special attention. The proposed syllabus (for an overview, see Appendix 1) suggests devoting a lecture to each of these ingredients; in addition, students are invited to "discussion" breaks where they can reflect upon some of the existing debates on the identified working principles.

## GENERATIVE MODELS

The first, and most general, principle that is transversal to the six pieces of research discussed in the "Research puzzles" module poses that explaining requires building a *generative model*. According to Boudon, a generative model is a set of hypotheses that allows to understand a statistical structure as a consequence of those hypotheses – in his own words, "... a theory containing two logical core elements: first, a description of the logic postulated to regulate the actions of the individuals observed in a survey or some other kind of observation from which quantitative data are derived; and second, a description of the social constraints within which the logic of individual action develops" (Boudon 1979a, p. 52). The heuristic value of exposing students to this principle is to make them familiar with the idea that explaining an empirical observation requires being specific about the details of the mechanisms that are likely to be responsible for the observation (see also Hedström's and Stolz's chapters in this book). With respect to this first principle of thinking through generative models, the lecture's discussion break will provide a forum to raise the question whether Boudon really was at the origin of this notion, and, if not, from whom he may have borrowed it (see Manzo 2024).

The remaining four working principles are in fact principles that provide more specific instructions on how to design (principles 2, 3 and 4) and to study (principle 5) a generative model. They outline the building blocks of a generative model and provide guidance on how students can deduce logical consequences from their combination.

#### THE NOTION OF *RATIONALITY*

356 In particular, the second working principle concerns the micro-sociological moment of the model building process, i.e. the first element (the “actions”) of Boudon’s above-mentioned definition of a generative model. It is about what Boudon (2010, p. 18) lately called “cognitive equilibrium principle” stating that “people believe that *X* is true, acceptable, good, legitimate, etc. as soon as they have the feeling that *X* rests upon a set of acceptable reasons”. This is the basic principle behind what Boudon initially called “subjective” (Boudon 1989), then “cognitive” (Boudon 1996), and, ultimately, “ordinary” *rationality* (Boudon 2012a), a model of actors that he asserts is able to explain all types of beliefs behind actors’ choices, whether these beliefs are positive or normative (Boudon 2014). The pedagogic value of exposing students to this principle is to make them reflect upon the actual possibility of opening the black box of an actor’s mind as well as upon the conditions under which doing this is necessary to achieve explanatory depth (see Hedström’s and Esser’s chapters in this book). With respect to the notion of rationality, the lecture’s discussion break then raises the question of the extent to which Boudon’s specific model of actors is defensible (see Opp 2014; see, also Demeulenaere’s chapter in this book).

#### INTERDEPENDENCY STRUCTURES

The third working principle shifts the focus to the second element, i.e. the “social constraints”, of Boudon’s above-mentioned definition of a generative model. It emphasizes a particular type of constraint, namely the interdependency among social actions (see Boudon 1979b, ch. 4), which the early Boudon understood as a central driver of *reproductive* (Boudon 1979b, ch. 5), *cumulative* and *transformative* processes (Boudon 1979b, ch. 6). The pedagogic value of exposing students to this principle is to push them to think about society as “complex entanglements of systems of interaction”, in Boudon’s (1979b, p. 113 [Eng. trans.: 1981, p. 56]) own words, thus forcing them to the mental gym of considering the possibility that a given macroscopic pattern may arise as an unintended effect of how actors impinge on one another (on the notion of “perverse effect”, see Boudon 1977, pp. 5-15 [Eng. trans.: 1982,



pp. 1-10]). With respect to the principle of taking seriously interdependency structures, the lecture's discussion break then raises the question of the extent to which Boudon has progressively paid more attention to actors than to interdependency among them,<sup>1</sup> and, on the other hand, whether he actually always focused more on "parametric" forms of interdependency rather than on interdependency embedded in dyadic and higher-order interactions – a comparison with Granovetter (1978, 1983) is proposed here to students.<sup>2</sup>

#### THE MICRO-MACRO LINK

The fourth principle concerns the relationship between the elements (1), i.e., the actions, and (2), i.e., the "social constraints", of Boudon's above-mentioned definition of a generative model. The principle invites the modeler to do their best in connecting the micro- and the macro-levels of analysis recursively, which Boudon (1981, p. 46) nicely expressed with the formula  $M=M\{m[S(P)]\}$ . It states that a given phenomenon to be explained ( $M$ ) can be seen as the outcome of actions ( $m$ ), which are themselves the outcome of the social environment of the actors ( $S$ ), which is itself the outcome of higher-level macro-sociological variables ( $P$ ). One of the added values of exposing students to a discussion of this principle is to make them aware that the usual presentation of the methodological individualism according to which only individuals' actions matter to explain a given social phenomenon only partially corresponds to the way Boudon's defined this perspective (see also Bulle's chapter in this book). With respect to the micro-macro principle, the lecture's discussion break then

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- 1 On this point is telling to compare Boudon's (1979a, pp. 51-60; 1977 [Eng. trans.: 1982, chs. 4, 5]) earlier generative models where game theory is explicitly used to formalize how actors' actions depend on one another with later definitions of this notion where the "contextual" component conceived in terms of interdependency disappears (consider for instance how Boudon (2002a, p. 21, 22) explained his research strategy to approach a wide range of statistical distributions from 1990 and 1998 World Values Survey data: "[...] I have attempted to penetrate it [...] using the generative models method. Here, it consists in trying to impute to ideal-typical respondents a system of reasons that can explain, at a qualitative (ordinal) level, the characteristics of the observed distributions [...] with the aim of identifying the micro-sociological origin of the macro-sociological tendencies we detect, by applying the theory of rationality that I have defended, notably in Boudon (1998, 2001a)" (my own translation).
- 2 Boudon (2012a, p. 18) lately seemed to admit the distinction within a context however where he criticizes others for not doing what one may have expected him to do: "Incident remark: Networks are today a popular topic of sociological research. But they are often treated in a mere descriptive or mechanical fashion, while a connection with the theory of ordinary rationality would make network research more fruitful, as many classical and modern sociological works suggest."

invites students to consider who else defended this recursive understanding of the micro-macro link (see Raub and Voss 2017), and who criticized it (see Jepperson and Meyer 2011).

#### NUMERICAL SIMULATIONS

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The last working principle that is behind some of the six pieces of research discussed in the “Research puzzles” module (namely those addressing questions 1, 2, 3, and 4) is methodological rather than substantive. It concerns Boudon’s (1965) suggestion that algorithm-based computer simulation and numerical analysis of mathematical models (i.e., numerical simulations) can be used to verify the extent to which a given generative model can produce numerical structures that are in line with the empirical patterns to be explained. The pedagogic value of exposing students to this principle is to make them aware that statistical methods are not the only tool that sociologists can use for hypothesis testing. With respect to numerical simulations, the lecture’s discussion break then raises the question of the extent to which Boudon has progressively de-emphasized the use of formal tools to study generative models, and, in particular, possibly failed to appreciate the importance for their rigorous study of the most recent and advanced developments in the field of computational modeling (see Manzo 2012, pp. 50-57).

#### THE ROLE OF CHANCE

Although it cannot be regarded as a working principle transversal to Boudon’s pieces of research discussed in the “Research puzzles” module, I do believe that there is an additional ingredient of Boudon’s view of generative model building that would be very profitable to students. It is Boudon’s (1984, pp. 184-190) advice to give more attention to the role of *chance* in modeling social processes where *chance* is understood as the possible intersection of independent causal chains (Boudon 1984, p. 186, 189). This is a topic that Boudon only addressed explicitly once. Still, it seems sufficiently general to be brought to students’ attention: it may indeed help them to develop a reflection on how contingency may be measured. In this sense, thinking more about *chance* constitutes a warning for students against the temptation of over-emphasizing social determinism, for the simple reason that indicators supposedly capable of quantifying it are more easily accessible than indicators of the by-chance event (see Erikson’s chapter in this book).

The *modus operandi* followed by Boudon in his empirically-oriented pieces of research exposed in the module “Research puzzles” thus offers a clear set of

research guidelines to students. The six working principles briefly discussed can indeed easily be turned into simple research heuristics: “design generative models!” (principle 1); “Think about actors’ action’ logic!” (principle 2); “Do not forget interdependency structures!” (principle 3); “Connect recursively the micro- and macro-levels!” (principle 4); “Possibly put in motion the model through simulation!” (principle 5); and, “pay attention to chance!” (principle 6). That each of them still generates debates, as testified by each lecture’s “discussion” break, suggests that Boudon’s legacy for students is real and can have strong training value.

## RESEARCH QUALITY

According to the requirements of the imaginary setting that I have described in the introduction, the introductory course to sociology based on Boudon’s works, had to provide not only practical guidelines on how to design sociological research but it also had to help students to appreciate the current state of sociology as a discipline. Meeting this second demand was relatively easy, as Boudon wrote extensively on this topic throughout his career. The selection of his writings that I suggest including in the course’s third module “Research quality” (see Appendix 1 for an overview) addresses more particularly what we may call the “quality” debate, in the sense that it concerns the criteria of demarcation between “good” and less good sociology (see Gunnar et al. 2024).

For pedagogic clarity, the “Research quality” module organizes Boudon’s contribution to the “quality” debate as responses to the following five questions:

1. Does sociology have a single identity?
2. What are the reasons of sociology’s heterogeneity?
3. How can we describe sociology’s heterogeneity?
4. What is a good theory?
5. What are the strategies to handle sociology’s heterogeneity?

The five lectures composing the module treats each question in turn by focusing on Boudon’s pieces of works, or portions of them, where the clearest answer to the question is provided. Here I briefly summarize these answers.

### DOES SOCIOLOGY HAVE A SINGLE IDENTITY?

To this question, Boudon has replied consistently negatively over his entire career. Very early, in a collection of essays published in 1971 under the title *La Crise de la sociologie* (*The Crisis of Sociology*), Boudon (1971b, p. 16, 17, 27,

28, 35) speaks of “polymorphism” to qualify sociology’s heterogeneity.<sup>3</sup> Later, he will suggest that the word sociology should actually be used in the plural rather than in the singular form because, he claims, there are only “sociological traditions” or “types of sociology” (Boudon 1996, p. 57).<sup>4</sup> Even later, he uses the metaphor of a “house with many mansions” to describe sociology’s diversity (Boudon 2002b, p. 372). Toward the end of his life, Boudon (2012b, 1004) seems resigned to admit the “irreducible diversity of sociology”.

#### WHAT ARE THE REASONS FOR SOCIOLOGY’S HETEROGENEITY?

Boudon’s reply to this question points to the following factors: a. an intrinsic difficulty to define the object of sociology (Boudon 1971b, p. 11); b. a hesitation among various definitions of what a theory is (Boudon 1971b, p. 16); c. a weak interaction between theory and empirics, with a tendency to give priority to description over explanation (Boudon 1971b, p. 17); d. a tendency to be attracted by the critique of the social order rather than by the explanation of social facts; e. the lack of extensive and appropriate sources of data (Boudon 1971b, p. 44); f. the diffusion of various intellectual movements – he speaks of “post-modernism”, “nihilism”, “constructivism”, “relativism” (see for instance Boudon 1996, pp. 57-58) – that tend to destroy, according to him, the difference between facts and values, thus favoring the belief that scientific knowledge is one among many other species of knowledge.

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#### HOW CAN WE DESCRIBE SOCIOLOGY’S HETEROGENEITY?

To answer this question, Boudon progressively coined a *typology of sociology*, which contained the following types:

1. a form of “descriptive sociology”, which he actually valued, that can be either qualitative or quantitative, whose goal is to produce knowledge of facts otherwise difficult to see (Boudon 1992, p. 11)
2. a form of descriptive sociology, called “cameral”, which only generates data in response to specific demands from various political and social groups (Boudon 1992, p. 11; see also Boudon 1996, p. 73)

3 But this term appears even in later writings (see, for instance, Boudon 1996, p. 54, 74).

4 Here is the full quotation in French: “Ainsi, la sociologie au singulier n’existe pas. Il existe des traditions sociologiques, et des types de sociologie. Lesquels ? Quels sont les plus importants ? Pourquoi verse-t-on dans l’un plutôt que dans l’autre ? Je dirai dans la suite celui que je considère le plus important et pourquoi j’y ai versé, mais auparavant, il me faut revenir sur un point laissé en suspens.” (Boudon 1996, p. 57).

3. a “critical” sociology, which aims at denouncing various unbearable social situations (Boudon 1992, p. 12; see also Boudon 1996, p. 74)
4. a form of sociology whose goal is to explain well-defined phenomena (“circonscrire”, in French, see Boudon 1996, p. 63;) with a puzzling character (Boudon 1992, p. 17; see also Boudon 1996, p. 59, 67).

Initially, this last type of sociology is simply named the “scientific” understanding of sociology (Boudon 1996, p. 58) or sociology with a scientific goal (“*sociologie à visée scientifique*”) (Boudon 1996, p. 58). The label “scientific” or “cognitive” sociology will arrive later, namely in the article “Sociology that really matters” (see Boudon 2002b) (see Barbera’s chapter in this book). With this, the nuance between the two forms of “descriptive” sociology previously distinguished (see types A and B above) disappears under the common label “cameral” sociology; on the other side, a new type appears, the so-called “aesthetic” or “expressive” sociology (Boudon 2002b, p. 372)<sup>5</sup>, which, in early writings, was simply referred as “essays” (Boudon 1971a, p. 16, 1971b, p. 44, 1996, p. 73) or “literature” (Boudon 1996, p. 73).

#### WHAT IS A GOOD THEORY?

Boudon offers his answer to this question while discussing research examples (often from the classics) of the type of sociology that he sees as “scientific”. Within this context, a scientific theory is defined as a set of statements that explain a well-defined phenomenon. According to him, these statements belong to two classes: propositions that one can demonstrate being in line with the empirical observations; and propositions that cannot be testable empirically but can be considered as acceptable (Boudon 1996, p. 59, 60) – “acceptable”, Boudon (1996, p. 61) claims, either because they are deduced from other “strong” theories or because they are used in many other theories. I believe it is important to emphasize that, to Boudon, these are features of good theories in general, meaning for whatever discipline one considers (see Boudon 1996, p. 59; 2002b, p. 374). Boudon did not claim for sociology a specific epistemology. This is an important message to be delivered to students. To be noted in passing: at this point, the lecture’s discussion break will be opened to explain to students where the notion of middle-range theory – defended by

5 Expressive sociology is defined by Boudon (2002b, p.372) as a sociology that formulates “in an original and effective fashion feelings which many people experience in their everyday social lives, such as the feeling that they are manipulated by anonymous forces, or that hypocrisy is a dominant feature of social interaction.”

Boudon (1991) – is located within the larger set of meanings sociologists have given the notion of theory (see Abend 2008).

#### WHAT ARE THE STRATEGIES TO HANDLE SOCIOLOGY'S HETEROGENEITY?

If Boudon's perception of sociology's fragmentation (question 1), of its possible causes (question 2), as well as of its typification (questions 3 and 4) is relatively stable over the years, Boudon's reply to the question of how coping with this state-of-affairs evolves throughout his career. Three main attitudes can be identified, which I will call *laissez-faire*, *tolerant pluralism*, and *scientific activism*, respectively.

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An optimistic *laissez-faire* characterizes Boudon's (1971b, p. 11) early writings. There, he explicitly claimed that sociology's porosity to diffuse social factors as well as its attraction for critical sociology were the result of sociology's epistemological uncertainties ("incertitudes épistémologiques"). These uncertainties were seen by Boudon as a temporary state. Sociologists' reflexivity – what Boudon called "critical sociology" (thus using here the term "critical" in a positive, different sense from the "critical" sociology that he criticized in his typology of sociological styles, see point 3 above; see also Barbera's chapter in this book) – as well as the accumulation of richer sources of empirical data were expected to lead to the resorption of sociology's epistemological uncertainties. In the second chapter of *La Crise de la sociologie*, meaningfully titled the "Sociology in the year 2000" ("La sociologie de l'an 2000"), Boudon (1971b, p. 47) actually even made a specific prediction: toward 2000 ("probably", he added), we will observe a "formalization" of the language of sociology, both in terms of theory and tools, and, the sociology interested in actors' life experiences as well as sociology based on "rhetoric", "dialectic" and text exegesis will belong to the past or (more probably, he added) will have another name.<sup>6</sup>

6 Here is the full quotation in French: "Cette innovation [i.e. the increasing availability of data, my note], qui n'est qu'à ses débuts, aura sûrement une importance extrême pour la sociologie de l'an 2000 [...] De façon générale, les quatre tendances que nous avons brièvement décrites conduiront comme on peut s'en apercevoir dès maintenant, à une formalisation du langage sociologique, tant au niveau de la théorisation qu'à celui des instruments d'analyse. À long terme, l'image de la sociologie, comme la nature du travail sociologique et la formation du sociologue devraient s'en trouver profondément modifiées [...] Lorsqu'il existera – vers l'an 2000 probablement, s'il plaît à Dieu – il est probable que ce type de sociologie, encore bien vivant en France, qui comme la science aristotélicienne s'appuie sur la « rhétorique », la « dialectique » et la glose des nouveaux textes sacrés, appartiendra au passé ou – plus vraisemblablement – portera un autre nom."

Twenty years later, in particular in the introduction to his *Traité de sociologie*, Boudon (1992, p. 15) has already changed his mind. He asks whether we should “regret” sociology’s heterogeneity, and then explicitly replies that we should not. Boudon’s proposal at this stage was to accept sociology’s diversity without accepting nevertheless that “everything goes” (Boudon 1992, p. 16). Thus, he framed his *Traité* as an illustration of a form of scientific sociology, in particular the sociology of action, which he explicitly presented as *one of* the paradigms of sociology but he admitted that there are others (Boudon 1992, p. 19). A view that, as I said, I propose to label *tolerant pluralism*.

A few years later, Boudon was obliged to admit that his prediction for the year 2000 was wrong; he recognized that sociology’s polymorphism “has developed over the last years” (“s’est accentué ces dernières années”, Boudon 1996, p. 74), under the pressure of various social demands and the diffusion of relativism. The crisis of sociology that Boudon saw as temporary in the early seventies is now qualified as a “chronic” (Boudon 1996, p. 54) or “permanent” (Boudon 1996, p. 55) state of the discipline. It is reasonable to admit that it is in reaction to the perception of this trend that Boudon’s “tolerant pluralism” characterizing the *Traité* in the early nineties was progressively transformed in “scientific activism” – “Je me suis toujours reconnu dans une conception scientifique de la sociologie” (“I have always believed in a scientific approach to sociology”, my own translation, see Boudon 1996, p. 75) – leading him to more and more overt claims that the *sociology that really matters* (Boudon 2002b, p. 376) equates to the “cognitive” or “scientific” type, as also finally synthetically expressed in the title of Boudon’s (2010) late auto-biography *La Sociologie comme science* (2010).

Proposing to include a “Research quality” module in an introductory course to sociology for first-year Master’s students is probably not a common choice. I do believe, however, that making students aware as early as possible of sociology’s complex landscape is key to help them realize that sociology is a diverse discipline where various sociological styles co-exist, and that this leads to different types and “qualities” of knowledge. Being informed about these facts can help them to make more reasoned choices about the training and research paths they want to follow, or avoid. Part of Boudon’s legacy is to provide students with resources to navigate this debate.

In this respect, let me finally note that Boudon remained attached to the “quality” debate until the end. In 2012, he still had the energy to ask a long list of well-known colleagues of different confessions what they thought about the question of sociology’s identity. This generated a set of 24 short essays that Boudon (2012b) collected in a special issue of the French journal *Commentaire*. As shown by the syllabus overview in Appendix 1, this is a resource that, in spite



of its low visibility among sociologists, could constitute additional material on its own to organize a dedicated reading group where students could pursue the discussion of contemporary sociology's diversity.

## CONCLUSION

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Boudon began the preface of *La Logique du social* (1979) with the following statement: "The book aims to be *an introduction to sociological analysis*" (italics added). This echoes the book's subtitle. Some paragraphs below, Boudon adds: "This book, therefore, deals with the principles, postulates, and objectives of sociological analysis rather than with the history or data of sociology". And, in the book's postscript, Boudon (1979, p. 295 [Eng. trans.: 1981, p. 169]) writes: "I have been concerned here, as the reader will have understood, with a description of the nature of sociological knowledge as it arises, not in an a priori classification of the sciences, but from the works of sociologists themselves". As explained in a long series of interviews only published in French, *The Logic of Social Action* originated from an invitation that Boudon received from the French historian François Furet around the mid-seventies to write a textbook introducing sociology to a "large audience" ("un public assez large", Boudon 2003, p. 59). The two quotations above thus suggest that Boudon decided to present sociology to that audience by combining research examples with the explanation of the research principles underlying them.

In this chapter, I endorsed a similar perspective, but in my case, the "large audience" I had in mind was made up of first-year Master's sociology students. In addition, while Boudon relied on a selection of materials drawn from sociology at large, having ultimately in mind the question of assessing Boudon's legacy, my exercise has rather consisted in restraining myself only to Boudon's work as a possible source for the to-be-designed introductory course to sociology. Given this goal and constraints, I ended up with a selection of Boudon's articles and book's chapters organized in three modules of six lectures each. The first module ("Research puzzles") follows the principle of teaching sociology by dissecting the machinery of specific pieces of research: it focuses on a selection of substantive phenomena studied by Boudon, and presents the substantive content of the explanations proposed by Boudon to a series of *why*-questions. The second module ("Research heuristics") discusses the *modus operandi* behind the pieces of research presented in the first module: it identifies six principles that can help student to design their own sociological projects by following a clear set of research heuristics. The last module ("Research quality") shifts the focus to the diversity of sociology and provides students with resources to appreciate the debates on the possibility of establishing



criteria to distinguish high- from low-quality academic research. An overview of the course is given in Appendix 1.

This exercise was intended to answer the question of Boudon's legacy. It was premised on the intuition that a good way to determine an author's impact is to decide if we want to transmit their work to the next generation, and, if so, which aspects of the work we want to transmit. My answer was that a variety of Boudon's writings still have training virtues. This is either because they proposed, or contributed to proposing, new ways of doing sociology, or because they touched upon fundamental problems that deserve to be further investigated. Within sociology's contemporary landscape, Boudon's style is distinctive, and students may benefit from understanding why, and in what sense, this is the case.

In this respect, an important open question raised by Boudon's continuous monitoring of the state of the discipline is the extent to which, by increasingly arguing in favor of what he saw as "scientific" sociology, he succeeded in making this type of sociology more visible and diffused. In a comment on the *Handbook of Sociological Science* edited in 2022 by Gërzhani, de Graaf and Raub, Jesper Sorensen (2024, pp. 249-250) wondered whether not "naming names", meaning not explicitly criticizing what one sees as "non-rigorous" sociology, is "the right strategy, if one truly believes that sociology is, or should be, a science". As explained in this chapter's last section, Boudon progressively moved to a more and more overt "naming names" attitude. But, was he successful in modifying the relative sizes of the various types of sociology populating the discipline? If not, why, and what other options do we have to cope with sociology's diversity? These are important questions that Boudon left to students and young scholars. It is our responsibility to continue to meditate on them.

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APPENDIX 1

|                                                                                                                                                                                       |         |                                                                                                                                                                                                                                                                |
|---------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|---------|----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Overview of the structure, of the topics and the associated assigned readings for a first-year, Masters-level introductory course to sociology based on a selection of Boudon's works |         |                                                                                                                                                                                                                                                                |
| Module 1: "Research puzzles"                                                                                                                                                          |         |                                                                                                                                                                                                                                                                |
| Short description: Dissect the machinery behind how Boudon replied to well-defined counter-intuitive why-questions concerning specific substantive macroscopic phenomena              |         |                                                                                                                                                                                                                                                                |
| Class 1                                                                                                                                                                               | Topic   | Why does the frequency of justice decisions resulting in dropping the case ("affaires classées sans suite") increase in France between 1831 and 1950?                                                                                                          |
|                                                                                                                                                                                       | Reading | Davidovitch A. and Boudon R., 1964, "Les mécanismes sociaux des abandons de poursuites : Analyse expérimentale par simulation," <i>L'Année Sociologique</i> , 15, pp. 111-244.                                                                                 |
| Class 2                                                                                                                                                                               | Topic   | Why do actors with high social background tend to make more ambitious educational choices compared to actors with low social background even when they have similar grades?                                                                                    |
|                                                                                                                                                                                       | Reading | Boudon R., 1973, <i>L'Inégalité des chances</i> , Paris, Armand Colin (Eng. trans.: <i>Education, Opportunity and Social Inequality. Changing Prospects in Western Society</i> , New York, John Wiley and Sons, 1974, ch. 4 "A dynamic IEO model").            |
| Class 3                                                                                                                                                                               | Topic   | Why may an increase in the number of highly-educated individuals not lead to a proportional increase in the rate of absolute intergenerational social mobility?                                                                                                |
|                                                                                                                                                                                       | Reading | Boudon R., 1973, <i>L'Inégalité des chances</i> , Paris, Armand Colin (Eng. trans.: <i>Education, Opportunity and Social Inequality. Changing Prospects in Western Society</i> , New York, John Wiley and Sons, 1974, ch. 8 "Towards a formal theory of ISO"). |
| Class 4                                                                                                                                                                               | Topic   | Why may the fraction of unhappy actors initially increase despite the fact that the number of existing places giving access to a certain goods expands?                                                                                                        |
|                                                                                                                                                                                       | Reading | Boudon R., 1977, <i>Effets pervers et Ordre social</i> , Paris, Presses Universitaires de France (Eng. trans.: <i>The Unintended Consequences of Social Action</i> , New York, St. Martin's Press, 1982, ch. 5 "The Logic of Relative Frustration.")           |
| Class 5                                                                                                                                                                               | Topic   | Why were French students, compared to students in other countries, so massively in favour of protesting in May and June 1968?                                                                                                                                  |
|                                                                                                                                                                                       | Reading | Boudon R., 1971a, "Sources of Student Protest in France," <i>The Annals of the American Academy of Political and Social Science</i> , 395, pp. 139-149, DOI: 10.1177/000271627139500113.                                                                       |

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|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|------------|----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Class 6                                                                                                                                                                                                                                                            | Topic      | Why do highly-educated citizens tend to have markedly different levels of tolerance to moral and behavioural diversity compared to low-educated ones?                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                      |
|                                                                                                                                                                                                                                                                    | Reading    | Boudon R., 2002a, <i>Déclin de la morale? Déclin des valeurs?</i> Paris, Presses Universitaires de France.                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                 |
| <b>Module 2: "Research heuristic"</b>                                                                                                                                                                                                                              |            |                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                            |
| <i>Short description:</i> Explains the general principles defining the sociological style behind the six pieces of research discussed in module 1, and proposes "critical breaks" (discussion) to think about their history, limitations, and possible extensions. |            |                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                            |
| Class 1                                                                                                                                                                                                                                                            | Topic      | "Design generative models!"                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                |
|                                                                                                                                                                                                                                                                    | Reading    | Boudon R., 1979a, "Generating Models as A Research Strategy," in <i>Qualitative and Quantitative Social Research. Papers in Honor of Paul F. Lazarsfeld</i> , edited by R. K. Merton, J. S. Coleman, P. H. Rossi, New York, The Free Press, pp. 51-64.                                                                                                                                                                                                                                                                                                                                                                                                                     |
|                                                                                                                                                                                                                                                                    | Discussion | <i>Was Boudon the inventor of this notion?</i><br>Reading: Manzo G., 2024, "Antecedents of generative thinking in analytical sociology: the contribution of Tom Fararo," <i>The Journal of Mathematical Sociology</i> , pp. 1-22, DOI: 10.1080/0022250X.2024.2423946.                                                                                                                                                                                                                                                                                                                                                                                                      |
| Class 2                                                                                                                                                                                                                                                            | Topic      | "Think about actors' action' logic!"                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                       |
|                                                                                                                                                                                                                                                                    | Reading    | Boudon R., 1989, "Subjective Rationality and the Explanation of Social Behavior," <i>Rationality and Society</i> , 1, 2, pp. 171-196, DOI: 10.1177/1043463189001002002; Boudon R., 1996b, "The cognitivist model. A generalized rational-choice model", <i>Rationality and Society</i> , 8, 2, pp. 123-150, DOI: 10.1177/104346396008002001; Boudon R., 2012a, "Analytical Sociology and the Explanation of Beliefs," <i>Revue Européenne des Sciences Sociales</i> , 50, 2, pp. 7-34, DOI: 10.4000/ress.2165; Boudon R., 2014, "What is Context?" <i>Kölner Zeitschrift für Soziologie und Sozialpsychologie</i> , 66 (Suppl), pp. 17-45, DOI: 10.1007/s11577-014-0269-2. |
|                                                                                                                                                                                                                                                                    | Discussion | <i>Is Boudon's theory of action defensible?</i><br>Reading: Opp K.-D., 2014, "The Explanation of Everything. A Critical Assessment of Raymond Boudon's Theory Explaining Descriptive and Normative Beliefs, Attitudes, Preferences and Behaviour," <i>Papers</i> , 99, 4, pp. 481-514, DOI: 10.5565/rev/papers.2076.                                                                                                                                                                                                                                                                                                                                                       |
| Class 3                                                                                                                                                                                                                                                            | Topic      | "Do not forget interdependency structures!"                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                |
|                                                                                                                                                                                                                                                                    | Reading    | Boudon R., 1979b, <i>La Logique du social</i> , Paris, Hachette (Eng. trans.: <i>The Logic of Social Action</i> , Boston, Routledge & Kegan Paul, 1981, chs. 4, 5 and 6).                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                  |
|                                                                                                                                                                                                                                                                    | Discussion | <i>What form of actions' interdependency did Boudon really care of?</i><br>Readings: Granovetter M., 1973, "The Strength of Weak Ties," <i>American Journal of Sociology</i> , 78, pp. 1360-1380, DOI: 10.1086/225469; Granovetter M., 1983, "The Strength of Weak Ties: A Network Theory Revisited," <i>Sociological Theory</i> , 1, pp. 201-233, DOI: 10.2307/202051.                                                                                                                                                                                                                                                                                                    |
| Class 4                                                                                                                                                                                                                                                            | Topic      | "Connect recursively the micro- and macro-levels!"                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                         |
|                                                                                                                                                                                                                                                                    | Reading    | Boudon R., 1981, "The Individualist tradition in sociology," in <i>The Micro-Macro Link</i> , edited by R. K. Merton, J. S. Coleman and P. H. Rossi, New York, The Free Press, ch. 1, pp. 45-70.                                                                                                                                                                                                                                                                                                                                                                                                                                                                           |
|                                                                                                                                                                                                                                                                    | Discussion | <i>Who else defended Boudon's view of the micro-macro link, and who criticized it?</i><br>Readings: Raub W. and Voss T., 2017, "Micro-Macro Models in Sociology: Antecedents of Coleman's Diagram," in <i>Social Dilemmas, Institutions, and the Evolution of Cooperation</i> , edited by B. Jann and W. Przepiorka, Berlin, De Gruyter, pp. 11-36; Jepperson R. and Meyer J. W., 2011, "Multiple Levels of Analysis and the Limitations of Methodological Individualisms," <i>Sociological Theory</i> , 29, 1, pp. 54-73, DOI: 10.1111/j.1467-9558.2010.01387.x.                                                                                                          |

|                                                                                                                                                                                                                                 |            |                                                                                                                                                                                                                                                                                                                                                      |
|---------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|------------|------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Class 5                                                                                                                                                                                                                         | Topic      | "Possibly put in motion the model through simulation"                                                                                                                                                                                                                                                                                                |
|                                                                                                                                                                                                                                 | Reading    | Boudon R., 1965, "Réflexion sur la logique des modèles simulés," <i>Archives européennes de sociologie</i> , VI/1, pp. 3-20, DOI: 10.1017/S0003975600001119.                                                                                                                                                                                         |
|                                                                                                                                                                                                                                 | Discussion | <i>How did Boudon see the latest developments of the field of computational methods?</i><br>Reading: Manzo G., 2012, "Reason-based explanations and analytical sociology. A rejoinder to Boudon," <i>Revue Européenne des Sciences Sociales</i> , 50, 2, pp. 35-66, DOI: 10.4000/ress.2231.                                                          |
| Class 6                                                                                                                                                                                                                         | Topic      | "Pay attention to chance"                                                                                                                                                                                                                                                                                                                            |
|                                                                                                                                                                                                                                 | Reading    | Boudon R., 1984, <i>La place du désordre</i> , Paris, Presses Universitaires de France (Eng. trans.: <i>Theories of Social Change: A Critical Appraisal</i> , Cambridge, Polity Press, 1986, ch. 6, pp. 184-190).                                                                                                                                    |
| Module 3: "Research Quality"                                                                                                                                                                                                    |            |                                                                                                                                                                                                                                                                                                                                                      |
| <i>Short description:</i> Provides tools to appreciate the current state of sociology by discussing Boudon's view on the "quality" debate, i.e. analyses of the criteria of demarcation between "good" and less good sociology. |            |                                                                                                                                                                                                                                                                                                                                                      |
| Class 1                                                                                                                                                                                                                         | Topic      | Does sociology have a single identity?                                                                                                                                                                                                                                                                                                               |
|                                                                                                                                                                                                                                 | Reading    | Boudon R., 1993, "European Sociology: The Identity Lost?" in <i>Sociology in Europe: In Search of Identity</i> , edited by B. Nedelmann and P. Sztompka, Berlin and New York, De Gruyter, 1993, pp. 27-44.                                                                                                                                           |
| Class 2                                                                                                                                                                                                                         | Topic      | What are the reasons of sociology's heterogeneity?                                                                                                                                                                                                                                                                                                   |
|                                                                                                                                                                                                                                 | Reading    | Boudon, R., 1971b, <i>La Crise de la sociologie</i> , Genève, Droz (Eng. trans.: <i>The Crisis of Sociology</i> , New York, Columbia UP, 1980, ch. 1).                                                                                                                                                                                               |
| Class 3                                                                                                                                                                                                                         | Topic      | How can we describe sociology's heterogeneity?                                                                                                                                                                                                                                                                                                       |
|                                                                                                                                                                                                                                 | Reading    | Boudon R., 2002b, "Sociology That Really Matters: European Academy of Sociology, First Annual Lecture, 26 October 2001, Swedish Cultural Center," <i>European Sociological Review</i> , 18, 3, pp. 371-378, DOI: 10.1093/esr/18.3.371.                                                                                                               |
| Class 4                                                                                                                                                                                                                         | Topic      | What is a good theory?                                                                                                                                                                                                                                                                                                                               |
|                                                                                                                                                                                                                                 | Reading    | Boudon R., 1991, "What Middle-Range Theories Are," <i>Contemporary Sociology</i> , 20, 4, pp. 519-522, DOI: 10.2307/2071781.                                                                                                                                                                                                                         |
|                                                                                                                                                                                                                                 | Discussion | <i>How does Boudon's understanding of theory stand within the larger set of definitions given to "theory" within contemporary sociology?</i><br>Reading: Abend G., 2008, "The Meaning of 'Theory,'" <i>Sociological Theory</i> , 26, 2, pp. 173-199, DOI: 10.1111/j.1467-9558.2008.00324.x.                                                          |
| Class 5                                                                                                                                                                                                                         | Topic      | What are the strategies to handle sociology's heterogeneity?                                                                                                                                                                                                                                                                                         |
|                                                                                                                                                                                                                                 | Reading    | Boudon, R., 1971b, <i>La Crise de la sociologie</i> , Genève, Droz (Eng. trans.: <i>The Crisis of Sociology</i> , New York, Columbia UP, ch. 2 "La sociologie de l'an 2000"); Boudon R., 1996a, "Pourquoi devenir sociologue ? Réflexions et évocations," <i>Revue Française de Science Politique</i> , 46, 1, pp. 52-79., DOI: 10.3917/rfsp.461.52. |
| Reading group                                                                                                                                                                                                                   | Topic      | 24 points of view on sociology's diversity                                                                                                                                                                                                                                                                                                           |
|                                                                                                                                                                                                                                 | Reading    | Boudon R., 2012b, "La sociologie: science ou discipline?" <i>Commentaire</i> , 136, pp. 1001-1093.                                                                                                                                                                                                                                                   |

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Choix scolaires en France et en Italie au XX<sup>e</sup> siècle*  
Gianluca Manzo

## ACCLAIMS

This remarkably well-structured volume accomplishes two feats at once. It offers a critical engagement with the multiple facets and contributions of Raymond Boudon's sociological oeuvre, for example: the modeling of relative deprivation, the generative approach to social stratification, the plea for methodological individualism, the analysis of unintended consequences and social change, the epistemology of sociological investigations, and the reflection on rationality and belief formation. Through this critical engagement – here is the second feat – this volume tackles substantive and methodological issues central to contemporary developments in the discipline of sociology, whether the focus is on formal models, simulation work, counterfactual reasoning, social mobility and its measurements, the significance of Rational Choice, or our understanding of processual dynamics.

Ivan Ermakoff, Professor of Sociology,  
University of Wisconsin-Madison

Without indulging in praise, this collective volume – bringing together 18 substantial chapters – aims to shed light on the enduring legacy of Raymond Boudon's sociology. It addresses a notable gap: the lack of a detailed, multifaceted examination of the work of one of the foremost figures in both French and international sociology. The reader will find not only an assessment of Boudon's intellectual contributions but also a critical appraisal of their limitations and the avenues they open for further research into contemporary issues. The book will appeal both to specialists familiar with the evolution of Boudon's thought over time and to those wishing to discover it, explore it in greater depth, or draw upon it for teaching purposes.

Gérald Gaglio, Professor of Sociology,  
Université Côte d'Azur

This book is a splendid tribute to Raymond Boudon, one of the most important sociologists of the second half of the 20<sup>th</sup> century. The contributions, in their appreciative and critical aspects alike, clearly bring out the intellectual depth and challenging nature of Boudon's work and its continuing relevance in the study of modern societies.

John H. Goldthorpe, Emeritus Fellow,  
Nuffield College, University of Oxford

This collection of papers, expertly curated by Gianluca Manzo, is as wide-ranging and thought-provoking as Raymond Boudon himself. It is sure to stimulate interest in a now-sometimes-forgotten giant of French sociology.

Neil Gross, Charles A. Dana Professor of Sociology,  
Colby College (Maine)

This Memorial Festschrift honors Raymond Boudon (1934–2013) by considering his contributions to conceptualization, theory, and empirics, as well as their associated methods, across foundational topical domains in sociology and guided by expert commentators. It is not only a superb assessment, and its value will grow in three main ways. First, like most Festschriften, it provides a portrait of the growth and trajectory of Boudon's ideas, embedded in his relations with other scholars, both teachers, peers, and students. This portrait will grow over time. Second, as the historian David Knowles wrote about the *quaestiones quodlibetales* of the medieval university (especially the University of Paris) and the debates held during Advent and Lent when anyone could ask any question of any master, Festschrift discussions are a valuable index to what is “in the air” – in this case both when Boudon was working and now. Third, Boudon believed in the promise of mathematics, and it will be possible to trace over time the progress of the  $X \rightarrow Y$  relations in the book, as they travel from general functions to specific functions.

Guillermina Jasso, Professor of Sociology,  
Silver Professor of Arts and Science, New York University

This book is not a hagiography. Unusually, its title truly reflects its content. Twenty-two sociologists from different countries and different generations take a fresh look at the work of Raymond Boudon. In keeping with his approach but without complacency, they highlight the theoretical and methodological contributions of his sociology, its limitations, its errors, its relevance for teaching sociology to the new generations, and the perspectives that remain open in several thematic areas.

Dominique Vidal, Professor of Sociology,  
Université Paris Cité