THE PUZZLE OF STRIKES

In this book Roberto Franzosi presents an analysis of the temporal dynamics of postwar Italian strikes in the industrial sector. The book is novel in a variety of ways. First, Franzosi adopts an unusual inductive approach to social-scientific explanations. He begins by highlighting a set of characteristics of the strike data that call for explanation: the pieces of the puzzle. Then, chapter by chapter, he uses a broad range of available strike theories — business cycle, economic hardship, resource mobilization, bargaining structure, political exchange, and Marxist — as clues to assemble the puzzle. As a result, the book has the narrative flavor of a mystery story, with its sequence of false steps, pat solutions, and unexpected twists and turns (typical is the complete reversal of the causal argument in the final chapters). Second, Franzosi combines the use of statistical, historical, ethnographic, newspaper, and survey material. A constant dialogue runs through the book between different methodological approaches (in particular, statistical versus historical) and theoretical approaches (in particular, Marxist versus non-Marxist). Third, strikes are viewed as the strategic interaction between organized interests (“It takes two to tango”). The focus is as much on employers’ and state actions as on workers’ actions. Finally, the book’s aim is not merely descriptive, nor does it seek simply to test the explanatory power of existing strike theories. Rather, the goal is to disentangle the causal structure in the historical interactions among economic, institutional, and political processes. Two broad questions loom in the background: What moves history forward and what role does conflict play?

The result of Franzosi’s eclectic methodological and theoretical approach is that, unexpectedly, each theory works, each theory contributes to fitting at least some of the pieces of the puzzle. Business cycle theories explain the periodic ups and downs of strike frequency: when unemployment soars, the frequency of strikes declines — although their duration increases. Resource-mobilization theories account for the close link between the availability of organizational resources and workers’ capacity to mount collective action (particularly, successful actions). Institutionalization theories of collective bargaining are best equipped to deal with the periodic rhythm of industrial conflict imparted by the renewal of labor contracts. Political exchange theories explain the overall change in the shape of strikes (toward shorter, less frequent, but much larger strikes) during the late 1970s, as the Italian Communist Party was slowly being brought into a government coalition. Finally, strike waves emerge from the analyses as motors of socio-political change. Theories broadly conceived within a Marxist theoretical tradition provide the most plausible explanation for the occurrence of mobilization processes of such momentous proportions. Of course, if all theories are right, the puzzling theoretical questions are: Under what conditions will a given theory hold? How can we make predictions about future strike patterns?
The puzzle of strikes: class and state strategies in postwar Italy

Franzosi, Roberto.

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To Maddalena and Marianna, my daughters,
in fulfillment of a long-standing promise.

To those who dream
and upon whom circumstances
force the courage to struggle
to make their dreams come true.

So do flux and reflux – the rhythm of change –
alternate and persist in everything under the sky.
Thomas Hardy (1978, p. 399)

Nescire autem quid ante quam natus sic acciderit, id est semper esse puerum.
Quid enim est aetas hominis, nisi ea memoria rerum veterum cum superiorum
aetate contextur?
(To be ignorant of what occurred before you were born is to remain always a
child. For what is the worth of human life, unless it is woven into the life of our
ancestors by the records of history?)
Cicero (1962, pp. 394–5 – xxxiv, 120)
Preface

We know more and more about less and less
until at the end we know everything
about nothing.

This book took four years to write and ten to rewrite (in an ideal sense, anyway, because the actual rewriting took place between April 1992 and September 1993, with further spells of writing between April and December 1994). It started as a dissertation project in the Department of Sociology (back then, Social Relations) at The Johns Hopkins University in 1978. I carried out the empirical work and the final writing of the dissertation at the Centro Studi Confindustria in Rome, during the two years I spent there as a researcher, from September 1979 to August 1981. I began the rewriting during the 1981–82 academic year at the Center for Research on Social Organizations at the University of Michigan and continued it at the University of Wisconsin.

Several people back then thought the dissertation to be of sufficient quality to be publishable as a book (it was accepted as such in 1986 by another press). Over the years they encouraged me, pushed me, threatened me to “get it out.” Why, then, has it taken me ten years to do so? The answer to this question is bound up with both professional and personal strands of my life. In 1982, I believed that the dissertation was a good piece of statistical analysis. After all, I had taken great care in digging out long forgotten and buried data, in teasing out the statistical details of the arguments, in always being true to the data and what they had to tell me.

But there was the problem: statistics. I did not want to publish just a piece of statistical virtuosity. By the end of my empirical work on the dissertation, I had realized that there was a lot more behind the statistical paraphernalia of the dissertation. The finding that economic, institutional, and organizational variables all contributed to determine the temporal movements of Italian strikes was an important story. After all, the prevailing theories concerning strikes in Italy painted a somewhat different picture. Even at the level of popular culture, the notion that there were empirically discernible and predictable patterns in the temporal movements of Italian strikes seemed to contradict basic gut feelings about the irrationality of Italian labor relations, particularly during the 1970s. Nonetheless, the story left me somewhat uneasy. I felt that it told only half of the truth. Both the data and the detailed statistical work were bringing out a much more complex
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picture. The final picture seemed to be one in which the determinants of strikes (the "independent variables") changed over time with the levels and forms of conflict. But at the time, I was not ready to accept such a reversal of the causal argument. Neither was I ready to accept the idea that a worker's diary, an employer's interview, or three data points could constitute empirical evidence. The quantitative strike literature could no longer help me in making sense of this new picture. Unfortunately, I knew no other literature. I had exhausted the limits of my historical/theoretical knowledge. In pursuing a mathematical/statistical background, I had had no time to expand my historical/theoretical knowledge beyond the limits of a literature review. Yet my own data, my own findings, the very products of my intensive quantitative background, were now crying out to me: "There is history behind your time series." Or, as Michelle Perrot (1968, p. 120) would have it, "there are people behind numbers." Where had sociology gone among all of this? Where had history? Where had theory?

But if conflict itself changed the parameters that shaped its course, these parameters were not under workers' control. Only conflict itself was. What else was behind my strike data, my spectral coherences and regression coefficients? What other actors in the picture had been obscured by the emphasis on quantitative strike research? My empirical findings on the relationships between strikes and the bargaining structure, strikes and the business cycle, offered some suggestions: The state had institutionalized in the law the broad framework in which collective bargaining took place; both employers and the state made the economic decisions (investment, monetary, labor policies) that determined the outline of the business cycle. But employers and the state rarely enter into the picture in quantitative strike research. The connections were right there in the data, but I did not know how to draw them. I had dug myself deeper and deeper into the hole of specialization of knowledge and statistical expertise. I did not know how to read history except through the coefficients of time-series analysis. There was no other meaning, for me, behind the immediate meaning of the coefficients. It was a time of terrible personal frustration with the scientific enterprise, with the emptiness and narrowness it can foster, with the "one-dimensional man" that is all too common among social scientists.

A meeting I had in 1980 with a union cadre at CGIL best illustrates where I stood then and how far I still had to journey. The cadre, intrigued by the fact that I had studied for years in the United States, that I was working for i padroni of Confindustria (both class enemies), and that I was using computers to study strikes, worriedly asked me: "So, what have you found?" "Well," I proudly replied, "my main findings are that unemployment rate has a negative effect on the frequency of strikes and that strike size and unionization seem to go together, although I have to do further work to disintangle the causal direction of the relationship." After some translating of statistical jargon into plain Italian, the union cadre paused for a second, and then, with a broad smile (the class enemies apparently did not have one up on him) he said to me: "Is that it? You mean to tell me that someone is paying
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you good money and that you are using computers to discover that? Well, next time,” he concluded, “come talk to me first and I will tell you the same things and you can take a two-year vacation at the expense of i padroni.” He did not seem to be too impressed with the fact that I had used the most appropriate data and the most up-to-date statistical techniques to tease out those results. Neither was he impressed with the fact that my findings contradicted Snyder’s work on Italy – the best econometric analysis of Italian strikes. He had never heard of Snyder, and, perhaps, “he too should have come talk to me.” At the time I found consolation in the fact that at least I had not distorted reality. But, in fact, is that enough to justify years of training, a good income, and the use of cutting-edge technology?

Slowly at first, at the University of Michigan, and at an increasing pace at the University of Wisconsin, I began a long journey in history and theory. History (and theory) was creeping in through the courses I was teaching, through the Ph.D. preliminary examinations I was correcting, the seminars in which I was participating, the informal conversations with colleagues and students, the corridor gossip, the network of friendships. In the Class Analysis Program, the struggle between history and theory (“us”) and “number crunching” (“them”) was being waged daily. But the historical and theoretical knowledge necessary to reframe my manuscript was not forthcoming, not, at least, as fast as I thought it should be. And while I was waiting for that to happen, the manuscript, too, was waiting in a drawer. Not that I forgot about it. On the contrary. Throughout all those years the manuscript was haunting me, as I kept thinking about the arguments, as I kept trying to find answers to my questions: How can I overcome the limitations of a statistical approach to the study of strikes? How can I provide a broader picture of the actors and actions involved?

By 1985, I was probably ready to get the manuscript out, casting it in the kind of historical/theoretical framework that I had been developing – at least so it seems, judging from an unpublished paper from that year. Unfortunately, by that time, a new project had rolled in. Frustrated by the poverty of information content in the official strike statistics that I had used in my dissertation, in 1982 I had started exploring new ways of studying industrial conflict. Influenced by the work of Charles Tilly and his use of newspapers as sources of historical data, I started developing a methodology of data collection from text sources that would allow me to collect highly qualitative data to be analyzed statistically. The goal was that of achieving quantity without sacrificing quality. In January 1986 I set up a project of data collection in Genoa, Italy. From 1986 to 1991, when I finally completed data collection on the 1919–22, 1968–72, and 1986–87 periods in Italy, the new project entirely absorbed my energies. It periodically kept me away from Madison for long stretches of time. The development of an entirely new linguistic- and computer-based approach to content analysis proved to be more problematic than originally anticipated. I spent incredibly long and unrewarding hours doing computer programming. No less problematic was the long-distance supervision of the project, despite modern computer-communications technology. Only stubborn
determination to finish what I had set out to accomplish saw me through it. In the meantime, the practical problems of completing the data collection and some extensive writing about the new methodology did not allow me the peace of mind necessary to work on the manuscript. In 1991, data collection on those projects finally came to an end. In April 1992, having been denied tenure at the University of Wisconsin and having no job, I finally had the time and no choice but to write this book. My earlier solution to the problem of obtaining richer data had led to the development of a methodology that would combine quality and quantity within a single approach. In this book, I offer yet a different solution based on the simultaneous use of a variety of both quantitative and qualitative methodologies. The eclectic approach to data and method that I embrace in this book is the cumulative result of my personal agony over the question how to avoid throwing the baby out with the bath water. Having lost faith in the legendarium of high-powered statistics, I agonized for years over the fate of the manuscript, striving to find a format that not only would be statistically elegant but also would be true to the historical record. The Puzzle of Strikes is thus, first and foremost, an attempt to solve the personal puzzle of how to approach the problem of scientific explanations in the social sciences.

If the decade-long agony over the fate of this manuscript had been trying, it was the fifteen months of final writing that brought out a stormy process of personal understanding and substantive breakthroughs that made the writing of this book an incredibly emotional experience. As I was glued to the computer for months on end, relentlessly, the ghosts of history that I was bringing back to life kept me good company, making me laugh and cry with them. And with those ghosts of history came the ghosts of my own past, caught as I had been in the dilemma of “a contradictory class location.” Born to a well-to-do father and a mother from a poor family and raised in the extended paternal family that never fully accepted my mother, the experience of class had been part of my most formative years. The ambiguity of early childhood permanently marked my personality and my life with the stamp of contradictory polarity: living in Italy or in the United States, a bourgeois or a Marxist, a theorist or a methodologist, and so forth. The “hidden injuries of class” have imbued my work over the years with moral overtones and have provided much of the emotional charge. I am well aware of the dangers involved in holding a moral position of truth in the social sciences. But there is even more danger in upholding the truth that comes from the uncritical acceptance of scientific paradigms.

For years I was blown about by the contradictions of my childhood experience, without much idea of what was happening. Only recently have I understood the roots of my personality, through trying years of introspection over the motivations and the actions of the players involved in the social relations of my family. And lately I have come to think that it is time to “forgive and forget.” Interestingly enough, just as in my life I was trying to understand and overcome personal contradictions, in this book I try to overcome contradictions between modes of
social-science productions (historical, ethnographic, statistical, etc.) and theoretical approaches (economic bargaining, resource mobilization, Marxist and non-Marxist, etc.). During the months of final writing, I was playing out my personal dilemmas and contradictions in the dilemmas and contradictions of historical actors. The strategic interactions among the people who populated my childhood blurred into the interactions among Rico, Barbisin, Giuseppe Dozzo, Paolo Migliaccio, Aris Accornero, Roberto Sibona, Cesare Cosi, Nico Ciarciaiglino, Giovanni Agnelli, Leopoldo Pirelli, Guido Carli, Fiorentino Sullo, Carlo Donat Cattin, Enrico Berlinguer, Romolo Gobbi, Guido Viale, and the countless others who crowded the stage of history during the period studied in this book. And so, during the final writing, I gave everyone a voice: workers, the unions, employers, state officials, radicals. I listened to what each had to say. I analyzed what each did and could have done. I empathized and understood. But my heart, I have to admit, was and is with those who tried and, perhaps, lost. Given my personal biography, that is indeed how it should be, perhaps the only way it could have been.

Writing as an Italian far from home and writing in a second language gave me some of the distance necessary to at least try to keep the passion and emotional involvement in check. Writing with no job, with virtually no professional ties to the discipline, and facing the serious possibility of having to get out of academia, further gave me the freedom to maintain my independence from professional camps for the sake of a career . . . that I did not have. So, with a tragic sense of battles lost and won, of the strong passions that animated all sides, of high hopes and despair, it is time to put it all to rest. The view of history that emerges is one of people making their own history – but not just as they please. If there is any sense to C. Wright Mills’s “intersection between biography and history,” that has indeed been the personal experience behind this book.
Acknowledgments

Given the long history of this book, I have accumulated many debts along the way, involving many different people and institutions in the United States and in Italy. I started the work at Johns Hopkins under the direction of Richard Rubin. While at Hopkins, my friends Francesco Caramazza and Alberto Devoto helped me to make my first steps in econometrics and spectral analysis. I carried out some of the early work of data collection and data analysis in Italy at the Centro Studi Confindustria (Confederazione Generale dell’Industria Italiana) between September 1979 and August 1981. Throughout that entire period, Paolo Savona, Confindustria director general, and Enzo Grilli, the director of the Centro Studi, granted me the material support and the research freedom necessary to pursue my interests and follow my research agenda. I can truly say that without Enzo Grilli’s constant help and friendship this work would never have been possible. There is no other single person to whom this book owes more.

Many other people at Confindustria were generous with their time. Librarians, computer operators, secretaries, and colleagues (my office mate Mauro La Noce, in particular) were always more than supportive and patient with me—this despite the fact that I always pressed them to work at my maniacal pace, even in the more relaxed atmosphere of Roman work habits. Franco Adolini, Gianna Bargagli, Massimo Chirichini, Angelo Farcis, Giovanna Guidi, Franco Martone, Maurizio Tarquini, Anna Maria Carandente, and, in particular, Giuseppina Jagher provided invaluable help with data collection. The endless conversations I had with Giulio de Capraris greatly helped me to clarify my ideas. For over a decade, until the day before sending off the manuscript for copyediting, Giulio was always there ready to grapple one more time with The Puzzle, providing bibliographical references and data and sharing with me his views on the current state of labor relations. Isidoro Mariani’s and Massimo Pagani’s knowledge of the details and hidden idiosyncrasies of many Italian data series was invaluable. More than ten years have now lapsed, and so has much of my arrogant belief in high-powered statistics. Gwillym Jenkins, already dying from leukemia, once announced in a class I took with him, “You must fall in love with your data. You must take your data to bed not for a one-night stand but for a long-term relationship, so as to get to know their most intimate aspects.” These simple lessons not only have stuck in my mind, but also have become part of my standard approach to data analysis.
xxiv Acknowledgments

The long years of rewriting started in the Fall of 1981 at the Center for Research on Social Organizations (CRSO) at the University of Michigan, where I spent an academic year as a postdoctoral fellow thanks to a grant from Consiglio Nazionale delle Ricerche, Confindustria’s Giovani Imprenditori, and Associazione Imprenditori Marche. At CRSO, Charles Tilly taught me an unforgettable lesson in history and theory. There, I opened a window on the wide world behind regression coefficients. It took me several years of work, as an assistant professor at the University of Wisconsin, to explore that world.

At the University of Wisconsin, many people contributed to my intellectual development. My colleagues and friends Ron Aminzade, Richard Lachman, and Erik Wright helped me through the years along my new path. They were always there to give me books and articles, to suggest historical and theoretical readings, to include me among the circle of those who appreciate history and theory – “our camp.” They never talked about the fact that I was teaching a seminar in advanced statistics – the “other camp.” In particular, Erik Wright helped me to clarify my arguments. Alberto Palloni shared with me from the beginning my anguish over the tension between theoretical and empirical work.

Throughout the long period of both writing and rewriting, I have always been able to count on another friend: Alberto Devoto. During the last year of terrible isolation, his daily electronic messages to the cry of Forza Paris made life more bearable. Svetlana Kropp read and reread drafts of chapters, ran statistical analyses, and prepared tables. She was not always understanding of my obsession with The Puzzle (yet always supportive), but it is hard for me to imagine having written this book without her. For several years she was forced to share the anxieties and fears that have accompanied the book. Joseph Kepecs played a significant role in exploring the personal, deep seated roots of those fears.

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Toni Schulze, Virginia Rogers, Patricia McDaniel, and Mirjana Sotirović helped with some of the typing. At the end, preparing a “camera-ready” copy of the manuscript was truly a nightmare. My friend Charles Palit provided invaluable support in that process. I spent countless hours at his house. Svetlana Kropp, Piero Squillante, and Eric Newman, production editor at Cambridge, pitched in at the
end. As frustrating as it was at times to stretch the limit of what WordPerfect can do as a typesetting tool, the technical support staff at WordPerfect was always courteous and occasionally even helpful.

I owe much of what I have learned about writing in the English language to the patience of Ann Althouse. Editors at Cambridge University Press carefully went over the manuscript, thoroughly eliminating the many inaccuracies that come from writing in a second language. Gloria Cook helped me in reviewing some of those changes. Vivek Chibber, Denis O’Hearn, Katharine Jones, Fulvio Venturino, and Erik Wright read and provided comments on individual chapters. Gerald Friedman, Richard Lachman, Kenneth Land, John Markoff, Patricia McDaniel, Nader Sohrabi, and Mirjana Sotirović generously offered comments on the entire manuscript at different stages of production. I am particularly indebted to Gian Primo Cella, Samuel Cohn, Giulio de Caprariis, and Michael Shalev for their close reading of the final manuscript. Samuel Cohn’s inspirational review of the manuscript gave me much of the determination to continue. As I was debating whether I had crossed the fine line between determination and delusion, his review gave me the courage to write the kind of book that I had always dreamt of and that I eventually wrote. The title is a combination of suggestions from the Cambridge Syndicate and Gerald Marwell.

Last but not least, I would like to acknowledge the great intellectual debt that I have incurred over the years with two scholars and friends: Charles Tilly and Erik Wright. In different ways, they have provided role models for me to follow, through their relentless pursuit of broad issues, through the depth and breadth of the historical and theoretical questions they asked. I will never forget Charles Tilly asking a bewildered speaker, in all friendliness: “Can you explain to me in three minutes or less why I should be here listening to you?” I have asked that same question always of myself and, occasionally, of others. Why is the problem at hand worth pursuing? What is its relevance? If it is not worth an hour of a listener’s time, then, why should it be worth years of ours?

To all these institutions, to all these and many more people who, without necessarily agreeing with the opinions expressed in this book or the conclusions reached, have contributed their time, ideas, and expertise, I wish to express my gratitude. It is no fault of theirs that I have done no better.