

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?

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THE PUZZLE
OF STRIKES

CLASS AND STATE STRATEGIES
IN POSTWAR ITALY

ROBERTO FRANZOSI
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Published by the Press Syndicate of the University of Cambridge
The Pitt Building, Trumpington Street, Cambridge CB2 1RP
40 West 20th Street, New York, NY 10011, USA
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

© Cambridge University Press 1995

First published 1995

Printed in the United States of America

Library of Congress Cataloging-in-Publication Data

Franzosi, Roberto.

The puzzle of strikes : class and state strategies in postwar Italy / Roberto Franzosi.

p. cm. – (Cambridge studies in comparative politics)

Includes bibliographical references and index.

ISBN 0-521-45287-2

1. Strikes and lockouts – Italy. 2. Labor market – Italy. 3. Italy – Economic conditions – 1945–90.

4. Labor policy – Italy. 5. Collective bargaining – Italy. I. Title. II. Series.

HD5383.F73 1995

331.89'2945 – dc20

DNLM/DLC
for Library of Congress

93-51254
CIP

A catalog record for this book is available from the British Library.

ISBN 0-521-45287-2 hardback

Contents

<i>List of tables, figures, and equations</i>	<i>page</i> ix
<i>Preface</i>	xvii
<i>Acknowledgments</i>	xxiii
1 The puzzle box	1
1.1 Why strikes? Why Italy?	1
1.2 Meet the Italian strike	2
1.3 Letting the data talk: the pieces of the puzzle	4
1.4 How to fit the puzzle: the available theories	7
1.5 Serious problems: dialogue among the deaf	12
1.6 More serious problems: it takes two to tango	15
1.7 Really serious problems: the automatic pilot (writing schemata and regression blenders)	18
1.8 In search of a solution	21
1.9 Organization of the book	24
1.10 Pat solutions, red herrings, and paradoxes	26
2 Labor-market conditions and bargaining power	30
2.1 How the labor-market argument runs	30
2.2 The economists' tradition of strike research: the Ashenfelter and Johnson model	31
2.3 Test of the Ashenfelter and Johnson model: empirical results	33
2.4 A word of caution	35
2.5 Further problems: Is that all we can say?	38
2.6 Examining the residuals	41
2.7 Subsample analyses	42
2.8 Checking the results against economic history	47
2.9 Fitting the puzzle: the first step	54
3 When do workers strike? How the economy matters	56
3.1 Did I go wrong?	56
3.2 Beyond strike-frequency models	57
3.3 Beyond labor-market models	57

3.3.1	Exploratory analyses	59
3.3.2	Multivariate analyses	67
3.4	Structural characteristics of the Italian economy	72
3.4.1	Long-term sectoral shifts in the occupational structure	75
3.4.2	The mode of production: technology and social organization	78
3.4.3	Labor-market structure	80
3.4.4	The industrial structure: the distribution of plant sizes	84
3.5	Even if bargaining parties had perfect knowledge: the Marxist view of conflict and the economy	89
3.6	Where we stand	92
4	Organizational resources and collective action	96
4.1	Shifting gears	96
4.2	Hardship, discontent, and labor unrest	97
4.3	Resource-mobilization theories of collective action	99
4.4	<i>La longue durée</i> : moral economy and repertoires of collective action	101
4.5	Testing the organizational model	106
4.6	Back to exploratory analyses	109
4.7	More history: the organization of interests	114
4.7.1	Workers	114
4.7.2	Employers	119
4.8	Muddled causality: further probing into the role of organization	124
4.9	<i>Aiutati che il ciel t'aiuta</i> : the Marxist approach to organization	134
4.10	Fitting more pieces to the puzzle	139
5	The structure of collective bargaining	143
5.1	Unanswered questions	143
5.2	Collective bargaining in postwar Italy: a brief historical overview	144
5.3	Does the structure of collective bargaining make a difference?	153
5.4	Back to the Ashenfelter and Johnson model	161
5.5	Unexplained residuals: Why models of the number of strikers perform so poorly	168
5.6	Plant-level bargaining	175
5.7	On the cost of strikes (the employers' view)	179
5.8	Unforeseen pieces fall into place	182
5.9	A false sense of security	185
5.10	The picture emerges	187
6	Class power, politics, and conflict	190
6.1	Left to explain: the 1975–78 strike shapes	190
6.2	Political models of strikes: the long term	191
6.3	Political models of strikes: the short term	193

6.4	Italian postwar politics: blocked opportunities on the left	196
6.5	Political subcultures: Red regions, White regions	207
6.6	An overall model of power	212
6.6.1	On the cost of strikes (the workers' view), or, What happens to conflict when a group does not have power?	213
6.6.2	Corporatism <i>all'italiana</i> , or, What happens to conflict when a group does have power?	224
6.7	Short term and long term, economics and politics: the unions' dilemmas	231
6.8	Economic versus organizational/political models of strikes: Snyder's argument on Italy	237
6.9	The micro and the macro, the economic and the political: modes of regulation of labor	240
6.10	The power of statistics and the statistics of power	242
6.11	The finished picture?	253
7	Mobilization processes: the 1969 <i>autunno caldo</i>	257
7.1	Clearly an outlier: 1969	257
7.2	Strike waves and cycles of struggle	258
7.3	The supermarket at Fiat Mirafiori: the workers	264
7.4	The tactics: "everyone did what they wanted"	267
7.5	The demands: <i>vogliamo tutto</i> ("we want everything")	272
7.6	The radical Left	283
7.7	Structure and culture	289
7.8	The limits of participation	293
7.9	Strike waves: political or economic explanations?	297
8	Countermobilization processes: reactions by the state and employers to strike waves	301
8.1	Switching sides: the view from above	301
8.2	State responses	302
8.3	The long aftermath	306
8.4	The great fear: from paternalism to personnel management	308
8.5	Collective responses: reaffirming <i>la centralità dell'impresa</i>	314
8.6	Converging interests: <i>inquadramento unico (mobilità interna)</i>	317
8.7	Housecleaning (<i>mobilità esterna</i>)	323
8.8	Machines don't strike	328
8.8.1	Experimenting with new technologies	330
8.9	Small plants don't strike	332
8.10	Against the market and labor: the employers' dream of total flexibility	335
8.11	The puzzle is complete	338

9 The picture in the puzzle	343
9.1 Unexpected findings, one more time: class conflict as the <i>independent</i> variable	343
9.2 Summing up what we know (firm empirical grounds)	345
9.3 Summing up what we don't know (theoretical puzzles and tentative solutions)	349
9.4 Looking into the crystal ball: venturing predictions from the model	355
9.5 The test of history, one last time: the 1980s	359
9.6 Looking back, looking forth: the 1969 <i>autunno caldo</i> in historical perspective	368
9.7 Which road to the past? Methodological dilemmas	372
Epilogue	378
Appendix: the data	379
<i>Notes</i>	395
<i>Bibliography</i>	449
<i>Index</i>	485

Tables, figures, and equations

TABLES

2.1.	<i>Test of the Ashenfelter and Johnson model</i>	page 34
2.2.	<i>Almon polynomial distributed lag coefficients for models in Table 2.1</i>	36
2.3.	<i>Seasonal model of strike frequency</i>	39
2.4.	<i>Subsample estimates for the Ashenfelter and Johnson model</i>	44
2.5.	<i>Almon polynomial distributed lag coefficients for models in Table 2.4</i>	46
2.6.	<i>Testing the effects of unemployment during the 1950s</i>	47
2.7.	<i>Unemployment rate in Lombardy and Italy (1959–65)</i>	52
3.1.	<i>Test of the Ashenfelter and Johnson model using numbers of strikers and hours lost as dependent variables</i>	58
3.2.	<i>Correlation coefficients between business-cycle measures and numbers of strikes, strikers, and hours lost</i>	59
3.3.	<i>Coherence values at seasonal frequencies between industrial production and numbers of strikes, strikers, and hours lost (manufacturing industry, 1950–01/1978–12)</i>	66
3.4.	<i>Testing the Ashenfelter and Johnson model with labor-market and product-market variables</i>	68
3.5.	<i>Almon polynomial distributed lag coefficients for models in Table 3.4</i>	69
3.6.	<i>Measuring the effects of product-market variables on numbers of strikes, strikers, and hours lost</i>	71
3.7.	<i>More on labor-market and product-market effects</i>	73
3.8.	<i>Nonperiodic cycles of numbers of strikes, strikers, and hours lost</i>	74
3.9.	<i>Chronology of Italian business cycles between 1945 and 1983</i>	75
3.10.	<i>Distribution of the labor force by sector and period (%)</i>	76
3.11.	<i>Hire rates for blue-collar workers in Milanese firms (1959–62)</i>	81
3.12.	<i>Historical transformation of the job hierarchy in Italian firms (1950–68)</i>	83
3.13.	<i>Distribution of Italian plants by size (number of plants per size class)</i>	85
3.14.	<i>Distribution of Italian plants by size (total number of employees per size class)</i>	85

3.15. <i>Distribution of strike-free plants by plant size and strike issue (%)</i>	87
3.16. <i>Confindustria survey data of absenteeism by year and plant size</i>	88
4.1. <i>Testing the organizational model</i>	107
4.2. <i>Signs and significance of the coefficients of the wage and union variables at various lags (0, 1, 2, 3) (estimates of equation 4.1)</i>	108
4.3. <i>Correlation coefficients between numbers of strikes, strikers, and hours lost and union membership and unionization rate (1950–78)</i>	112
4.4. <i>Correlation coefficients between numbers of strikes, strikers, and hours lost and union membership and unionization rate (1956–78)</i>	113
4.5. <i>Correlation coefficients between detrended values of unemployment and numbers of strikes, strikers, and hours lost, by scope (manufacturing industry, 1955–II/1975–IV)</i>	114
4.6. <i>Distribution of Confindustria member firms by firm size in 1969</i>	121
4.7. <i>Distribution of commissioni interne elections by plant size (1969)</i>	127
4.8. <i>Unionization by firm size in the Turin province in metalworking industry (1971)</i>	128
4.9. <i>Organizational success and strike activity by firm size (1972)</i>	129
4.10. <i>Distribution of Bolognese firms and employees by firm size and trade-union presence (1975)</i>	130
4.11. <i>Plant-level characteristics of union structures and collective action mobilization by firm size (%)</i>	132
5.1. <i>Number of plant-level agreements in industry (1953–61)</i>	148
5.2. <i>Bargaining agents in plant-level agreements (%)</i>	148
5.3. <i>Distribution of strike durations at Alfasud (%)</i>	150
5.4. <i>Distribution of number of strikers by strike duration at Alfasud (%) (March 17, 1975, to July 31, 1975)</i>	150
5.5. <i>Scala mobile coverage of wages in manufacturing industries</i>	152
5.6. <i>Number of plant-level agreements in industry (1969–72)</i>	157
5.7. <i>Number of firms involved in plant-level bargaining, and numbers of strikes and strikers</i>	157
5.8. <i>Number of large-scale strikes and percentages of strikers involved</i>	160
5.9. <i>Work stoppages for the renewal of the 1969 metalworkers' collective contract (dates and durations in hours)</i>	161
5.10. <i>Correlation coefficients between numbers of strikes and strikers and the error components from model 4 of Table 2.1 and model 2 of Table 3.6</i>	164

5.11. Correlation coefficients between contract indicator and numbers of strikes, strikers, and hours lost at various levels	165
5.12. Testing the role of the bargaining structure	166
5.13. Almon polynomial distributed lag coefficients for models of Table 5.12	167
5.14. Combining organizational and collective bargaining effects	168
5.15. Checking the effect of unexplained observations (1974, 1975, 1977)	172
5.16. Per capita spending for wages by firm size	178
5.17. Hours worked and hours lost due to labor unrest and absenteeism	181
5.18. Confindustria survey data on absenteeism by year and type	182
5.19. Correlation coefficients between numbers of strikes, strikers, and hours lost and labor share of national income (economywide, detrended data, 1955–78)	185
6.1. Testing a long-term political model of strikes	193
6.2. Testing a short-term political model of strikes	195
6.3. Workers' participation in strikes and unionization in the Red Valdelsa and White Bassano districts, by firm size (%)	210
6.4. Repression of workers (1948–56)	214
6.5. Number of injunctions and fines against employers	217
6.6. Testing the effect of workers' layoffs	223
6.7. Replication of Snyder's work	239
6.8. Testing the effect of the compromesso storico	244
7.1. Is 1969 an influential observation?	260
7.2. Frequency distribution of firms holding elections for commissioni interne, by year and firm size	262
7.3. Percentage of firms with no strikes, by firm size and year	262
7.4. Unionization by firm size and year (%)	263
7.5. CGIL-FIOM union growth in the Turin province, in metalworking, by firm size (% values, 1971/1964)	263
7.6. Workers' participation in collective action	266
7.7. Distribution of strike tactics adopted between 1969 and 1971, by firm size	270
7.8. Strike demands in plant-level bargaining (1970–71)	273
7.9. Wages by skill level in historical perspective	274
7.10. Correlation coefficients between numbers of strikes, strikers, and hours lost and number of nonfatal work-related injuries (industry, 1950–I/1978–IV)	275
7.11. Distribution of blue-collar workers by skill level and year in selected firms	279
7.12. Distribution of work force in industry by skill level, sex, and year	280
7.13. Percentages of workers at various skill levels in the textile and metalworking industries, by year	280

7.14. <i>Rates of participation in collective action of male and female blue-collar workers</i>	296
7.15. <i>Rates of participation in collective action of blue-collar and white-collar workers</i>	296
8.1. <i>Involvement of the Ministry of Labor in labor disputes</i>	305
8.2. <i>Skill-grading system: the old and the new</i>	318
8.3. <i>Distribution of blue-collar workers by skill level in metalworking industry</i>	320
8.4. <i>People looking for job by reason (April 1975)</i>	326
8.5. <i>Ratio between borrowed and internally generated capital, by firm size and year</i>	330
8.6. <i>Percentage change in blue-collar employment by firm size and period</i>	336
9.1. <i>Percentage of firms with egalitarian wage increases, by firm size</i>	363
9.2. <i>Percentage of firm-level contracts characterized by wage incentives, by year and firm size</i>	363
A.1. <i>Measuring the extent of bias in Italian strike data</i>	382
N.2.1. <i>Strike frequency model with moving seasonality</i>	397
N.3.1. <i>Cross-national productivity indices</i>	403
N.3.2. <i>Distribution of Italian plants by size (6–99 employees)</i>	405
N.4.1. <i>Correlation coefficients between unemployment rate and numbers of strikes, strikers, and hours lost, by cause (1959–II/1975–IV) (economywide data, first-differenced values)</i>	408
N.5.1. <i>Absenteeism data for Fiat and Pirelli (1969–81)</i>	419
N.6.1. <i>Economic and political strikes</i>	432

FIGURES

1.1. <i>Distribution of strikes by numbers of strikers (median values 1976–80)</i>	3
1.2. <i>Distribution of strikes by numbers of hours lost (median values 1976–80)</i>	3
1.3. <i>Plot of yearly numbers of strikes in industry</i>	4
1.4. <i>Plot of yearly numbers of strikers in industry</i>	5
1.5. <i>Plot of yearly numbers of hours lost in industry</i>	5
1.6. <i>Yearly shapes of strikes</i>	8–9
2.1. <i>Plot of the residuals (model 2 in Table 2.1)</i>	42
2.2. <i>Plots of observed and estimated values (model 2 in Table 2.1)</i>	43
2.3. <i>Plots of measures of unemployment in industry</i>	51
3.1. <i>Plots of the index of industrial production</i>	60

3.2.	<i>Plots of ISCO composite business-cycle indicator</i>	61
3.3.	<i>Plot of capacity utilization</i>	61
3.4.	<i>Plots of capacity utilization and number of strikes</i>	63
3.5.	<i>Plots of capacity utilization and number of strikers</i>	65
3.6.	<i>Bar charts of numbers of strikes in agriculture, industry, and services (1951–1980, five-year averages)</i>	76
3.7.	<i>Bar charts of numbers of strikers in agriculture, industry, and services (1951–1980, five-year averages)</i>	77
3.8.	<i>Bar charts of numbers of hours lost in agriculture, industry, and services (1951–1980, five-year averages)</i>	77
3.9.	<i>Plot of firm profits</i>	91
4.1.	<i>Plot of CGIL + CISL union membership (industry)</i>	109
4.2.	<i>Plots of CGIL and CISL union membership (industry)</i>	111
4.3.	<i>Plots of percentage of votes obtained by CGIL and CISL in yearly elections of commissioni interne</i>	112
4.4.	<i>Plots of smoothed data for number of strikers and CGIL + CISL union membership</i>	125
4.5.	<i>Plot of PCI membership</i>	138
5.1.	<i>Plot of monthly numbers of plant-level strikes and industrywide strikers</i>	156
5.2.	<i>Plot of the percentage of all strikers accounted for by contract-renewal strikes</i>	158
5.3.	<i>Plot of the percentage of all strikers accounted for by solidarity strikes</i>	159
5.4.	<i>Plot of the percentage of all strikers accounted for by industry-wide strikes</i>	159
5.5.	<i>Plots of the number of large-scale strikes and of the percentage of strikers involved</i>	160
5.6.	<i>Plot of the residuals (model 2 in Table 2.1) with quarters of contract renewal and high strike frequency in metalworking industry clearly marked</i>	163
5.7.	<i>Plot of the yearly percentage of industrial workers subject to contract renewal</i>	170
5.8.	<i>Plot of the number of strikes involving the largest time lost (“most costly strikes”)</i>	171
5.9.	<i>Plot of the percentage of total number of hours lost accounted for by strikes with the largest number of hours lost</i>	171
5.10.	<i>Plot of the percentage of workers on strike for “other” reasons</i>	175
5.11.	<i>Plot of quarterly number of hours worked in the manufacturing industry</i>	180
5.12.	<i>Plot of rate of change of contractual minimum wages (metalworking industry)</i>	184

6.1.	<i>Plot of number of layoffs of members of commissioni interne</i>	221
6.2.	<i>Plot of the percentage of layoff procedures fought by the unions</i>	227
6.3.	<i>Plots of Tarantelli's measures of workers' market power</i>	228
6.4.	<i>Plot of the durations of governments between 1947 and 1980</i>	245
6.5.	<i>Plot of the durations of government crises between 1947 and 1980</i>	246
6.6.	<i>Bar chart of number of government crises per year between 1950 and 1980</i>	246
6.7.	<i>Plot of monthly number of strikers (economic strikes)</i>	248–9
6.8.	<i>Plot of monthly number of strikers (political strikes)</i>	251
7.1.	<i>Plots of the number of work-related injuries and of the number of hours worked in industry</i>	276
8.1.	<i>Plot of transfer rate (1965–I/1977–IV)</i>	321
8.2.	<i>Plot of transfer rate (1958–01/1964–12)</i>	322
8.3.	<i>Plots of the numbers of individual layoffs for giustificato motivo and giusta causa</i>	324
8.4.	<i>Plots of the numbers of workers laid off and firms requesting layoffs</i>	324
8.5.	<i>Plot of layoff rate (1965–I/1978–IV)</i>	325
8.6.	<i>Plot of number of hours covered by cassa integrazione guadagni</i>	328
8.7.	<i>Plots of investment in industrial equipment and rate of change of capital-to-labor ratio (K/L)</i>	329
9.1.	<i>Causal structure of an economic model of strikes</i>	349
9.2.	<i>Causal structure of a power model of strikes</i>	350
9.3.	<i>Causal structure of a power model of strikes</i>	352
9.4.	<i>Plots of unemployment rate and numbers of hours of cassa integrazione guadagni</i>	364
9.5.	<i>Plot of the number of strikes in industry</i>	365
9.6.	<i>Plot of the number of strikers in industry</i>	366
9.7.	<i>Plot of the number of hours lost in industry</i>	366
9.8.	<i>Plot of union membership in industry (CGIL + CISL)</i>	367
9.9.	<i>Plot of GNP growth</i>	368

EQUATIONS

(2.1)	<i>The original Ashenfelter and Johnson strike frequency model</i>	32
(2.2)	<i>A respecification of the Ashenfelter and Johnson strike frequency model for the Italian case</i>	32
(3.1)	<i>Extending the Ashenfelter and Johnson model to the numbers of strikers and hours lost</i>	57
(3.2)	<i>Extending the Ashenfelter and Johnson strike frequency model beyond labor-market variables: the effect of product-market variables</i>	67

(3.3) <i>Extending the Ashenfelter and Johnson strike frequency model: combining labor-market and product-market variables</i>	67
(3.4) <i>Problems of simultaneous-equation bias: strike frequency and product-market variables (a detrended, first-differenced model)</i>	70
(4.1) <i>An organizational model of the numbers of strikes, strikers, and hours lost</i>	106
(5.1) <i>A respecification of equation (2.2) to account for the effect of the bargaining structure</i>	164
(5.2) <i>A respecification of equation (3.2) to account for the effect of the bargaining structure</i>	165
(5.3) <i>A respecification of equation (3.3) to account for the effect of the bargaining structure</i>	165
(5.4) <i>A respecification of equation (4.1) to account for the effect of the bargaining structure</i>	165
(5.5) <i>A respecification of equation (5.4) to account for the effect of unexplained observations (1974, 1975, 1977)</i>	170
(6.1) <i>A long-term, political model of the numbers of strikes, strikers, and hours lost</i>	192
(6.2) <i>A short-term, political model of the numbers of strikes, strikers, and hours lost</i>	194
(6.3) <i>Testing the effect of workers' layoffs on the numbers of strikes, strikers, and hours lost</i>	221
(7.1) <i>A respecification of equation (4.1) for the number of hours lost to account for the effect of the 1969 autunno caldo</i>	258
(7.2) <i>A respecification of equation (5.4) for the number of hours lost to account for the effect of the 1969 autunno caldo</i>	258
(7.3) <i>A respecification of equation (6.1) for the number of hours lost to account for the effect of the 1969 autunno caldo</i>	258
(7.4) <i>A respecification of equation (6.2) for the number of hours lost to account for the effect of the 1969 autunno caldo</i>	258
(7.5) <i>A respecification of equation (6.3) for the number of hours lost to account for the effect of the 1969 autunno caldo</i>	259

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 contextitur?
(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

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 disentangle 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

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 legerdemain 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”¹ 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 Ciarciaglino, 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.

Preface

1. For a very empathetic account of the hidden injuries of class, see Sennett and Cobb, 1973.