


University of Oxford

*Department of Politics and International Relations*

Thesis submitted in partial fulfilment of the requirements for the degree of
DOCTOR OF PHILOSOPHY IN POLITICS

---

# Crime and Equality, or Crime and Punishment? Population Heterogeneity and Fear of Crime as Determinants of Redistribution Preferences

**Karl Kahn**
Nuffield College

Word count:
98,927

Hilary Term, 2019

© 2019
Karl Kahn
All right reserved except for those granted under the Creative Commons Attribution-Non Commercial (CC-BY-NC) 4.0 International License

# Crime and Equality, or Crime and Punishment? Population Heterogeneity and Fear of Crime as Determinants of Redistribution Preferences

Karl Kahn
Nuffield College

**Abstract**

Despite its significance as a social, economic and—as I argue below—fundamentally political phenomenon, crime remains a largely overlooked topic in political economy. This dissertation argues that this represents an unfortunate omission. Over the pages that follow, I analyse the political implications of crime and fear of crime for the politics of inequality and redistribution. Modelling fear of crime as a negative externality of inequality, I argue that its consequences depend on *who* the criminals are: building on work in social psychology, I find that the identity of those who are perceived as likely perpetrators of crime significantly affects how criminal behaviour is interpreted. This comes with important implications for views about how crime as a social problem can (and should) be solved. Using a combination of observational data from Western Europe and the US, together with the results from a laboratory experiment conducted in Oxford, I first demonstrate that there exists a robust and systematic positive relationship between the local level of income inequality and fear of crime. In a second step, I show that shared group membership—manifested empirically as low population heterogeneity—facilitates situational explanations of crime, including poverty, while divergent identities promote dispositional attributions unrelated to inequality. As a result, while fear of crime in homogenous contexts is associated with a significant increase in demand for redistribution, particularly among high-income voters, I find no parallel effect in setting that are more diverse. The opposite relationship holds true for support for harsher punishment and law enforcement spending: whereas fear of crime increases support for such policies in settings that are sufficiently heterogenous, the same does not hold true when the level of population heterogeneity is lower. This finding both challenges previous accounts of the relationship between inequality, fear of crime and support for redistribution, and helps to explain cross-national differences in criminal justice policy.

*This page is intentionally left blank.*

# Contents

**Acknowledgments** **ix**

**1 Introduction** **1**
1.1 Redistribution and inequality . . . . . 4
1.2 The Puzzles . . . . . 10
1.2.1 Demand for redistribution and the affluent . . . . . 11
1.2.2 Variation in punitive attitudes . . . . . 14
1.3 The Argument . . . . . 17
1.4 Empirical strategy . . . . . 23
1.5 Overview of the dissertation . . . . . 25

**2 The Argument** **27**
2.1 Taxes and transfers . . . . . 30
2.2 Externalities of inequality . . . . . 36
2.3 Inequality, crime and redistribution . . . . . 41
2.4 Law enforcement and private security . . . . . 45
2.5 Equality or punishment? . . . . . 50
2.6 Private security, sorting and other options . . . . . 60
2.7 Summary and expectations . . . . . 66

**3 Demand for Redistribution and Punishment in Europe** **69**
3.1 Data . . . . . 72
3.1.1 Dependent variables . . . . . 74
3.1.2 Independent variables . . . . . 78
3.1.3 Macro-level control variables . . . . . 83
3.1.4 Individual-level control variables . . . . . 85
3.2 Statistical model . . . . . 87

3.3 Descriptive patterns . . . . . 90
3.4 Results . . . . . 97
3.4.1 Inequality and fear of crime . . . . . 98
3.4.2 Demand for redistribution . . . . . 102
3.4.3 Demand for punishment . . . . . 113
3.5 Robustness tests . . . . . 123
3.5.1 Redistribution preferences . . . . . 123
3.5.2 Punishment preferences . . . . . 129
3.6 Summary . . . . . 133

**4 Crime, Redistribution and Punishment in the United States 135**
4.1 Theoretical background . . . . . 139
4.2 Data . . . . . 144
4.2.1 Dependent variables . . . . . 146
4.2.2 Independent variables . . . . . 150
4.2.3 Individual-level control variables . . . . . 153
4.2.4 Macro-level control variables . . . . . 155
4.3 Descriptive patterns . . . . . 156
4.4 Results . . . . . 164
4.4.1 Inequality and fear of crime . . . . . 165
4.4.2 Demand for redistribution . . . . . 168
4.4.3 Demand for law enforcement spending . . . . . 183
4.5 Robustness tests . . . . . 189
4.5.1 Redistribution preferences . . . . . 189
4.5.2 Law enforcement preferences . . . . . 194
4.6 Summary . . . . . 198

**5 Identity and Crime Preferences: Experimental Evidence 201**
5.1 Experimental design . . . . . 203
5.1.1 Participants . . . . . 203
5.1.2 Design and procedure . . . . . 204
5.1.3 Summary of the experimental design . . . . . 214
5.2 Descriptive patterns . . . . . 216
5.3 Multivariate results . . . . . 224
5.3.1 Identity and relative preferences . . . . . 225

5.3.2 Identity and redistribution . . . . . 234
5.3.3 Identity and punishment . . . . . 241
5.4 Summary . . . . . 245

**6 Conclusion** **247**

**Appendices** **257**

**A Appendix to Chapter 3** **259**
Full regression results . . . . . 259
Details of country-years included in the ESS . . . . . 262
Descriptive statistics . . . . . 263
Relative income . . . . . 264

**B Appendix to Chapter 4** **265**
Full regression results . . . . . 265
Descriptive statistics . . . . . 269

**C Appendix to Chapter 5** **271**
Rational criminals? . . . . . 271
Information sheet . . . . . 280
Paintings . . . . . 281
Instructions . . . . . 282
Descriptive statistics . . . . . 290
Payoff details . . . . . 291
Endowment distributions . . . . . 295

**List of Tables** **298**

**List of Figures** **301**

**Bibliography** **303**

# Acknowledgments

After eight years in Oxford—three as an undergraduate, and now five as a graduate student—I have accumulated a large debt of gratitude. On more levels than one, this thesis and what came before it would not have been possible without the generous help form colleagues, mentors, family and friends along the way.

I would like to begin by thanking David Rueda. Since my early undergraduate tutorials in politics, I have benefited hugely from his unwavering honesty about my work, and his patience with me and my many ideas of varying quality. More than anyone else, he taught me the power of arguments that are not only rigorous but also interesting and important. His encouragement and advice over the years has been invaluable, both as a supervisor and, more generally, as a mentor.

In their different capacities, Ben Ansell and Raymond Duch also deserve recognition. Their detailed comments on earlier versions of this dissertation—for my transfer of status, confirmation of status, and in-between—helped me to move this project forward and to see things in a different, much improved light. In a more indirect way, I also owe much to Walter Mattli. Like few others, he both introduced me to a host of new, fascinating topics in political economy, and provided the encouragement needed to bring this dissertation to its completion.

I am similarly obliged to the Economics and Social Research Council, the Department of Politics and International Relations in the University of Oxford, and the Nuffield Centre for Experimental Social Sciences for funding different parts of my doctorate. My thanks are also due to Nuffield College, which is a remarkable place for aspiring social scientists, and Merton College, where what was to become this dissertation initially took shape. I am also grateful to Yale University and the Department of Political Science for hosting me as a visiting scholar.

For comments, discussions, help and valuable advice in relation to differ-

ent parts of what follows, I am similarly indebted to John Jensenius, Sonke Ehret, Felipe Torres Raposo, Verena Fetscher, Jane Gingrich, Desmond King, Julia du Pont de Romemont, Sofia Knutsson, Elizabeth Pfeffer, Gerda Hooijer, Ceasar Samolov, Bo Rothstein, Shanto Iyengar, Carl Dahlstrom, Johannes Lindvall, Daniel Stegmueller, David Soskice, Pablo Beramendi, Philip Rehm, Lucy Barnes, Spyros Kosmidis, Vera Troeger and Sergi Pardos Prado.

This dissertation aside, a number of others have made my time in Oxford truly special. William Kelley and Gretchen Greer did much to lift my spirits, both literally and metaphorically. The same goes for Alessandro Geraldini and Luca Rottoli, who also taught me much about everything from penguins to tokamaks. Julia du Pont de Romemont—again—was always there when I needed it.

More than most, Jessica Sinyor deserves more recognition than there is space for here. Her willingness—and patience—to listen to my many dissertation woes, and her endless encouragement and support made all the difference over the last few years. They say that doctoral work is a lonely, solitary and perhaps alienating endeavour. Because of Jess my experience was the opposite.

Last but certainly not least, I also need to thank my family—my four brothers, many nieces and nephews, and in particular, my parents, Ulla Kahn and Olle Ljungstrom. In more ways than I can count, they worked as hard as I did towards the completion of this project. It would be impossible to do justice to them here, but hope that what follows might make them think that it was worth it.

# Chapter 1

# Introduction

What is the relationship between inequality and demand for redistribution? Traditional political economy models have a straightforward answer: demand for redistribution will increase with inequality for the simple reason that more inequality implies that more people stand to benefit from redistribution. A central tenet of this argument is that the poor support redistribution while the affluent oppose it. This dissertation, however, suggests that reality is more complicated. Empirically, I demonstrate that although demand for redistribution from low-income voters in advanced industrial democracies is fairly ubiquitous, opposition to redistribution from the affluent is crucially not: whereas the rich in some regions of Europe and the United States are indeed as hostile to redistribution as one might expect, this is far from true everywhere. Theoretically, I argue that this observation is best explained with reference neither to demand for social insurance nor to altruism or other-regarding concerns, but by the relationship between inequality and crime, on the one hand, and that between social identity and crime control preferences, on the other. The former mechanism implies that crime can be modelled as an externality of inequality with negative welfare implications for the affluent. The second, in turn, suggests that the consequences of this depend, simply put, on who the criminals are: identity matters because it affects how crime is interpreted, and consequently for views about how it can be solved. Where the poor belong to the same identity group as the rich, the

latter are more likely to draw a causal inference from inequality to crime, and hence more likely to demand redistribution as a solution to it. Where the poor are more heterogenous, by contrast, the affluent become more likely to attribute crime to dispositional traits (see Heider, 1958)—in essence, to adhere to the view that criminals are, in some sense, *bad* people—and consequently more prone to support punitive solutions to crime, rather than redistribution.

The main take-home message of this dissertation, therefore, is perhaps both counterintuitive and somewhat discouraging: while the affluent become more supportive of redistribution as inequality increases in homogenous settings, they respond to inequality by becoming more punitive—that is, they demand harsher punishments, longer prison sentences, and more policing as protection against crime—when the poor are, in some significant and salient sense, different from themselves. As such, the theory and empirical analysis presented in this dissertation makes two main intellectual contributions. First, it adds nuance and qualifications to two important literatures in comparative political economy about the determinants of redistribution preferences: one emphasising the relationship between inequality, relative income and the demand for redistribution, the other highlighting the potential for population heterogeneity, immigration and concerns related to social identity to act as inhibitors of social policy preferences. The argument developed below, in short, suggests that these contributions have overlooked an important causal mechanism—the interaction between crime and heterogeneity—linking inequality and identity to demand for redistribution. Second, in specifying a direct relationship from social identity, group status and population heterogeneity to specific attitudes towards crime control, it also provides new insights for the criminology and sociology literatures studying how such attitudes are formed. As I discuss below, most previous arguments in this vein focus on racial animosity and stereotypes in the US, often combined with specific or historicist supply-side conditions (the 'war on drugs', the Republicans' southern strategy, and so on), and thus propose theories that do not necessarily travel to other contexts. The argument developed in this dissertation, by contrast, implies that the US experience not unique.

More substantively, the major themes emphasised in this dissertation—inequality, social identity and immigration, crime, and redistribution—are also far from peripheral. For a start, to suggest that inequality is an increasingly important social phenomenon does not require much argument (but for informative

examples, see especially Atkinson, 2015; Stiglitz, 2013). Accordingly, it is not surprising that the political consequences (and, perhaps, causes) of inequality has become a major research topic in comparative political economy, together with its structural roots in the transition away from a 'Fordist' mode of production to what is variously referred to as the 'knowledge economy', the 'new economy', the 'service economy', or for short, late capitalism (see, for example, the contributions in Beramendi et al., 2015). No doubt, this growing interest in inequality stems both from fundamental normative considerations—is inequality wrong?—and from the empirical relationship between inequality and a diverse range of social and political outcomes. The latter are too numerous to list, but range from democratic or regime stability (Boix, 2003; Acemoglu and Robinson, 2005; Muller, 1988, 1995, see also Ansell and Samuels, 2010, 2014) to political participation and influence (Solt, 2008; Bartels, 2008; Gilens, 2012), economic growth (Perotti, 1996; Barro, 2000), public health (Kahn et al., 2000; Wilkinson and Pickett, 2006) and importantly from the point-of-view of this dissertation, crime and fear of crime (Becker, 1968). In terms of the former, on the other hand, the concern for inequality stems not least from its relationship to moral luck. Since many (if not most) of the factors that determine an individual's relative income or wealth are, at least partially, arbitrary—that is, they are due to chance rather than to choice or effort: think genes, perhaps intelligence, parents, inherited wealth, and so on—large inequalities in outcomes appear stand in conflict with the commonly held notion that only deserved material differences between individuals can be morally justified (Rawls, 1971; Dworkin, 1981a,b; Roemer, 1996; Cohen, 1989; but compare, for example, Nozick, 1974; Anderson, 1999). Despite their differences, these perspectives all share the view that inequality matters.

Crime and punishment, by contrast, have received much less attention by political economists—and by political scientists in general—with some notable exceptions (for example Lacey, 2008, 2010; Beckett and Western, 2001; Lacey and Soskice, 2015). Instead, much of what we know about the politics of crime and punishment come from work in criminology, sociology and, to some extent, economics. This omission is both surprising and ultimately unfortunate, for several reasons. First, crime and its consequences—for victims, perpetrators and others—are arguably important social outcomes. Across both Europe and the US, both incarceration and crime rates vary widely—albeit not always in parallel, and with significant differences both cross-sectionally and within countries

over time. These patterns deserve an explanation, and part of that explanation is arguably political. Yet, and as accurately noted by Lacey and Soskice (2015), although much of the criminology and sociology literatures on crime policy and punishment mentions politics, it '…rarely scrutinises the relevant preferences and interests, and the political institutions through which criminal justice preferences and policies are constructed and filtered over time' (pp. 546). As a result, little is known about the drivers of criminal justice policy, either in terms of how preferences regarding crime and punishment are formed, or how they are channelled by supply-side factors to produce political outcomes. Without professing to accomplish all of this, the argument and analysis developed in this dissertation represent one attempt at correcting this shortfall.

The remainder of this chapter is structured as follows. The next section sets the scene, by describing some of what we know about the relationship between inequality and redistribution, together with some of the things that we might still not know. Section 1.2 outlines the more specific empirical puzzles this dissertation tries to explain, focusing on variation on the redistribution preferences of the rich, on the one hand, and cross-national differences in punitive and other crime related attitudes, on the other. Section 1.3 summarises my theoretical argument, and Section 1.4 describes the empirical strategy used to test it. Section 1.5, finally, provides an overview of the remaining chapters.

## 1.1 Redistribution and inequality

At the onset, it is important to acknowledge that the question of how inequality matters for redistribution is of course not new. Rather—and together with the more fundamental question of how it is possible to combine democracy, as a political system based on a principle of formal political equality, and capitalism, as an economic system that (at least when left to its own devices) tends to produce vast and persistent inequalities in income, property and wealth—it has been at the forefront of comparative political economy since its inception as a discipline, and has occupied the minds of theorists since Aristotle.

Intrinsic to much of this debate is the idea—or, depending on who you ask, either hope or fear—that not only are democratic political institutions hard to reconcile with significant inequalities in the economic sphere, but that economic equality should follow more or less logically from political equality. The rea-

soning behind this argument is simple, and admittedly compelling. It goes as follows. Democracy, by combining the principles of majority rule and—at least formally—equal political rights, grants political power to the majority of citizens. And since in any country with a skewed income or wealth distribution, there will always be more poor people than there will be rich people, it follows that there will always be more people who stand to benefit from redistribution—the majority—than people who stand to be harmed by it. Hence, simple material self-interest implies that inequality should be followed by demand for redistribution, which in a democracy should translate into actual redistribution. Or expressed as comparative statics: in a democracy, increasing inequality should lead to increased demand for redistribution, which in turn should produce a greater supply of redistribution. In other words, the formal political equality inherent to democracy—because it grants the poor the power to 'soak the rich', as the common saying goes—ought to bring with it economic equality.

As Przeworski (1986, 2009, 2010), Shapiro, Swenson, and Donno (2008), Ansell and Samuels (2014) and many others have noted, the validity of this syllogism—moving from the seemingly plausible premises of political equality and self-interested redistribution preferences to economic equality—has, unlike most things, long been accepted by socialists, conservatives, libertarians, and people of most other political and moral persuasions alike. It has been used in arguments made by proponents of democracy, as well as in those made by its sceptics. For the late Marx, having witnessed the failure of the revolutions of 1830 and 1848, democracy became a stepping stone: as he said, the distributional conflicts inherent to it would 'unchain the class struggle' and facilitate the development of the kind of proletarian class consciousness that had proved to be missing (Marx, [1850] 1934). The same sentiment, and the idea that with political equality comes economic equality, was echoed by later socialists competing in democratic elections not merely as a means to an end: in France, Jean Jaures proclaimed that 'the triumph of socialism will not be a break with the French Revolution, but the fulfilment of the French Revolution in new economic conditions', and in Germany, Eduard Bernstein described socialism as 'democracy brought to its logical conclusion' (both cited in Przeworski, 2010). On the other side of the political spectrum, the same assumed relationship between inequality and redistribution under democracy has, for at least as long, been a source not of great anticipation but of great anxiety. Echoing concerns going back to

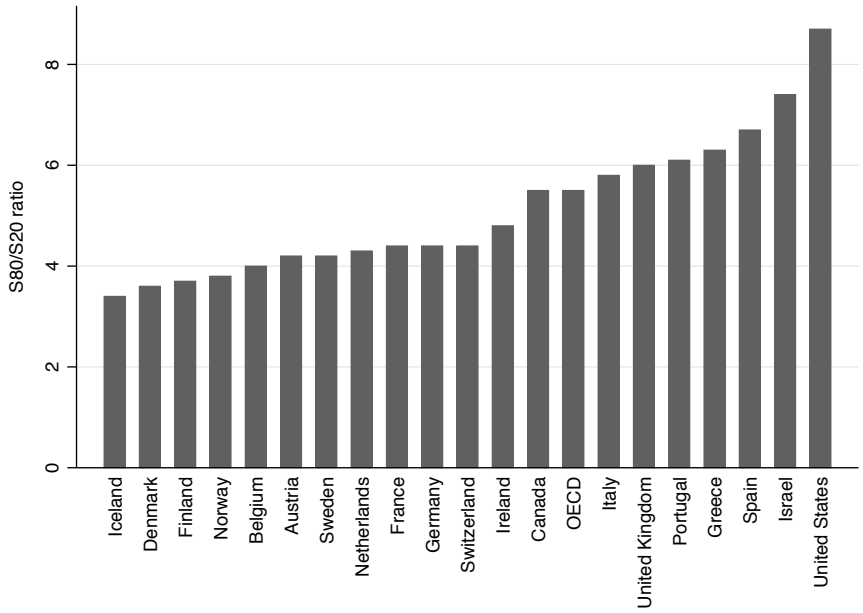
Aristotle's *Politics* (see Book 3, Chapter VII), James Mackintosh argued, in 1818, that if the franchise was expanded to the propertyless 'a permanent animosity between opinion and property must be the consequence', and in 1848, Thomas Macaulay said—in response to the same revolutionary wave that led Marx to endorse democracy, for similar reasons—that 'universal suffrage is incompatible, not only with this or that form of government, and with everything for the sake of which government exists …it is incompatible with property and it is consequently incompatible with civilisation' (in Przeworski, 2009). Others, ranging from David Ricardo to James Madison and J.S. Mill, voiced similar concerns.

More recently, Meltzer and Richard (1981, see also Romer, 1975) formalised this intuition, using a simple model of self-interested redistribution preferences combined with Downs' (1957) median voter theorem, into what has become the 'workhorse' or 'canonical' model of inequality and redistribution in political economy. Summarised briefly, their argument models individual labour-leisure choices in a one-period economy, where the government redistributes a lump-sum transfer to all citizens financed through a linear income tax. Individuals derive utility from consumption and leisure only, and have different productivities which determine their market income. Allowing for tax disincentive effects—meaning that there are diminishing revenue returns to taxation—the model moves away from corner solutions, and implies that voters below the mean will demand redistribution up to the point where its benefit to them is outweighed by the deadweight costs of taxation. Politically, the equilibrium outcome therefore is the point where the marginal benefit of redistribution equals its marginal cost to the median voter, which increases with the income distance between the mean and the median. Thus, the model—again—leads to the well-known prediction that inequality and redistribution are positively related: any change in the level of inequality that increases the difference between mean and median income should be followed by an increase in the supply redistribution. Cross-sectionally, this also means that we should find the highest levels of redistribution in the countries with the most right-skewed pre-tax income distributions, ceteris paribus.

Given this history, it is perhaps surprising that the empirical relationship between inequality and redistribution remains not only deeply contested, but in important ways poorly understood. Superficially, a cursory look at patterns of inequality and redistribution in the contemporary advanced industrial democracies reveals a picture that stands in reasonably stark contrast to the argument that

democracy and equality necessarily go together, at least in any straightforward or deductive way. On the one hand, large inequalities in disposable income persist, both in the established democracies of Western Europe and North America—most of which have now had proper democratic institutions for what is approaching century—and in the younger democracies of the developing world. Figure 1.1 provides a first illustration of this, by showing the ratio between the share of total post-tax post-transfer income received by those in the top quintile of the income distribution (that is, by those in the 80th percentile or above), and the share received by those in the bottom quintile (20th percentile or below), for a sample of OECD countries. In the US, the income share of the top quintile is more eight times that of the bottom quintile. This difference is smaller in Europe, but still remains far from negligible: in the UK, the same S80/S20 ratio is around

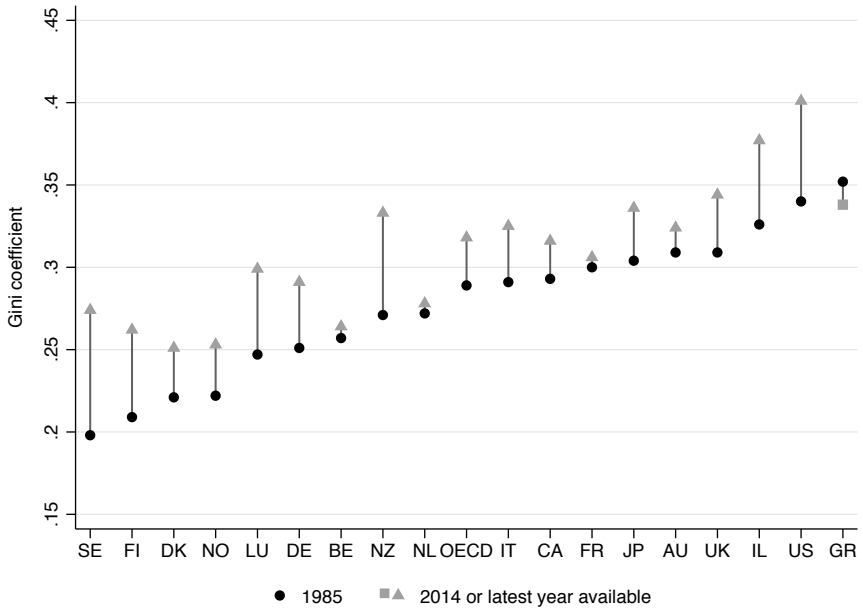
**Figure 1.1:** Income inequality in the OECD (S80/S20 ratios)




*Note:* OECD data (OECD, 2019a) Estimates are for 2014, or for the latest year available. The 80/20, or S80/S20, ratio gives the share of total income received by those in the top quintile of the income distribution divided by the share of total income received by those in the bottom quintile, after taxes and transfers. It can also be interpreted as the ratio between the average income in the top quintile and the average income in the bottom quintile.

six, in both France and Germany it is 4.4, and even in all of Scandinavia it is well above three. As is well known, moreover, these differences are increasing rather than diminishing. Figure 1.2 summarises these developments by plotting post-fisc Gini coefficients for the same set of countries, for 1985 and 2014 (or the latest year available). The trend is unequivocal: in all countries but Greece, inequality was higher at the end of this period than it was at the beginning. In 1985, the average Gini coefficient in this sample was around 0.29, but by the mid-2010s this had increased by around 10 percent to 0.33. In broad terms, this pattern has been driven by two main forces: on the one hand, a rapid and sustained increase in the income share of the top 10% of earners (and even more so for the top 1%), and on the other, stagnating real incomes in the lower half of the income distribution. The former trend is particularly dramatic. In the US, where matters are again the most extreme, tax return data suggests that the

**Figure 1.2:** Income inequality in the OECD, 1985 and 2014 (Gini coefficients)




*Note:* Changes calculated from OECD data (see OECD, 2015, 2019a). Inequality estimates are for disposable income, that is, after taxes and transfers. The Gini coefficient, or Gini index, is a summary measure of inequality, and theoretically ranges from near 1 (indicting near perfect inequality) to 0 (indicating perfect equality). See the main text for details.

income share of the richest percentile increased from around 8.3% in 1975, to well over 22% in 2015. By comparison, over the same period the income share of the bottom 50% of earners declined, from just above 18.2% to approximately 10%. These figures are again less exceptional in Europe, but still fairly striking: in the UK, the top percentile now accounts for around 13.9% of total income (up from 6% in the mid-1970s), in Germany for 13%, and in Sweden for just below 9% (WID, 2019). It goes without saying that from the perspective of the argument discussed above, these developments are indeed puzzling.

As I discuss in some detail in the following chapter, the empirical relationship between inequality and redistribution is also disputed in the more systematic comparative political economy literature. For one, and in contradiction to 19th century hopes and fears, nearly all cross-sectional studies have failed to produce support for the expectation that more unequal countries should redistribute more (Iversen and Soskice, 2006, 2009; Moene and Wallerstein, 2001, 2003; Lindert, 2004; Alesina and Glaeser, 2004; Shelton, 2007, and many others). This result—variously referred to as the 'paradox of redistribution' (Dimick, 2015) or the 'Robin Hood' puzzle (Lindert, 2004; Iversen and Soskice, 2009)—has in turn generated a large body of scholarship seeking to establish both where the Meltzer-Richard model goes wrong (a 'cottage industry', in the words of Ansell and Samuels, 2014), and what alternative, theoretical reasons there are for expecting a negative rather than a positive relationship between inequality and redistribution. On the other hand, studies emphasising time-series variation in inequality and redistribution within countries have more consistently found evidence for a positive relationship between these variables (Milanovic, 2000; Kenworthy and Pontusson, 2005, but for contrasting results see Kenworthy and McCall, 2008, also Cavaille, 2014).

Nonetheless, the importance of these macro-level empirical results for how we think about the relationship between inequality and demand for redistribution should not be overstated, for several reasons. As I argue below, the rejection of the RMR-model on the aggregate-level (as the finding that countries that are more unequal do not, in fact, redistribute more) does not imply that relative income is not an important determinant of individual-level redistribution preferences. It is perfectly possible for the latter to hold true while the former does not. Indeed, many alternative explanations are possible: from the well-known, inherent limitations of the median voter theorem to the relationship between inequality and turnout, factors to do with partisanship and party strategies, and the potential

multi-dimensionality of preferences. Thus, even taking the lack of macro-level evidence for the Meltzer-Richard model as given, this conclusion should not be extrapolated to the preferences-side without further clarification or argument.

To begin at the beginning, therefore, if we want to understand how inequality affects redistribution, then looking at the relationship between inequality and popular demand for redistribution is a natural starting point (Brooks and Manza, 2006, 2007; Kang and Powell, 2010; Svallfors, 1997, 2004). If the material self-interest hypothesis does not hold up on the individual-level, either in whole or in part, then the puzzle described over the previous paragraphs immediately becomes less puzzling. If it does stand up to empirical scrutiny, by contrast, then it follows that the explanation for the discrepancy between theory and reality is to be found elsewhere. Still, and although the recent growth in scholarship on the determinants of redistribution preferences is impressive, important questions with significant implications for how we think about the macro-level relationship between inequality and redistribution remain controversial. Are views regarding redistribution primarily a matter of material self-interest (Romer, 1975; Meltzer and Richard, 1981; Iversen and Soskice, 2001; Moene and Wallerstein, 2003; Rehm, 2009), are other-regarding preferences more important (Shayo, 2009; Fehr and Schmidt, 2006; Lupu and Pontusson, 2011), or do both types of considerations matter (Dimick, Rueda, and Stegmueller, 2016; Rueda, 2018)? Do the poor have higher redistribution preferences than the rich (Finseraas, 2009; Rueda and Stegmueller, 2016; Page and Jacobs, 2009; Gilens, 2005), or are the rich more supportive of redistribution than the poor (Moene and Wallerstein, 2001; Alesina and Glaeser, 2004)? And if macro-level inequality does indeed matter for redistribution preferences, then which causal mechanisms link it to the micro-level? Given that redistribution preferences are the building blocks of most political economy arguments, moreover, these questions are also consequential for explanations of outcomes as diverse as varieties of capitalism, the welfare state, partisanship, voting behaviour, and democratisation, to name but some.

## 1.2 The Puzzles

More narrowly, this dissertation is oriented around two distinct empirical puzzles. As I have said above, one concerns the redistribution preferences of the affluent, and variation in the difference between the redistribution preferences of high-

and low-income groups. The other, in turn, has to do with similar variation in punitive preferences, and heterogeneity in the relationship between fear of crime and support for harsher and more punitive forms of criminal justice policy.

### 1.2.1 Demand for redistribution and the affluent

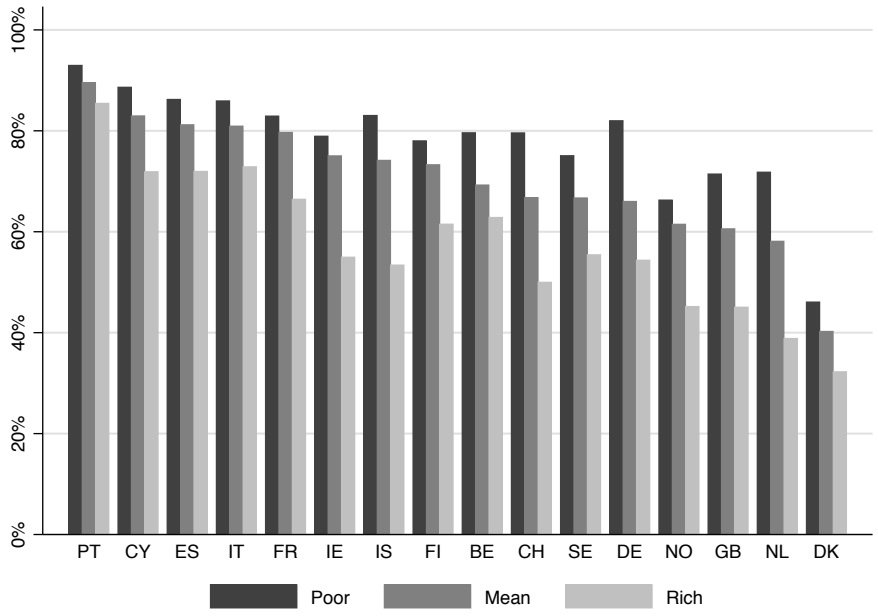
An important yet open question in the comparative political economy literature concerns the large amount of cross-sectional variation in the relationship between income and redistribution preferences. In some countries and in some regions within countries, the core individual-level prediction of the Meltzer-Richard model—that demand for redistribution declines with relative income—closely matches up with reality: the poor support redistribution while the affluent oppose it. Elsewhere, on the other hand, this is much less evidently the case. As a consequence, the redistribution preferences of the rich and the poor are markedly different in some places, but much more similar in others. Moreover—and as demonstrated more systematically in later chapters—most of this variation appears to be driven by the preferences of the relatively affluent: whereas support for redistribution from those with low incomes is reasonably stable, the same does not hold true about opposition to redistribution from the rich. This observation comes with important consequences for how we think about the issues raised in the previous section, yet has received comparatively little scholarly attention.

It also raises two more specific questions. First, why would the affluent, who do not have a plausible tax-and-transfer motive for supporting either progressive taxation or redistribution, ever demand or support such policies in the first place? And second, why does the extent to which they do so—or equally, the importance of relative income for redistribution preferences—vary so much from one place to the next? Without disputing the possibility that other explanations (the most prominent of which are discussed in some detail in Chapter 2) may also be important, the argument of this dissertation offers one possible set of answers: that the key to understanding these patterns has to do with the relationship between inequality and fear of crime, on the one hand, and that between population heterogeneity and preferences over alternative means of reducing crime, on the other.

To provide an initial illustration of this empirical point, Figure 1.3 plots the average level of support for redistribution among the rich (defined as those in or above the 80th percentile of the national income distribution) and the poor (as

those with incomes in or below the 20th percentile) in 16 West European democracies, using data from the 2008, 2010, and 2012 waves of the European Social Survey (ESS). For each country, the bars represent the share of respondents in each income category that either 'agree' or 'strongly agree' with the statement that 'the government should reduce differences in income levels'. Although these statistics should be interpreted with some degree of caution—in particular, it should be kept in mind that supporting redistribution in a country that already is highly redistributive, in Denmark for example, may mean something quite different from supporting redistribution in a country that is less redistributive, such as the UK—they highlight the two important and interesting empirical observations discussed in the previous paragraph. The first is that whereas the poor are more supportive of redistribution than the rich in all countries in this sample, the degree to which this is the case shows an impressive amount of cross-national variation. In these countries, the difference in the share of pro-

**Figure 1.3:** Support for redistribution and income in Europe

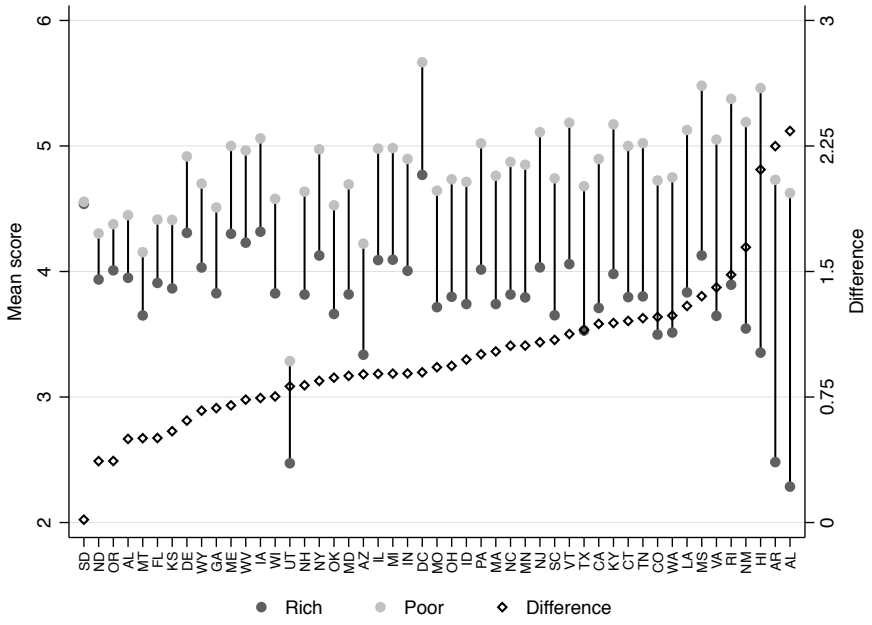



*Note:* European Social Survey data, 2008-2012. The 'rich' are defined as those in the 80th percentile of above, and the poor as those in the 20th percentile or below. The bars represent the share of respondents in each income group that support redistribution.

redistribution respondents between the two income groups ranges from 32.5 percentage points in the Netherlands to 7.5 points in Portugal, with the remaining countries spaced relatively evenly between these two extremes. Second, this variation appears to be driven less by the preferences of the poor, and more by those of the rich: whereas in excess of 70% of the poor in all countries but Denmark and Norway support redistribution, the preferences of the rich are much more varied. In Portugal, more than 80% of the rich support redistribution, in six countries between 60% and 80% do so, in seven countries the same share is between 40% and 60%, and in two countries it is below 40%.

This pattern is furthermore not limited to Western Europe. Figure 1.4 plots support for redistribution by income group in the US, for each state plus the

**Figure 1.4:** Support for redistribution and income in the US




*Note:* GSS data, 1978-2010. The 'rich' are defined as those in the 80th percentile or above, and the poor as those in the 20th percentile or below. The dots (scale on the left $y$-axis) represent the average score in either income group in each state on a seven-point scale measuring responses to the question of whether 'the government ought to reduce income differences between the rich and the poor', with higher scores indicating greater support. The diamonds (scale on the right $y$-axis) gives the difference between the average score among the poor and the rich. See Section 4.2.1 on page 146 for a more thorough discussion of the survey item used to derive these statistics.

District of Columbia, based on pooled cross-sectional data from the General Social Survey (GSS) covering the time-period from 1978 to 2010. As before, the rich are defined as those in the top quintile (with an income of $23,560, in 2010 dollars, above the mean or more), and the poor as those in the bottom quintile (earning $37,080 or less below the mean) of the national income distribution in a given year. In Figure 1.4, dots represent the average response, among the rich (dark grey) and the poor (light grey) in each state, to a survey item asking whether respondents agree that 'the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor', measured on a seven point scale where higher scores represent greater agreement.

Although the preferences of the poor admittedly show greater variation than in the European data, the key points discussed above are still clearly visible. First, there is a clear relative income effect on preferences: in all states, average support for redistribution is higher among the poor than it is among the more affluent. Once again, however, there are significant between-state differences in this income-redistribution relationship. In Alaska, Arkansas, Hawaii and New Mexico, on one end of the spectrum, the rich and the poor are very dissimilar indeed—with differences of 2.31, 2.25, 2.11 and 1.64 respectively. Elsewhere, on the other hand, this difference is much less impressive: in Oregon it is 0.37, in North Dakota it is 0.36, and in South Dakota it is 0.02—essentially zero. As discussed in the next section, the argument of this dissertation offers one possible explanations for these outcomes: it suggests that crime matters more to the preferences of the affluent than it does to those of the poor, and shows that the patterns noted above can be accounted for by the relationship between crime and population heterogeneity.

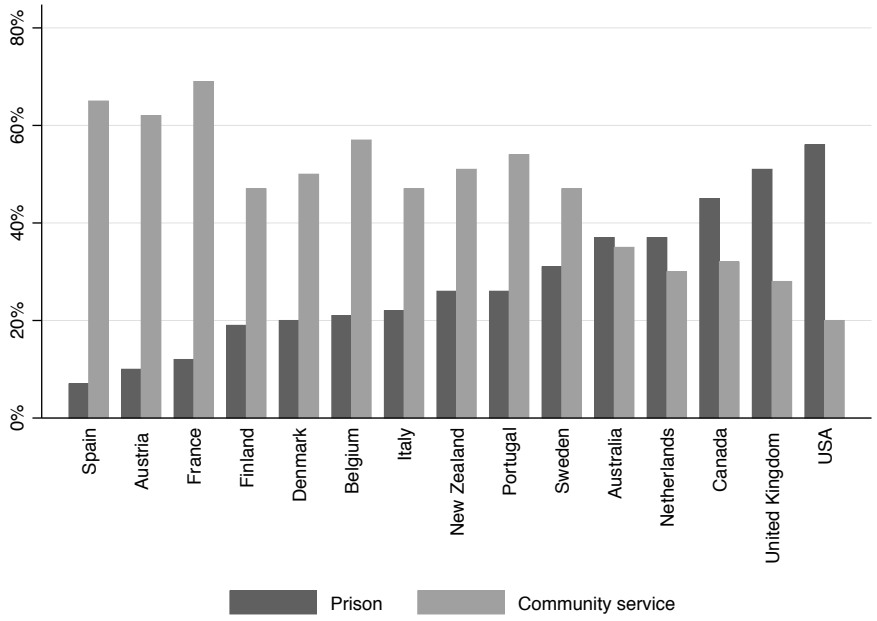
### 1.2.2 Variation in punitive attitudes

The second puzzle concerns the similarly large degree of variation in popular support for harsher punishment and other forms of punitive criminal justice policy. As noted in much of the previous literature and documented throughout the later chapters of this dissertation, while support for such criminal justice measures is more than widespread in certain places, it remains conspicuously low in others. What is more, most of this macro-level variation cannot be easily

explained either with reference to actual crime rates, or in terms of self-reported perceptions of fear of crime—at least not without further qualification.

Figure 1.5 provides a very initial empirical illustration of this fact, using data from the International Crime Victimisation Survey (ICVS). The ICVS measures punitive attitudes by asking what sentence respondents think would be appropriate for a 21-year old man who has stolen a television set, and is found guilty of theft for a second time (Mayhew and van Kesteren, 2002). Using this item, the bars in Figure 1.5 represent the percentage of respondents in each county who have answered that the burglar should be sentenced to prison (dark grey bars) and community service (light grey). Of these two alternatives, prison is undoubtedly the more punitive option. Looking at the data presented in Figure 1.5, the most striking thing to note is the impressive amount of cross-national variation in these attitudes. In Spain, respondents suggesting that the man should be given community service outweigh those answering that he should be given a prison

**Figure 1.5:** Cross-national differences in crime control preferences




*Note:* ICVS data, adapted from Mayhew and van Kesteren (2002). The bars represent the percentage of respondents who support either prison or community service for a 21-year old man who has stolen a television set, and is convicted of theft for the second time.

sentence by nearly a factor of ten. In Austria, they do so by a factor of around six, and in France, those preferring community service are more than five times as numerous as those preferring imprisonment. Elsewhere, however, a very different picture emerges: in the US, 56% of respondents support a prison sentence compared to only 20% preferring community service, in the UK, 51% prefer prison and 28% community service, and in Canada, the corresponding numbers are 45% to 32%. These aggregate-level statistics hide a great deal of lower-level variation, but they do point to an important phenomenon that demands explanation.

The question of where punitive attitudes come from—and consequently, why citizens of some countries appear to be so much more punitive than those of others—has, of course, received significant attention in both sociology and criminology. It has also been addressed by psychologists. Broadly speaking, and following the terminology suggested by Tyler and Boeckmann (1997), arguments in this literature can be divided into two main groups: those that emphasise instrumental concerns, including fear of crime, and those that see punitiveness as something inherently emotive, expressive or symbolic. Leaving aside a more detailed discussion of this distinction for the following chapter, an important finding in the former literature is that fear of crime and punitiveness do not appear to be systematically related: although some studies do find evidence for a positive relationship between fear of crime and support for harsher punishment (Langworthy and Whitehead, 1986; Costelloe et al., 2002; Costelloe, Chiricos, and Gertz, 2009; Dowler, 2003), others have arrived at the opposite conclusion (Tyler and Boeckmann, 1997; King and Maruna, 2009; Kleck and Jackson, 2016; Barkan and Cohn, 1994). Given the intuitive appeal of the logic linking fear of crime to punitiveness, in turn, this result is admittedly puzzling.

The argument summarised below offers an explanation for these inconsistencies. As discussed in the next section, there are persuasive theoretical reasons for expecting the relationship between perceptions of crime—including fear—and punitiveness to depend on the social identity of potential perpetrators: in particular, because identity matters for how crime is interpreted and causally attributed, it comes with important implications for the relationship between fear of crime and preferences over alternative means of dealing with crime. Later chapters demonstrate this empirically: although fear of crime is consistently linked to punitiveness in contexts of high population heterogeneity, it has no similar effects where the population is more homogenous.

## 1.3 The Argument

To explain these patterns, the theoretical argument of this dissertation makes four central points about the formation of preferences for redistribution and law enforcement. Taken together, they emphasise the relationship between inequality and fear of crime, on the one hand, and that between population heterogeneity and fear of crime as determinants of individual preferences over redistribution and punitive forms of criminal justice policy, on the other. This argument is developed in greater detail in Chapter 2, but summarised in the pages below.

First, I start from the assumption that simple, economic self-interest is an important driver of redistribution preferences. Although it is clear—as the previous discussion has shown—that such considerations are most certainly not all that matters, this theoretical setup nevertheless provides a useful point of departure for the subsequent parts of the argument. Material self-interest can be considered in static terms or inter-temporally. The latter perspective directs attention to the relationship between redistribution and lifetime income, and thus emphasises the importance of risk (for example, Moene and Wallerstein, 2001, 2003; Rehm, 2009), skill profiles and human capital investments (Iversen and Soskice, 2001; Hall and Soskice, 2001), and prospects of upward social mobility (Benabou and Ok, 2001; Alesina and Giuliano, 2011) for individual redistribution preferences. While agreeing that these considerations are important, the baseline model developed below focuses on the arguably simplest form of material self-interest: present post-tax and post-transfer income maximisation. Given that redistribution implies, as the term suggests, redistribution from those with high incomes to those with lower incomes, this means that I expect an individual's relative position in the income distribution to be an important determinant of his or her demand for redistribution. The comparative political economy literature aside (Finseraas, 2009; McCarty, Poole, and Rosenthal, 2006; Rueda and Stegmueller, 2016, see also Huber and Stanig 2009), moreover, this basic expectation should strike most people as reasonable.

As such, the starting point for the argument is very similar to the demand-side logic of the models of redistributive politics proposed by Romer (1975) and Meltzer and Richard (1981): assuming that (i) individuals have preferences only over consumption and leisure, (ii) the government redistributes an equal lump-sum transfer to all citizens financed through a linear income tax, and (iii) there are

certain deadweight or efficiency costs to taxation, it follows that demand for redistribution will be a negative function market income up to the mean. On these accounts, all citizens with an income below the mean will demand some positive amount of redistribution and prefer a non-zero tax rate, whereas everyone with an income above the mean will prefer zero taxation with no redistribution. The intuition, once again, is straightforward: the poor will support redistribution because it makes them better-off in pure post-tax post-transfer income terms (that is, they pay less in taxes than they receive in transfers), and the rich will oppose redistribution because it makes them worse-off by the same token (they pay more than they receive). An important implication of this framework is that individuals' preferred level of redistribution and taxation declines monotonically over the entire lower half of the income distribution (since the tax rate is bounded at zero): that is, an individual in the 5th percentile will demand more redistribution than someone in the 10th percentile, just like someone in the 40th percentile will demand more redistribution than someone in the 45th percentile, and so on. Holding pre-tax income constant, moreover, this logic suggests that individuals will demand more redistribution following any change in inequality that increases the distance between their own income and the mean, provided the difference is negative.

The second part of the argument distinguishes these tax-and-transfer motivations from those having to do with the externalities of inequality. The former implies that inequality matters because it affects whether and to what extent a particular individual has an income maximisation motive for demanding redistribution. The latter mechanism, by contrast, emphasises that inequality can also matter because of its other consequences, or externalities. Theoretically, this idea is relatively simple: if inequality promotes specific externalities with clear individual-level welfare implications, it is reasonable to expect redistribution preferences to be affected. Individuals who are adversely affected by the negative externalities of inequality have reason to demand more redistribution than they would otherwise have done, while the opposite holds true for those benefited by inequality. The initial plausibility of this logic is easy to appreciate. As noted earlier in this chapter, a voluminous literature has documented the relationship between inequality and a diverse range of outcomes, ranging from democratic or regime stability (Boix, 2003; Acemoglu and Robinson, 2005; Muller, 1988, 1995, see also Ansell and Samuels 2014) to political participation and influence (Solt, 2008; Bartels, 2008, 2009; Gilens, 2005), economic growth (Perotti, 1996;

Barro, 2000), public health (Kahn et al., 2000; Wilkinson and Pickett, 2006) and, importantly, crime (Becker, 1968; Bourguignon, 2001). To the extent that these effects have discernible individual-level implications, it follows that they are consequential for how we think about the relationship between inequality and demand for redistribution. Yet, and with few but notable exceptions (Rueda and Stegmueller, 2016, see also Imrohoroglu, Merlo, and Rupert 2000; Alesina and Giuliano 2011), this causal path from inequality to redistribution preferences have received relatively little attention from previous authors.

Paralleling the model proposed by Rueda and Stegmueller (2016), the theoretical argument proposed in this dissertation centres on the implications of one particular externality of inequality: crime. There are several reasons for this choice. First, there are theoretically both strong and fairly intuitive reasons for expecting crime and inequality to be positively related (Becker, 1968; Ehrlich, 1973; Bourguignon, 2001; Bourguignon, Nunez, and Sanchez, 2003; Freeman, 1996), and empirically there is ample evidence to suggest that this is indeed the case (Fajnzylber, Lederman, and Loayza, 2002a; Dahlberg and Gustavsson, 2008; Imrohoroglu, Merlo, and Rupert, 2000; Kelly, 2000). A second reason is empirical. Unlike what is the case with some of the other potentially relevant externalities of inequality, the welfare implications of crime are reasonably clear-cut: crime is unambiguously an economic 'bad', both for actual victims of crime and for those who feel afraid of crime. More practically, it is also notable that measures of both victimisation and fear of crime are readily available in several of the large-scale surveys that allow for a rigorous analysis of redistribution preferences, meaning that testing the externality mechanism using crime is therefore comparatively straightforward. The perhaps most important reason for focusing on crime, however, is—once again—that it remains a topic that has been markedly ignored by the previous political science literature. Little is known about how crime matters for political preferences, and political science has produced few plausible explanations for why both public opinion about crime and punitive policy outcomes show such significant variation between countries. The analysis developed in later chapters shows that this is an unfortunate omission. Nevertheless, exploring the importance of alternative externalities of inequality is a natural continuation of the argument and analysis proposed in this dissertation.

To anticipate my choices below, it is important to emphasise that while most of the economics literature on inequality and crime focuses on objective

crime rates, it is fear of crime—as a subjective assessment of the probability of victimisation—that matters more to my argument. This point is important, since it is well-established that fear of crime and criminal victimisation are both analytically distinct and, in practice, weakly correlated: old women, for example, tend to exhibit high fear of crime but low victimisation risk, while the opposite holds true for young men (Box, Hale, and Andrews, 1988). There are good reasons, however, for expecting inequality to matter for actual crime and fear of crime alike. As I argue below, my argument requires only that individuals who are afraid of crime have some understanding of objective crime rates, and consequently that inequality and fear of crime are positively related. This logic holds even if the relationship between actual victimisation risk and perceived fear of crime is imperfect. In line with the results of previous studies (see, for example, Vieno, Roccato, and Russo, 2013; Vauclair and Bratanova, 2017), finally, this expectation—that the level of inequality affects not only the incidence of actual crime, but also fear of crime—receives significant support from the empirical analysis developed below.

Taken together, these points suggest that inequality matters—or more precisely, has the potential to matter—for redistribution preferences though two distinct causal mechanisms: first, by determining tax-and-transfer motivations for demanding redistribution, and second, by producing negative externalities, including fear of crime, for which redistribution is a possible solution. The first implies that there should be a negative relationship between relative income and demand for redistribution, and the second suggests that net of such concerns, inequality can increase support for redistribution among those fearful of crime.

How do these causal mechanisms relate to each other? Like that proposed by Rueda and Stegmueller (2016), the argument developed below offers a straightforward answer: the potential importance of crime for redistribution preferences increases with income. A different way to put this is to say that security is a luxury good, meaning that demand for it increases more than proportionally with real income. To see why this might be the case, it is important to recognise that there is a temporal difference between demand for redistribution as income maximisation and demand for redistribution as a solution to its negative externalities. Whereas the former is fairly immediate, the latter is more long-term. This difference is particularly pertinent when it comes to crime, moreover, since part of its welfare effects are inherently probabilistic: that is, the relevance of crime to present choices hinge on the expected future costs of being the victim of crime, discounted by

the probability of not being victimised. This means, in turn, that variation in time preferences become central to understanding why the two mechanisms discussed above are of differential importance to individuals in different parts of the income distribution. As a large empirical literature spanning economics, sociology and psychology has shown, the future discount rate tends to vary inversely with income: in short, individuals with relatively low incomes tend, at least in the aggregate, to be more present-oriented in their economic behaviour compared to those who are materially more fortunate (Harrison, Lau, and Williams, 2002; Reimers et al., 2009; Dynan, Skinner, and Zeldes, 2004; Tanaka, Camerer, and Nguyen, 2010). Given the temporal difference between the two motives for demanding redistribution, this finding gives reason to expect the importance of crime to redistribution preferences to increase in income, all else equal.

This is, however, not the end of the theoretical story—quite the contrary, in fact. Redistribution is, of course, not the only means through which crime can be addressed, but rather one out of several plausible options. The standard rational-choice analysis of crime points to at least two important alternatives: (i) public security and law enforcement, including policing and punishment administered through the legal system (Becker, 1968; Freeman, 1996), and (ii) private self-protection, meaning individual efforts aimed at lowering the risk and associated cost of being the victim of crime (for theoretical discussions, see Shavell, 1991; Hui-Wen and Png, 1994; Clotfelter, 1977, 1978). Whereas inequality and redistribution affect the crime rate by increasing the opportunity cost of crime relative to legitimate work, these security variables deter crime by lowering the expected payoff tied to criminal activity. Empirically, moreover, support for the theoretical prediction that more policing, harsher punishments, and increased investments in private security deter crime—if not always so at least conditionally—is also relatively abundant (Marvell and Moody, 1994, 1996; Levitt, 1996, 1997, 2002; Levitt and Miles, 2007; Shavell, 1991; Corman and Mocan, 2000; Ayres and Levitt, 1998). There is no reason, therefore, neither theoretically nor empirically, for supposing that crime will always and everywhere give rise to demand for redistribution, rather than support for increased law enforcement or investments in private security measures. This insight is important, not only because it is novel to the political economy literature on crime and punishment, but also because it implies that the relationship between crime and redistribution is contingent. As such, without a theory for when and why crime gives rise to demand for policies

aimed at lowering inequality rather than demand for either public or private security, or some given bundle of these policies, the argument about crime as a mechanism from inequality to redistribution remains indeterminate.

Fourth, and in the main part of the argument, I explore the determinants of this choice. That is, I ask: when does concern about crime give rise to increased demand for redistribution, and when does it instead lead to further support for public law enforcement or spending on private self-protection? The first part of my answer to this question focuses on perceptions regarding the efficiency and relative cost of either option at reducing crime. In the theoretical model developed over the previous pages, crime has the potential to form a causal mechanism from inequality to demand for redistribution only because inequality promotes crime, and because redistribution is perceived as a solution to crime through its effect on inequality. Hence, implicit in this line of reasoning is the claim that individual preferences for alternative crime prevention policies—whether that is redistribution, public law enforcement, or private security—are indirect, and dependent on individual beliefs about their respective ability to reduce crime given some cost. In order to understand precisely how crime matters for preferences, therefore, it is necessary to think about and explore how perceptions of this kind are shaped and formed.

In doing so, I emphasise the importance of one key variable: the level of population heterogeneity among the poor. Heterogeneity matters, I argue, because it affects beliefs about the causal origins of crime, and hence comes with implications for the formation of preferences over alternative solutions for it. In terms of causal attribution more generally, a basic (although by no means exhaustive) distinction can be made between dispositional attribution, which roots a particular behaviour in a given person's character or 'type', and situational attribution, which sees it as the product of an individual's environment and context (Heider, 1958). Criminologists have long asserted that this distinction comes with important consequences for law enforcement preferences—as George Vold said in the late 1950s, 'There is an obvious and logical interdependence between what is done about crime and what is assumed to be the reason for or explanation of criminality' (1958, pp. 258)—but less is known about where such attributions come from, or why they would vary in such a way as to explain the cross-national differences in criminal justice preferences noted earlier in this chapter. Building on previous work in social psychology (Tajfel et al., 1971; Tajfel and Turner, 1986; Chen and

Li, 2009), I suggest that an important key to this puzzle lies in the social identity of potential victims of crime relative to that of potential perpetrators: while the former are more likely to rely on situational attributions of crime when the latter are in-group members, they are more likely to attribute crime to dispositional factors when criminals belong to an out-group. The connection between these distinctions and the discussion in the previous paragraphs is not hard to see. Given that the argument from crime to redistribution preferences presupposes a causal inference from inequality to crime, I expect this association to weaken as those most likely to engage in crime—which in practice means the relatively poor—become more heterogenous relative to the affluent. In such settings, the affluent are more prone to view criminals as 'bad' people, leaving them with little reason to demand redistribution as a solution to crime, but all the more reason to prefer punitive alternatives. Hence, support for punitive forms of criminal justice policy will increase. In homogenous contexts, by contrast, I expect the relationship between fear of crime and support for redistribution to remain important.

## 1.4 Empirical strategy

An empirical test of the argument presented above comes with several challenges. On the aggregate level, the main task is to delineate the causal effect of fear of crime on preferences for redistribution, on the one hand, and support for punitive criminal justice policy, on the other, and to show that these effects depend on the level of population heterogeneity. Crucially, an appropriate methodological setup also requires that the explanation for these outcomes offered in this dissertation is distinguishable from those suggested by alternative theoretical approaches, some of which have reasonably similar empirical implications (for example, Rueda, 2018; Dimick, Rueda, and Stegmueller, 2016). On the individual-level, furthermore, empirically testing the argument involves an assessment of the claim that identity and group-status—that is, shared or divergent group membership between the rich (as potential victims) and the poor (as potential criminals)—is an important determinant of preferences over alternative means for reducing crime. As always with individual-level causal mechanisms, this is an all but trivial objective. Moreover, given that the argument implies that, net of externality concerns, redistribution preferences are driven by a tax-and-transfer maximising logic, it also requires a test of the effect of relative income on redistribution preferences.

To meet these demands, the overall empirical strategy of this dissertation is divided into two main parts: one observational, the other experimental. The former examines the macro-comparative implications of the argument, as the expectation that the redistribution and law enforcement preferences of the rich are affected by fear of crime, yet contingent on the level of heterogeneity among the poor. As such, it aims to throw a wide net: the overarching objective of this part of the empirical analysis is to test whether the implications of the argument are borne out in an as large and diverse range of settings as possible, both temporally and geographically. To that end, the observational side of the investigation relies primarily on individual-level data from two main sources: the European Social Survey (ESS) and the General Social Survey (GSS). As the name suggests, the former covers a large sample of respondents from the West European countries of interest (from 2002 to 2014), while the latter centres on public opinion in the US (from 1990 to 2010). This diversity of data sources is useful, for several reasons. For one, while the European data allows me to first and foremost explore cross-national differences, the American case facilitates a more thorough analysis of sub-national—or, more specifically, state and city-level—variation in the main explanatory variables. Aside from the statistical advantage of increasing the number of observations under study, this width also helps to build confidence that the findings of the investigation are generalisable to a wider set of cases and settings.

To complement this observational and statistical analysis, the second part of the empirical strategy is experimental. It has a simple objective: to test the validity of the central, individual-level causal mechanism linking the identity of potential criminals to the preferences possible victims of crime have over alternative solutions to crime. The analysis of European and American survey data is useful for ascertaining whether the implications of the theoretical model are borne-out in practice—that is, whether the relationship between inequality, crime and preferences match the account summarised in the previous section—but it remains relatively silent about the causal mechanisms that account for these aggregate-level outcomes. An experimental setup helps to overcome this deficiency: in facilitating randomisation and control, experiments allow me to more directly trace (and test) the chain of causality that links the left-hand side of the model to the outcomes it seeks to explain. As such, while the observational side is crucial for examining the external and real-world implications of my theoretical claims, the experimental part facilitates a more robust and direct test of my causal explana-

tion for them. Combining experimental and observational methods thus allows me to maintain a high degree of internal as well as external validity, thereby avoiding the common criticisms raised against either approach when used in isolation. The experiment itself involves participants interacting in a two-person sequential stealing game, in which one player can attempt to steal from the other, who in turn makes strategic choices over alternative measures aimed at deterring crime.

A few words, finally, should be said about the scope conditions. Although the empirical analysis developed below is limited to what is generally referred to as the advanced industrial democracies of Western Europe and North America, there is no necessary reason for supposing that my argument—or extensions of it—should not apply elsewhere. Parts of Latin and South America, for example, combine high inequality and high levels of crime with heterogenous crime policy responses, and as such are likely to prove fruitful ground for further work (for a move in this direction, see Riviera and Zarate-Tenorio 2016).

## 1.5 Overview of the dissertation

The remainder of this dissertation is structured as follows. Chapter 2 develops the theoretical argument introduced above, and draws out the connection between the theoretical model proposed in this dissertation and the theory and evidence supplied by the previous literature. It thus replaces the more traditional literature review by intertwining an account of the argument proposed here with references to and descriptions of previous contributions where necessary.

The second part of the dissertation is empirical. Chapter 3 begins by testing the comparative implications of the argument using cross-sectional survey data from the European Social Survey (ESS), covering more than 140,000 observations from a total of 16 countries. Chapter 4 extends this analysis to the US using similar individual-level survey data from the General Social Survey (GSS), emphasising variation between metropolitan regions. The results of the analysis in both chapters lend strong support for my theoretical expectations: using a wide range of statistical models and estimation techniques, data from either side of the Atlantic, and while controlling for other possible explanations, the level of population heterogeneity among the poor has a statistically significant and substantively large mediating effect on the relationship between fear of crime and preferences. In Europe, natives crime preferences—over redistribution and

punishment—become increasingly punitive as the size of the immigrant population increases, and in the US, the same outcome follows for the preferences of whites as the population of their metropolitan area becomes more racially diverse.

Chapter 5, next, presents an experimental test of the argument that social identity matters for the formation of preferences over alternative means of dealing with crime. The bulk of the chapter focuses on the results of a laboratory experiment in which subjects can both steal from each other and make strategic investments in redistribution or punishment, both which which serve to make crime less profitable. To test the effect of heterogeneity and social identity on how this choice is made, subjects are divided into groups based in their art preferences—that is, using the minimal group paradigm (Tajfel and Turner, 1979; Chen and Li, 2009). Paralleling the findings from the observational analysis, the experimental results are in line with the implications of the theoretical model developed in Chapter 2: when faced with an out-group opponent, subjects in the stealing game substituted spending on punishment for spending on redistribution. When both the potential 'thief' and the potential victim of theft came from the same group, by contrast, the latter spent proportionally more on redistribution. Given the experimental setup, this effect of identity can confidently be interpreted as causal. These findings, therefore, add important flesh to the bones of the results derived throughout the preceding empirical chapters.

Chapter 6, finally, concludes. It relates the results derived throughout the empirical part of the dissertation to the empirical puzzles discusses earlier in this introduction, and outlined the implications of this for the study of redistribution preferences, on the one hand, and the study of punitive attitudes, on the other. More substantively, it also considers what these findings imply—both for understanding punitiveness in the US, and for thinking about where criminal justice policy and redistributive politics in Western Europe might be headed.

# Chapter 2

# The Argument

Using the previous literature as a stepping-stone, this chapter develops the theoretical argument outlined above in greater detail. To borrow a simile from Bartels (2008, pp. 148), the study of popular attitudes towards inequality and redistribution can be viewed either as a glass that is half full (and filling up) or one that is half empty. On the one hand, political economists have derived rigorous theoretical accounts of how and why inequality and redistribution are related, and produced an impressive body of empirical evidence in favour of their models. Across Western Europe and the US, individuals with lower incomes, less economic security, and fewer or less transferable skills are more likely to support redistribution than their richer, more secure, and more skilled counterparts. Since the study of redistribution preferences first emerged as a major research topic in comparative political economy a little more than a decade ago, we have greatly improved our understanding of both the individual-level motivations that underpin social policy preferences, and the macro-level conditions and institutions through which such demands are shaped and channeled.

Nonetheless, there is still arguably merit to the claim that the glass remains half empty. We know that material self-interest matters for redistribution preferences, but we also know that simple income maximisation is not all that matters. The match between relative income and demand for redistribution is far from perfect, and cross-nationally countries that are more unequal do not necessarily re-

distribute more. We know less, however, about what it is that explains these facts. The literature has produced a host of suggestions, emphasising factors ranging from insurance and the inter-temporal nature of preferences (Iversen and Soskice, 2001; Moene and Wallerstein, 2001, 2003), to altruism (Shayo, 2009; Rueda, 2018), religion (Stegmueller, 2013; Scheve and Stasavage, 2006), immigration and identity. These contributions have added valuable nuance and colour to how we think about the demand for redistribution. Yet, it is notable that little consensus exists as to which of these theories that hold the most merit, or alternatively, how they can be integrated into a unified and coherent theoretical framework.

The argument developed below does not resolve this issue, but seeks to highlight a causal mechanism from inequality to redistribution preferences that has received surprisingly little attention: crime, and in particular, fear of crime. As discussed briefly above and in some detail over the following pages, I argue that fear of crime has the potential to increase support for redistribution among high income groups, yet that this effect is heavily dependent on the identity of the poor. Where a sufficient share of the poor belong to a different identity group than the rich, in contrast, I expect fear of crime—and by extension, inequality—to produce increased demand for punitive criminal justice policy, but have little effect on preferences for redistribution. The reason for this contingent effect of fear of crime on preferences has to do with the relationship between identity and attribution: as I show in later chapters, shared group membership facilitates situational explanations of crime, including inequality, while divergent group identities promote dispositional explanations—the view that individuals engage in crime not because of the situation they are in, but because of the type of person they are.

The motivation behind this argument is part empirical, part theoretical. Empirically—as I have said above—the previous literature has done comparatively little to explain the observation that demand for redistribution is significantly more variable among high-income groups than it is among the poor, albeit with some notable exceptions (Dimick, Rueda, and Stegmueller, 2016; Rueda, 2018, see also Beramendi and Rehm, 2015). As I show below, support for redistribution from those on the very left of the income distribution is both widespread and relatively constant. The same cannot be said about opposition to redistribution from the rich. In some parts of Europe and the US, the affluent are indeed as hostile to redistribution as standard political economy models would predict, yet elsewhere they display an impressive level of support for such policies.

There are good reasons, therefore, for positing an interactive relationship between relative income and preferences, and for expecting the importance of material self-interest to demand for redistribution to decline with income. Despite this, much of the previous literature has either ignored the apparent variation in support for redistribution from the affluent, or focused on factors that may lead the poor to demand less redistribution than their self-interest dictates. The argument developed here suggests that the former is an important omission, and the analysis presented in later chapters show that the latter receives scant empirical support.

At the same time, political economists have largely ignored the topic of crime and punishment. This omission is surprising, given the centrality of politics in explanations both of criminal behaviour and the determinants of policies implemented to address crime. On the supply side, politics matters for crime because several important public policy variables—ranging from the level of redistribution and the shape of social insurance programmes to more traditional 'law and order' policies regarding policing and criminal justice—have a direct effect on the incentives for a given individual to engage in criminal activities. On the demand side, moreover, it is arguably reasonable to expect public concern about crime and voters' associated policy preferences to matter for the types of policies that are adopted with the intent of curbing crime—in a similar way to how redistribution preferences are assumed to matter, in some way, for redistributive outcomes. Yet, we know little about either of these processes. While much work in criminology and sociology claims to investigate the 'politics of crime', it does so in terms that are largely foreign to political scientists, often privileging contingent single-country explanations over generalisable theories. As a result, the question of how to explain cross-national differences, whether in the incidence of crime or in criminal justice policy, has been left largely unanswered. In presenting a comparative theory of how preferences over criminal justice policy are formed, the argument developed below represents a first attempt at correcting this notable shortfall.

The remainder of this chapter presents my theoretical argument in greater detail, and draws out its relationship to the previous literature. As I have said above, it thus also replaces the more traditional literature review. Given its role as the starting point for this dissertation's theoretical contribution, the first sections below largely follow the structure of Rueda and Stegmueller (2016). To that end, Section 2.1 first deals with short-term tax-and-transfer motivations and relative income, whereas Section 2.2 distinguishes such effects from those to do

with the externalities of macro-inequality, including fear of crime. Section 2.3 considers the empirical and theoretical relationship between inequality, crime and fear of crime, while Section 2.4 looks at the effects of public and private security on crime. In Section 2.5, I argue that the most important determinant of whether the affluent demand redistribution or security in response to fear of crime is the level of population heterogeneity amongst the poor. Section 2.6 in turn deals, first, with the relationship between support for public law enforcement and demand for private self-protection, and second, with concerns relating to residential sorting and the possibility that redistribution and security should be seen as club goods. Section 2.7, finally, summarises the argument and formulates the set of hypotheses that form the basis for the subsequent empirical analysis.

## 2.1 Taxes and transfers

As a starting point, I join an influential tradition in political economy by acknowledging that an important motive for demanding redistribution has to do with post-tax post-transfer income maximisation—or material self-interest, for short. The core individual-level assumption inherent to arguments in this vein is simple: individuals support redistribution when it increases their economic well-being, and oppose redistribution when it makes them worse-off economically. The previous chapter has already noted that this idea is far from novel—for centuries, conservatives, liberals and socialists all agreed that democracy would allow the poor to soak the rich, sharing the basic premise that demand for redistribution is first and foremost an expression of self-interest. More recently, this basic intuition has informed into two broad sets of models: the first emphasising current income and redistribution as transfers from the rich to the poor, and the second focusing on labour market risks and redistribution as social insurance.

The most well-known and perhaps simplest statement of the former approach is the theoretical model proposed by Romer (1975), and later developed and popularised by Meltzer and Richard (1981). On the demand side, these arguments model individual labour-leisure choices in a one-period economy, where the government redistributes a lump-sum transfer to all citizens financed through a linear income-tax scheme. Voters are assumed to derive utility from consumption only, and have different productivities which determine their potential market income. Given this setup, if the government faces a hard budget constraint and

taxation comes with no deadweight costs, it follows that the size of the lump-sum transfer necessarily equals the tax contribution of the mean income voter. Hence, everyone earning less than mean income always benefit from more redistribution, implying that their optimal choice is a 100% tax-rate. Conversely, redistribution never benefit those earning more than mean income, suggesting that they will prefer a 0% tax-rate with no redistribution. Allowing for tax disincentive effects moves the model away from corner solutions, such that voters demand redistribution only up to the point where its marginal benefit to them is outweighed by the deadweight cost of taxation. This means that all or a subset of individuals—depending on the shape of the deadweight cost function—with an income below the mean will demand some intermediate level of redistribution and taxation, and that this level will increase with pre-tax income up to the mean.

In aggregating preferences and linking them to political or policy outcomes, the Romer-Meltzer-Richard (RMR) model follows a standard median-voter logic (Downs, 1957). Given that the income distribution in all advanced industrial democracies is right-skewed, the income of the median voter is empirically always smaller than mean income. It follows, therefore, that the supply of redistribution will be positive, and that the level of redistribution will be determined by the distance between the income of the median voter and mean income. Thus, the model leads to the well-known prediction that redistribution and inequality are positively related: any change in inequality that increases the income distance between the median voter and the mean should be followed by an increase in the supply of redistribution. Cross-nationally, this also implies that we should find the highest levels of redistribution in the countries with the most unequal—or more accurately, most right-skewed—income distributions, and the lowest levels of redistribution in the countries that are the most equal.

The general and familiar consensus in the macro-comparative literature, however, is that this proposition is only weakly (if at all) supported by the empirical evidence. As mentioned in the previous chapter, recent empirical scholarship is largely characterised by a disconnect between the results of studies using cross-sectional data and levels of inequality, and those looking at changes in inequality over time. On the one hand, the cross-sectional literature has consistently derived very little in the way of empirical support for the core prediction of the RMR-model about a positive relationship between market inequality and the supply of redistribution (Iversen and Soskice, 2001, 2006; Moene and Wallerstein, 2001,

2003; Lindert, 2004; Alesina and Glaeser, 2004; Shelton, 2007, and many others). Departing further from the implications of the model, some authors even find a negative relationship between these two variables, paralleling the more casual observation that a generous welfare state appears to go together with a relatively equal market income distribution (as in Scandinavia, for example), whereas the opposite seems to hold true in the predominantly Anglo-Saxon, liberal market economies. The upshot of this literature, therefore, is that more unequal countries do not necessarily redistribute more. On the other hand, studies emphasising time-series variation in inequality and redistribution have more consistently found support for the argument increasing inequality tends to result in more redistribution (Milanovic, 2000; Kenworthy and Pontusson, 2005), yet even in this line of research the results are mixed (Kenworthy and McCall, 2008; Cavaille, 2014).

Perhaps because of the intuitive appeal of arguments giving primacy to material self-interest, this finding—variously referred to as the 'paradox of redistribution' (Dimick, 2015) or, more commonly, the 'Robin Hood' puzzle (Lindert, 2004; Iversen and Soskice, 2009)—has generated a large body of scholarship seeking to establish either where the RMR-model fails to capture the empirical reality, or what alternative reasons there are for expecting a positive rather than a negative relationship between the level of inequality and the supply of redistribution. Several strands of this literature can be distinguished. Some arguments, first, focus on how preferences for redistribution are aggregated and translated into political outputs. Iversen and Soskice (2006), for example, emphasise the role of proportional electoral institutions and commitment problems in determining the likelihood of centre-left government coalitions, arguing that in a two-party majoritarian system, members of the middle-class are more likely to vote for the centre-right given of their fear of excessive taxation in case a centre-left party decides to deviate to the left once in government. In this model, the problem for left parties in majoritarian systems is that they cannot credibly commit not to soak both the affluent and the middle-class once elected. Other arguments take the median-voter logic of the RMR-model as given, but emphasise the relationship between income, pre-tax inequality, and electoral turnout. The main thrust of this literature is that if inequality makes the poor less likely to participate in the political process, it does not necessarily follow that the difference between the income of the median voter and the mean increases with inequality. Hence, so the argument goes, the 'paradox of redistribution' immediately becomes less puzzling.

Other authors focus more directly on the demand side logic of the RMR-model, in different ways challenging the argument that tax-and-transfer motivations fundamentally underpin redistribution preferences, or its implication that support for redistribution declines with relative income. As I have said above, an influential set of models in this tradition highlight the fact that many redistributive policies not only facilitate transfers between income groups, but also functions as insurance against income shocks. On this logic, individuals may demand social insurance because they face a certain risk of job or income loss in the future, even if they are net contributors to such schemes in the present. This analytical move, in turn, opens the door for arguments that question the central assumption about a negative relationship between relative income and support for redistribution. In an influential contribution, for example, Moene and Wallerstein (2001) argue that social insurance (like most other forms of insurance) is a normal good, implying that individuals will consume more of it as their income increases. As such, the insurance and redistribution motives go in different directions with respect to income, and with a sufficiently high degree of risk-aversion, the former may outweigh the latter. The implication of their argument, therefore, is that demand for redistribution should increase with income but decline with a mean-preserving increase in inequality, holding the level and distribution of risk constant. Other arguments do not share this highly counter-intuitive result, but still emphasise the role of the insurance motive to redistribution preferences: Iversen and Soskice (2001), for instance, argue that workers with more specific skills will demand more redistribution to protect their human capital investments, while Rehm (2009) stresses the role of occupational unemployment.

Without denying the merit of these arguments, their importance for how we think about the relationship between relative income, inequality and demand for redistribution should not be overstated, for several reasons. First, as Dimick (2018; see also Persson and Tabellini, 2000; Saint-Paul, 2001; Saint-Paul and Verdier, 1996; Alesina and Giuliano, 2011; Dimick, Rueda, and Stegmueller, 2018) points out, much of the macro-level literature on the relationship between inequality and redistribution has failed to fully appreciate the contingent link between the median-mean income distance—in essence, the skew of the income distribution among those who vote—and alternative measures of inequality. The RMR-model only implies that the supply of redistribution increases as the income of the median voter and mean income grow further apart. In nearly all of

the previous literature, this has been interpreted as suggesting that 'inequality', without qualification, and redistribution will be positively related. Yet, once it is acknowledged that the income of the median voter and median income are not necessarily the same, there is no deductive reason for assuming an identity between skew and inequality, or, in turn, for supposing that a more unequal distribution is necessarily also more skewed. Given the literatures on income effects on both turnout and political influence, moreover, there are good reasons for expecting inequality—as measured by the Gini index, for example—and voting-population skew to be quite distinct in practice. Indeed, and in agreement with this line of reasoning, the few studies that explicitly look at the relationship between skewness and redistribution have more consistently found evidence for a possible relationship between these two variables (Karabarbounis, 2011; and inadvertently, Lupu and Pontusson, 2011), implying that the RMR-model may be more consistent with the empirical evidence than is often recognised.

More closely related to the focus of this dissertation, however, even if the RMR-model is rejected on the aggregate level, it does not necessarily follow that its central argument about preferences—emphasising the tax-and-transfer motive and the causal effect of relative income—should be dismissed with it. The model involves two distinct propositions: one about relative income and preferences, the other about how such preferences are aggregated into outcomes through the the median voter. The absence of a straightforward relationship between inequality and the supply of redistribution can stem from problems on either side of the theory. It does not necessarily mean that its individual-level logic regarding transfers, relative income and preferences is incorrect. Indeed, as the preceding discussion has suggested, many alternative explanations are possible. The median voter theorem is deceptively simplistic, if not in two-party systems so certainly in countries with more complex patterns of party competition. Additionally, the aforementioned factors to do with the electoral system (Iversen and Soskice, 2006), electoral turnout (Beramendi and Anderson, 2008; Boix, 2003; Mahler, 2008; Barnes, 2013; Bartels, 2009), partisanship (Rueda, 2005, 2007; Evans and Tilley, 2012), multi-dimensionality of preferences (Scheve and Stasavage, 2006; Stegmueller, 2013; Alesina and Glaeser, 2004), or the disproportionate influence of the rich on policy (Rodriguez, 1999; Karabarbounis, 2011), are all potential mechanisms able to explain how the demand side of the theory can hold true, even if its supply-side argument does not. Thus, even taking the

lack of macro-level evidence for the RMR-model as given, it is important not to dismiss its demand-side theory about preferences without further argument.

In fact, there are good reasons to expect the problem with the RMR-model to lie elsewhere than in its argument about preferences. Empirically, for one, individual-level evidence in support of the argument that demand for redistribution can be modelled as a negative function of relative income is relatively abundant, unlike what is the case in the macro-comparative literature on the relationship between inequality and the supply of redistribution. Using different data sources and diverse methodologies, Finseraas (2009), Iversen and Soskice (2001), Dimick, Rueda, and Stegmueller (2016), Alesina and Giuliano (2011), Corneo and Grüner (2002), Cusack, Iversen, and Rehm (2006) and McCarty, Poole, and Rosenthal (2006), to name but some, all report significant negative effects of relative income on demand for redistribution (however, for opposing results, see especially Kenworthy and McCall, 2008; Huber and Stanig, 2009). In the empirical analysis developed in later chapters, I arrive at the same conclusion: although the magnitude of this effect varies significantly between contexts, relative income has a highly robust and systematic negative effect on support for redistribution. Prima facie, these results lend support to the argument that the absence of macro-level support for the RMR-model has more to do with its political mechanism than it does with its argument about relative income and preferences.

As a baseline expectation, therefore, I anticipate support for redistribution to decline with relative income. As an individual's income increases relative to the mean, he or she has less to gain (or conversely, more to lose) from redistribution in pure tax-and-transfer terms. Hence, ceteris paribus, I expect support for redistribution to decline as relative income increases. The opposite is also true: if the mean increases but a given voter's income does not change, support for redistribution will increase as a result. Moreover, and in line with most of the previous literature on this topic, I assume that relative income matters for redistribution preferences across the income distribution. The motivation for this small departure from the basic model is relatively simple: under a stylised RMR-redistributive scheme, redistribution will matter more in real terms to individuals on either extreme of the income distribution than it will do for those with an income closer to the mean. Given that it is reasonable to expect these differences to be reflected in attitudes towards redistribution, it follows that income should matter to support for redistribution across the range of market incomes.

## 2.2 Externalities of inequality

Notwithstanding the plausibility of argument, however, there are good grounds for thinking that redistribution preferences are not only about present income maximisation. As discussed in the previous chapter, one such reason has to do with the redistribution preferences of the relatively affluent: although the poor in nearly all advanced industrial democracies have higher redistribution preferences than the rich (as implied by the standard material self-interest argument), support for redistribution in most countries still extends deep into income groups that arguably do not have a reasonable tax-and-transfer motive for demanding redistributive policies. If redistribution preferences are all about present income and taxes-and-transfers, this observation is indeed difficult to account for. To do so, therefore, it is both necessary and useful to look beyond relative income.

Until quite recently (see Alesina and Giuliano, 2011; Rueda and Stegmueller, 2016), a largely overlooked avenue for doing so focuses on how changes in the level of macro-inequality (that is, changes in the level of income inequality holding relative income, and hence material self-interest concerns, constant) can affect redistribution preferences indirectly when they are associated with negative or positive externalities. Theoretically, this idea is relatively straightforward: if inequality produces externalities with distinct individual-level consequences, it is reasonable to expect redistribution preferences to be affected, or at least have the potential to change. The initial plausibility of this causal logic is not difficult to appreciate. If inequality is harmful, rational actors should increase their support of policies aimed at reducing inequality, all else equal. If inequality is beneficial, on the other hand, the opposite outcome should follow. For this mechanism to work, of course, it is necessary both that individuals understand and internalise the causal connection between inequality and its externalities, and in the second instance, that they view redistribution as a plausible means to affect them. In Section 2.4 below, I return to this point in some detail in the context of the causal relationship between fear of crime, social identity and demand for redistribution.

Existing research, moreover, has identified a wide range of variables that could potentially give rise to this type of causal mechanism, going from inequality through its externalities to preferences for redistribution. A large and well-known literature in economics, for example, has investigated the relationship between inequality and growth, and highlighted the conditions under which in-

equality can stifle economic development (Kuznets, 1955; Persson and Tabellini, 1994; Barro, 2000; for a useful review of this research, see Stiglitz, 2016). Others have emphasised the link between inequality and lower-level economic variables, including unemployment (Bertola and Ichino, 1995), inflation (Albanesi, 2007; Desai, Olofsgard, and Yousef, 2003), labour productivity (Durlauf, 1996), and educational attainment (Gregorio and Lee, 2002). In comparative politics, in turn, several influential arguments have modelled the connection between inequality and regime change, in various ways stressing the role of distributive conflicts in determining both democratic stability (Muller, 1988, 1995; Karl, 2000) and the likelihood of democratic transitions (Boix, 2003; Acemoglu and Robinson, 2005; see also Ansell and Samuels, 2010, 2014). As is well-known outside of academia, inequality has also been found to matter for public health, negatively affecting health outcomes ranging from life expectancy and infant mortality to mental health and the prevalence of obesity (Wilkinson and Pickett, 2006, 2010; Kawachi et al., 1997). Finally, and as discussed in some detail below, there is robust evidence that inequality causes both crime and fear of crime (Becker, 1968; Ehrlich, 1973; Vauclair and Bratanova, 2017). To the extent that either one of these effects matter on the individual-level, therefore, they are consequential for how we think about the relationship between inequality and demand for redistribution. Yet, this causal mechanism has received little attention from previous authors in comparative political economy, albeit with some notable exceptions (for example, Rueda and Stegmueller, 2016; Imrohoroglu, Merlo, and Rupert, 2000; for a discussion, see also Alesina and Giuliano, 2011).

Accepting that these externality concerns may be important for redistribution preferences, however, begs the question of how such considerations relate to the material self-interest logic discussed in the previous section. Do the externalities of inequality matter equally for the preferences of the rich and the poor, or do such concerns matter more for some income-groups but less for others? To integrate these mechanisms, three points need to be considered. First, the different externalities mentioned above do not necessarily affect individuals across the income distribution to the same extent. It might be reasonable, for instance, to expect unemployment to matter more for low-income individuals in precarious forms of employment, while inflation should be a more pertinent concern for members of high-income groups with more significant savings. To analyse these perspectives together, therefore, it is necessary to consider who is affected by macro-inequality.

Second, it is important to remember that the externalities of inequality can work either in the same or in the opposite direction to material self-interest. Negative externalities of inequality, for one, will reinforce the material self-interest motive for supporting redistribution among those with low incomes, but counter the same logic for individuals with no tax-and-transfer motive for demanding redistribution. The opposite holds true to the extent that inequality is beneficial.

These points aside—and more generally—there are persuasive theoretical reasons for expecting tax-and-transfer considerations to dominate the redistribution preferences of the poor, but for externality concerns to make more of a difference to the preferences of the more affluent. Several arguments can be made in support of this claim. Some have argued, first, that because the relative value of receiving transfers for the poor outweigh the relative cost of paying taxes to the rich, the importance of motivations not related to material self-interest increases in income (Rueda and Stegmueller, 2016). More formally, the same result follows from a simple theoretical setup in which individuals have concave utility functions over consumption and a given externality effect—crime, say—and the welfare implications of the latter are distributed uniformly across the income distribution. Consider, for instance, an increase in inequality that leads to a corresponding increase in crime, making the rich and the poor alike worse off. Holding tax-and-transfer motivations constant, such a change in inequality will make a greater difference to the redistribution preferences of the affluent then it will to the preferences of those with comparatively lower incomes. This result follows because, at the margin, a poor person values consumption more than a rich person does, and should hence be less willing to substitute redistribution aimed at deterring crime for personal consumption. For a rich person, by contrast, marginal consumption matters less relative to the increase in crime, implying a higher marginal rate of substitution between the two 'goods'. On this logic, therefore, whereas the poor will always support redistribution because of its immediate tax-and-transfer implications, the negative (or positive) externalities of inequality have more potential to matter for those with higher incomes.

A complementary logic is time-related. Large literatures in economics and sociology as well as psychology have shown that temporal discount rates—that is, the tendency to prefer a benefit received sooner to one received later—generally correlates negatively with income. On the aggregate level, this implies that individuals with low incomes tend to be more present-oriented (or impatient) in their

decision-making, while those with higher incomes are more sensitive to future benefits and costs. Especially in economics, this idea has a long pedigree. As early as 1930, Fisher commented in his *Theory of Interest* that 'a small income, other things being equal, tend to produce a high rate of impatience' (pp. 73). More recent and more empirical studies have tended to confirm this intuition. Harrison, Lau, and Williams (2002), for example, conduct a series of field-experiments designed to elicit temporal discount rates in the context of household investment decisions, and find that income is one of the most significant drivers of impatience. Also using a field-experimental design, (Tanaka, Camerer, and Nguyen, 2010) report similar findings from Vietnam. Using diverse methods, many others have arrived at similar conclusions (for example, Green et al., 1996; Reimers et al., 2009; de Wit et al., 2007; Lawrance, 1991; Dynan, Skinner, and Zeldes, 2004; Hausman, 1979; Carvalho, 2010). Elsewhere, this observation has been used as an explanation for a wide range of empirical phenomena: from class differences in education levels and investment choices (Lawrance, 1991; Dynan, Skinner, and Zeldes, 2004) to obesity (Borghans and Golsteyn, 2006) and substance abuse (Bickel, Odum, and Madden, 1999; Khwaja, Silverman, and Sloan, 2007).

As an aside, it should be noted that most of this literature has treated time preferences as causally prior to income and wealth. That is, the traditional logic is not so much that poorer people tend to be more impatient, but rather that impatience causes people to be poor. Deaton (1989), for example, shows how heterogenous time preferences over time 'divides the population into two groups, one of which lives a little better than hand to mouth but never has more than enough to meet emergencies, while the other, as a group, saves and steadily accumulates assents' (pp. 76). Discounting has also been indirectly linked to economic outcomes through its effect on other variables, such as investments in education and health. More recently, however, a number of arguments have been proposed that reverse the direction of causality. For example, building on work in cognitive psychology, Spears (2011) highlights a number of mechanisms linking poverty to diminished behavioural control, and finds significant support for this argument using both experimental and observational data. In psychology, others have proposed alternative mechanisms, ranging from the neurological effects of scarcity (Haushofer and Fehr, 2014) to the increased cognitive demands associated with being poor (Mani et al., 2013). As an empirical regularity, however, the relationship between income and impatience remains undisputed.

Going back to the main concern of this chapter, these findings matter since there are persuasive reasons for expecting individual-level welfare concerns relating to the externalities of inequality to be more long-term relative to the tax-and-transfer effects of redistribution. The latter are short term: by definition, the material self-interest motive should not be affected by temporal discounting since the transfer benefits of redistribution are immediate in time. For the poor, receiving transfers today provides a benefit today, and similarly for the rich, paying taxes in the present implies a cost in the present. The same does not necessarily hold true for concerns relating to the externalities of inequality, however. As an empirical matter, most of the externality effects highlighted in the previous paragraphs are of a relatively long term nature. The individual-level benefits of increased educational attainments among the poor, as one possible negative externality of inequality, for example, are likely to make a difference over a longer time-frame relative to the immediate tax-and-transfer effects of redistribution. The same observation also likely holds true for the relationship between inequality and economic growth: although less inequality might promote growth eventually, which in turn may have positive individual-level consequences, this is a process that arguably takes time. Hence, even assuming a positive welfare effect in the long-run, such considerations will only affect demand for redistribution in the present to the extent that individuals are sufficiently forward-looking. Conversely, the relevance of material self-interest to preferences increases with impatience.

Moreover, it is reasonable to expect this distinction to be particularly important when it comes to crime, since fear of crime itself is inherently forward-looking. In the simplest of models, the relevance of crime to present choices depends on the expected future cost (whether physical or psychological) associated with being the victim of crime, discounted by the probability of not being victimised. Consequently, given some objective level of risk, those with the most patient time preferences should also be those that are most concerned with this possibility. In the case of crime, therefore, discounting matters in two places: when evaluating the relationship between redistribution today and crime in the future, and when discounting the expected future cost of crime to its present value. Although I know of no previous work that tests this expectation directly, it is notable that the distinct literatures on fear of crime and time discounting have generated largely congruent conclusions: age, for example, has been shown to be positively correlated both with low levels of discounting (Green et al., 1996;

Reimers et al., 2009) and with high levels of fear of crime, controlling for objective risk (Franklin, Franklin, and Fearn, 2008). In contrast, for individuals with a sufficiently high discount rate, crime should have little relevance for present choices.

Taken together, these arguments provide strong grounds for expecting the relevance of the externalities of inequality to preferences for redistribution to increase in income. To the extent that the poor exhibit high discount rates for future welfare—and there are good reasons for thinking that this is indeed the case—current tax-and-transfer motivations will matter more for their redistribution preferences than longer-term concerns relating to inequality. Thus, the poor will always demand redistribution because of their current tax-and-transfer motive for doing so, but their preferences will not be affected by macro-inequality net of such concerns. The opposite, in turn, holds true for the rich: in as far as they engage in less temporal discounting, their preferences will be more sensitive to the long-term effects of inequality, controlling for material self-interest. If inequality is associated with negative externalities, therefore, their support for redistribution will increase with inequality. It is important to emphasise, finally, that the consistency of this argument does not depend on the direction of causality between income and time discounting—that is, on whether income determines time preferences, or whether time preferences determine income. All it requires is the empirical regularity that the two are systematically correlated. In the remaining sections of this chapter, I explore the implications of this insight in greater detail.

## 2.3 Inequality, crime and redistribution

Different authors have stressed alternative micro-mechanisms through which this type of externality effect on redistribution preferences can operate. In economics, for one, an important set of arguments emphasise how the affluent may have a self-interested motive for demanding redistribution when credit constraints on the poor prevent them from investing in higher education (Romer, 1990; Nelson and Phelps, 1996; Alesina and Giuliano, 2011; Perotti, 1996; Benabou, 1996; see also Ansell, 2010). To the extent that some stand to gain from an increase in the average level of education in society, it thus follows that support for redistribution can increase with inequality. Alternatively and in the opposite direction, others have suggested that inequality can be beneficial to the extent that it is linked to positive labour incentive effects, at least up to a certain threshold (see, for example,

Schumpeter, 1944; Siebert, 1998; Bell and Freeman, 2001). As Alesina and Giuliano (2011, pp. 7) summarise this line of reasoning, if 'there are [positive] externalities [of inequality] in effort and education acquisition, this may work in favour of society as a whole, since the aggregate level of effort/investment in education would then go up'. On this logic, inequality should therefore reduce support for redistribution, ceteris paribus. The empirical validity of this argument (and perhaps even more so, its applicability at current, real-world levels of inequality) is, of course, widely contested both within academia and in general political discourse.

More closely related to the argument of this dissertation, Rueda and Stegmueller (2016) focus on the relationship between inequality and fear of crime. They argue that because inequality causes crime and by extension fear of crime, the rich in high-inequality regions have an incentive to support policies aimed at lowering inequality in order to increase their sense of security. Yet, and although we share a common focus on fear of crime as a causal mechanism from macro-inequality to redistribution preferences, it is important to emphasise that the argument of this dissertation differs from and contradicts that of Rueda and Stegmueller (2016) on several essential points. Perhaps most importantly, my argument suggests and my empirical analysis show that the effect of fear of crime on support for redistribution is contingent rather than necessary, and dependent on the level of population heterogeneity amongst the poor. In contrast, Rueda and Stegmueller (2015) make no such claims. Thus, we arrive at very distinct conclusions about when and how fear of crime—and by extension, macro-inequality—exerts a causal influence on the redistribution preferences of the rich: for Rueda and Stegmueller (2016), crime always and everywhere generates increased demand for redistribution, whereas I both argue and show that this is far from the case. These differences will become more apparent below.

Theoretically, the first step in this causal chain is grounded in one of the central tenets of standard economic models of crime and deterrence: that the crime rate in a given society is positively linked to its level of income inequality. Whereas the idea that potential criminal offenders respond to economic incentives dates back at least to the eighteenth century (Beccaria, [1764] 1995; Bentham, [1789] 1996), the canonical modern version of this argument stems from Becker (1968), and its extension as a time-allocation problem provided by Ehrlich (1973; see also Block and Heineke, 1975). The basic assumption of this model is that potential criminals—just like any other economic agents—are

rational, and thus seek to maximise their expected utility. Hence, an individual commits a crime if and only if the expected benefit of doing so exceeds its opportunity cost, which is most immediately determined by the wage associated with legitimate employment in the labour market. The payoff from crime, on the other side of the equation, depends on the average wealth of society, discounted by the probability of conviction, the associated cost of punishment and its timing.

Two important implications follow from this simple framework. The first is that crime becomes increasingly attractive relative to legitimate work as the potential wage that an individual can earn in the labour market declines, or equally, that only the poor ever have a rational motive for engaging in crime. To quote Ehrlich (1973, pp. 68): those with 'legitimate returns' that are 'well below the median have greater differential returns from property crimes and hence, greater incentive to participate in such crimes, relative to those with income well above the median'. The aforementioned relationship between income and time discounting works in the same direction: to the extent that most criminal sanctions are delayed in time while the benefits of crime are immediate, the deterrence effect of a given punishment declines with an individual's discount rate for future welfare (see Åkerlund et al., 2016; Listokin, 2007). Second, the incentive for the poor to specialise in crime increases with the level of income inequality. High-inequality mean that for the worst-off, the expected gain from crime is large, but its opportunity cost low. Low inequality, on the other hand, implies that a smaller subset of the population can expect to earn more from crime than from legitimate work. On this reasoning, therefore, more inequality leads to more crime, and less inequality leads to less crime (Glaeser, 1999; Levitt and Miles, 2007; Chiu and Madden, 1998; Imrohoroglu, Merlo, and Rupert, 2000; Kelly, 2000; for non-economic theories of crime with similar implications, see Merton, 1938; Shaw and McKay, 1942).

It is important to emphasise, moreover, that there are theoretical reasons for expecting this logic to be pertinent not only for property crime but also for violent crime. First, property crime and many forms of violent crime are intimately linked in practice—consider, for instance, violent robberies and burglaries, kidnappings and other similar offences where violence or the threat of violence is (intentionally or not) the by-product of crimes fundamentally motivated by economic gain. To the extent that inequality causes property crime, therefore, it is reasonable to expect the same relationship to hold true for violent crime. More structurally, Bourguignon (2001, pp. 208) develops an extension of the standard

Becker model in which violent crime is 'related to the control of illicit activity like gambling, prostitution, or drug dealing and trafficking'. Since these activities (and organised crime in general) are themselves causally related to inequality, it follows that inequality should affect not only property crime but also violent crime (for theoretical alternatives with similar implications, see also Chiu and Madden, 1998; Kelly, 2000; Soares, 2004). Non-economic theories of crime, finally, generally also support these intuitions. Strain theory, for example, suggests that individuals at the bottom of the social ladder are frustrated by their inability to attain the attributes of material success, and that this 'sense of failure' is aggravated when they are confronted with the success of others in the community. The resulting alienation from society leads the affected individuals to engage in both violent crime as well as property-oriented crime (Merton, 1938; Agnew, 1992).

Empirically, support for the argument that inequality and crime—both violent crime and property crime—are positively related is generally persuasive, although not entirely unambiguous. In one of the earliest studies to investigate the inequality-crime hypothesis empirically, Ehrlich (1973) used US state-level data to conclude that 'crimes against property (robbery, burglary, larceny, and auto theft)… vary positively with the percentage of families below one-half of the median income' (pp. 545). The more recent literature has tended to confirm this conclusion: as McAdams (2010) notes, whereas some studies have failed establish a systematic relationship between inequality and crime, almost none have found evidence for a negative effect, but many have done so for a positive effect (see also Freeman, 1996; Ehrlich, 1996). Fajnzylber, Lederman, and Loayza (2002a), for example, exploit cross-national panel data—and hence, unlike most others, country-fixed effects—from 39 advanced industrial democracies to conclude that 'income inequality, as measured by the Gini index, has a significant and positive effect on the incidence of crime' (pp. 25). Similarly, but seeking to explain why crime rates have declined in the US since the 1980s, Imrohoroglu, Merlo, and Rupert (2004) find that rising income inequality has been one of the most important factors working against this trend, suggesting that the drop in crime would have been more than twice as large had the level of income inequality remained constant. Additionally, Carroll and Jackson (1983); Fowles and Merva (1996); Patterson (1991); Kelly (2000); Bourguignon (2001); Dahlberg and Gustavsson (2008), and Whitworth (2012) all report similar results, amongst many others.

Finally, it should be stressed that although the discussion above concerns

actual crime rates, it is fear of crime—as a subjective assessment of the probability of victimisation, which may be more or less accurate—that matters more for my theoretical argument. The reason for this is relatively straightforward: on the logic of my argument, crime matters because it is an economic 'bad', and hence something that individuals are willing to pay to avoid. This willingness ultimately stems from how concerned about or afraid of crime a given individual is, rather than from the objective probability that the same individual is victimised. With this point in mind, however, there are good reasons for expecting inequality affect both actual crime and fear of crime in similar ways (see Ferraro, 1995; Stafford and Galle, 1984). As Rueda and Stegmueller (2016) also emphasise, the theoretical rationale behind the claim that inequality matters for redistribution preferences through fear of crime as an externality requires only individuals concerned about crime have some understanding of objective crime rates, and perceive there to be a causal relationship between inequality and crime. As such, this argument holds true even if the association between fear of crime and actual crime is imperfect, as past research suggests is the case (Box, Hale, and Andrews, 1988; Ferraro and LaGrange, 1987; Ortega and Myles, 1987). Although less studied than the relationship between inequality and crime, moreover, several studies have found evidence for a direct positive relationship between inequality and fear of crime (Hummelsheim et al., 2010; Vieno, Roccato, and Russo, 2013; Vauclair and Bratanova, 2017). The empirical analysis developed in later chapters of this dissertation—using both European and American data—arrives at the same conclusion.

## 2.4 Law enforcement and private security

Taken together, the above argument forms a good starting point for the theory of fear of crime as a determinant of support for redistribution. It suggests that inequality causes both crime and fear of crime, and hence that individuals can demand redistribution as a solution to these perceived problems. Given high temporal discount rates among the poor, moreover, it follows that the magnitude of this effect increases with income. Hence, the argument leads to the prediction that the redistribution preferences of the affluent should be sensitive to the level of macro-inequality: holding relative income constant, the redistribution preferences of the rich should increase with macro-inequality. As such, it offers a

possible explanation for the varying strength of the relationship between income and support for redistribution, as discussed in the previous chapter.

Arguably, however, this is the beginning rather than the end of this theoretical story. Most importantly, it overlooks the fact that neither is increased redistribution the only means available to the rich for the purpose of reducing fear of crime, nor is there any inherent reason for expecting them to always prefer it over the available alternatives. Why should we not expect the rich to demand more public law enforcement in response to growing concerns about crime, rather than more redistribution? Or divert resources to different forms of private security? Since both these alternatives relate explicitly to crime and crime control, it seems reasonable to expect them to matter for a theory about the political implications of crime. From a political economy perspective, this insight is also consequential since it suggests that the direct effect of fear of crime—and hence, the indirect effect of inequality—on preferences for redistribution may be contingent rather than necessary. Thus, without a theoretical explanation for when and why the affluent are likely to support redistribution as a solution to fear of crime, rather than one of the alternatives, the theory of crime as a causal link from inequality to redistribution preferences remains incomplete. This is an important caveat to the argument about the relevance of crime for redistribution preferences, yet one that has hitherto been ignored—or assumed away—in the political economy literature.

Theoretically, the intuition that crime is partly a function of variables affecting the likelihood of conviction and the cost of punishment is a basic feature of all standard economic models of crime, as discussed above (Becker, 1968; Stigler, 1970; Ehrlich, 1973; Posner, 1985; Polinsky and Shavell, 1979, 2000; Freeman, 1999). In policy terms, the former is generally taken to refer to policing, while the latter concerns the relative cost of punishment, encompassing both its form—that is, whether administrated as a monetary fine, a suspended sentence, incarceration, and in some jurisdictions, corporal punishment—and its severity. In the simplest theoretical case, whereas inequality matters for crime because it affects the benefit side of the equation (that is, it determines the difference between the expected gain from crime and the expected reward from legitimate work), the likelihood of conviction and the cost of punishment serve to deter crime by increasing its potential costs, thus discounting the expected benefit tied to criminal activities. Given some level of inequality, therefore, these models predict that the crime rate will decline as the probability of being caught increases and the associated cost of

punishment grows more severe. Consequentially for the argument developed in this chapter, this also implies that a reduction in crime can be achieved either by decreasing the difference between the payoff from (successful) crime and the payoff from legitimate work—through redistribution, for example—or by increasing either the severity or the probability of punishment (see Benoit and Osborne, 1995). These comparative statics, which form the basis of the economics of crime and punishment, should be easy to follow. The same goes for the theoretical logic underlying these predictions: as the severity of punishment and the likelihood of incurring that punishment increases, it seems reasonable to expect the attractiveness of crime relative to other activities to decline accordingly.

Assuming risk-neutrality, an important implication of this basic model is that changes in the probability of arrest and changes to the cost of punishment are perfect substitutes: doubling the cost of punishment and doubling the probability of incurring that punishment have identical effects on the decision to engage in crime. Indeed, it is this feature of the model that leads Becker (1968) to argue that governments should reduce spending on policing (which tends to be costly) and instead increase the severity of punishment (which can be nearly costless, as in the case of monetary fines, for example). In practice, however, a regular finding in the empirical literature suggests that crime tends to be more sensitive to changes in the probability of conviction than to similar changes in the conditional cost of punishment, especially when the latter is operationalised as the length of prison sentences (see, for example, Grogger, 1991; Neilson and Winter, 1997; Davis, 1988). Theoretically, in turn, this result can be explained by supposing that criminals are risk-seeking, or more plausibly, by arguing that they exhibit high future discount rates. There is a notable parallel, therefore, between this finding and the argument developed above, linking high levels temporal discounting among the poor to less concern for the externalities of inequality.

This point aside, and similarly to what is the case in the literature on the relationship between inequality and crime, empirical support for the theoretical argument that public law enforcement policies, whether in terms of policing (affecting the probability of punishment) or the severity of criminal sanctions (as the cost of punishment), affect crime is relatively abundant albeit not entirely unambiguous. An important part of the reason for this is likely methodological (for a useful discussion, see Levitt and Miles, 2007). Given data limitations, first, nearly all of the empirical literature relies solely on macro-comparative data, despite

the fact that the basic economic analysis of crime is fundamentally an individual-level model. As I return to below, a burgeoning set of experimental studies have recently begun to address this shortfall. A related but perhaps more challenging issue concerns the simultaneity between crime and law enforcement: as Glaeser (1999, pp. 6) puts it, 'unfortunately, causal links between crime and deterrence are almost always two-sided'. Crime may respond to changes in policing and punishment, but at the same time, the supply of both policing and punishment tends to respond to changes in the crime rate. For all the well-known reasons, this poses a significant challenge to causal inference. As a result, most empirical studies focus on the US, where the combination of high-quality comparative crime data and sub-national heterogeneity in law enforcement policies make these issues somewhat easier to overcome (see Bennett and Lynch, 1990; Cook and Khmilevska, 2005; Tonry and Farrington, 2005; and especially, Levitt, 1996, 1997, 2002).

With these caveats in mind, however, most studies to investigate the relationship between public law enforcement and crime have found support for the empirical implications of the standard model, suggesting that policing and punishment have negative effects on the crime rate. In one of the first studies to fully recognise the methodological difficulties arising from the simultaneity of crime and law enforcement, Marvell and Moody (1996) find significant evidence for the expectation that increases in the size of the police force results in less crime. More recently, and using an innovative empirical strategy aimed at exploiting electoral cycle effects in levels of police hiring in 59 US cities between 1970 and 1992, Levitt (1997, 2002; see also Klick and Tabarrok, 2005; McCrary, 2002) derives robust evidence in support of the argument that more policing causes less crime. Evans and Owens (2007), Di Tella and Schargrodsky (2004) and Corman and Mocan (2000) also report similar results. Others have examined the relationship between crime and the severity of penal sanctions, typically incarceration rates, generally to the conclusion that increases in the size of the prison population results in decreases in the crime rate (Marvell and Moody, 1994; Levitt, 1996; Shepherd, 2002; for studies on capital punishment, see Ehrlich, 1975, 1977a,b; Dezhbakhsh, Rubin, and Shepherd, 2003; Donohue and Wolfers, 2006). Whether this effect is primarily due to deterrence (that crime becomes less attractive as the severity of punishment increases) or incapacitation (that incarcerating those individuals most prone to engage in crime prevents them from committing further crimes), however, remains a largely open question in the comparative literature.

These studies all concern what can be termed public security: policing and punishment administered through the criminal justice system. The effect of private security on crime, however, has received much less attention, both theoretically and empirically. Using a narrow definition, private security can be thought of as individual efforts aimed at self-protection—examples include home alarm systems, window bars, security guards, doormen, and perhaps most controversially, guns. Substantively, the comparatively scant interest in private security is arguably surprising, not least in light of several estimates suggesting that private expenditure on self-protection typically exceed public spending on law enforcement in many advanced industrial democracies (Ayres and Levitt, 1998). Philipson and Posner (1996), for example, report that aggregate private spending on crime prevention in the United States amounts to more than $300 billion annually, compared to public expenditures on the criminal justice system of only $90 billion over the same period (see also Mandel et al., 1993; Laband and Sophocleus, 1992; van Dijk, Mayhew, and Killias, 1990). Similarly, the number of private security employees in the United States is estimated to outnumber the number of public police officers by a factor of three (Joh, 2004; Blackstone and Hakim, 2010), up from one private security employee for every 1.4 police officers in the 1970s (Shearing, 1992; also Benson and Meehan, 2017). Additionally, the opportunity cost of various crime-related behavioural distortions aimed at self-protection—avoiding certain areas at night, for example, or moving to a safer neighbourhood—are also likely to be substantial, albeit difficult to quantify. As such, private security is all but a negligible aspect of the political economy of crime and redistribution.

The theoretical rationale behind the expectation that private protection deters crime is similar to the logic applying to government spending on law enforcement: in different ways, self-protection increases the cost of crime, and hence discounts the expected benefit from illegal activities (Becker, 1968). Private protection differs from public security, however, since its benefits are not uniformly distributed amongst all potential victims: whereas public law enforcement approximates a (local) public good, the same does not hold true for most kinds of private protection. Therefore, as both Shavell (1991) and Clotfelter (1977, 1978) observe, the effect of private security on crime depends on whether or not the protective measures taken by potential victims of crime are ex ante observable to potential criminals (see also Hui-Wen and Png, 1994; Helsley and Strange, 2005). Simplifying somewhat, if private protection is observable—a home alarm system visible from the

street, for example, or open carry firearms—a criminal can simply decide to direct his or her efforts at an alternative and unprotected victim. Hence, whereas such measures do produce a specific deterrence effect on crimes against those with protection (Hakim and Shachmurove, 1996; Di Tella, Schargrodsky, and Galiani, 2010; Zimmerman, 2014), their aggregate-level effect on the crime rate is more ambiguous. Unobservable forms of private security—such as silent alarms, hidden tracking systems for cars, or concealed carry weapons—on the other hand, do not produce similar displacement effects, since criminals cannot distinguish protected targets from those lacking protection. Yet, as long as prospective criminals know that at least some targets are protected, they can serve to raise the expected cost of crime uniformly for protected and unprotected victims alike, thus producing general deterrence (Ayres and Levitt, 1998; Gonzalez-Navarro, 2013; Lott and Mustard, 1997; Lott, 2010; however, for competing arguments about concealed carry firearms see Ayres and Donohue, 2003; Duggan, 2001).

Leaving this distinction aside for the time being, the previous paragraphs call into question the premise that individuals will always and necessarily demand redistribution in response to concern about crime. Rather, the main implication of the theory and evidence discussed above—that public spending on law enforcement reduces crime, and that private security comes with specific or general deterrence effects—is that there is more than one viable option. Moreover, and although demand for redistribution and demand for security are not necessarily incompatible, it is important to emphasise that there is no inherent reason for assuming this to be the case, or for supposing that the choice between these alternatives will be constant. As the literature stands, however, we simply do not know when, or for that matter why, concern about crime gives rise to demand for redistribution, rather than support for public law enforcement or efforts aimed at self-protection. Not only is this a relevant concern for the study of crime as a mechanism from macro-inequality to redistribution preferences, but it also comes with implications for the literature on crime and inequality more generally. In the sections below, therefore, I seek to explain how this choice is made.

## 2.5 Equality or punishment?

The point emphasised in the paragraphs above begs the question of how this choice is determined: when does concern about crime give rise to demand for

redistribution, rather than support for public law enforcement or investments in private self-protection? Again, without this piece of the puzzle, the theory linking crime to demand for redistribution remains incomplete.

As discussed in the previous chapter, the question of where punitive attitudes come from—and consequently, why citizens of some countries appear to be so much more punitive than those of others—has received significant attention in both criminology and sociology. To a lesser degree, it has also been addressed by psychologists. Although the term is not always well defined, 'punitiveness' in this context typically refers to individual support of negative sanctions for convicted criminals (or, in some cases, perceived rule breakers more generally), and thus encompasses preferences over policies to do with both policing and the relative cost of punishment. It is possible two distinguish two broad theoretical approaches in this literature: on the one hand, arguments that view punitiveness as something instrumental (that is, as a rational response to crime), and on the other, those that view punitiveness as something emotive, expressive or symbolic.

The latter literature is likely the least well known to political scientists and economists. Common to arguments in this vein is the suggestion that punitiveness has little to do with crime or the perceived risk of victimisation, but should rather be seen as a non-cognitive, fundamentally emotional response to a wider set of social transformations and processes. These, in turn, are often traced to the broad, structural socio-economic changes that have characterised most advanced industrial democracies over the last several decades. On this logic, as institutions and traditional social structures change, individuals experience a deep sense of insecurity, which in turn increases their support for punitive policies. Different authors stress different mechanisms behind the last step of this causal chain. For some, punitiveness is a coping mechanism: by projecting one's own diffuse fears and insecurities towards some external other—in this case, criminals—these feelings become more intelligible and hence more manageable. As Hirtenlehner (2011, pp. 33) writes, 'the call for tough punishment consequently represents a form of symbolic (vicarious) struggle against sentiments of insecurity, quasi an attempt at assurance that contemporary social problems can be dealt with' (see also Roberts et al., 2003; Holloway and Jefferson, 2000). For others, punitiveness arises because of a displacement effect. As one of the last social groups that can be openly dismissed and discredited, criminals become scapegoats against whom others can direct their discontent with society at large, even when these feelings

have little to do with crime (Gaubatz, 1995). In attempts to provide an individual-level account of these largely macro-social theories, finally, the psychoanalytic foundations of this perspective are often made more explicit. For example, building on Freud ([1915] 1990) but perhaps more directly on the work of Melanie Klein (1984), Maruna, Matravers, and King (2004; see also Garland, 1990, 2001) argue that punitiveness can be understood as an ego defence mechanism: by splitting off unacceptable parts of the self, projecting it onto others and then punishing them for it, we can rid ourselves of the sense of anxiety or guilt that would otherwise emerge. As such, while these arguments thus differ in their details, they share the premise that punitiveness is only indirectly related to crime *per se*.

Despite this rich theoretical literature, it is notable that empirical work on the central macro-level hypothesis of the expressive model—linking broad socio-economic transformations to punitiveness—remains relatively sparse. The few studies that do exist, moreover, have tended to produce mixed results. Some authors, first, have investigated the relationship between economic insecurity and punitiveness, generally to the conclusion that there is little if any causal relationship between these two variables (Costelloe, Chiricos, and Gertz, 2009; Johnson, 2001; Hogan, Chiricos, and Gertz, 2005). Others have looked more directly at the relationship between social or moral concerns and the desire to punish criminals. Tyler and Boeckmann (1997), for example, focus on the effect of normative judgements about the nature of families, the community and 'society', and find that concerns about the 'breakdown of traditional family structures' and the 'increasing diversification of society' correlate significantly with punitiveness. In a similar vein, King and Maruna (2009) argue that generational anxieties, operationalised as a concern for 'the youth of today', are an important positive predictor of punitive preferences. Neither of these studies pay much attention to causality, however. Such issues aside, a more fundamental challenge for theories emphasising 'modernity' and its anxieties is that they fail to explain why these forces appear to have taken such different expressions in different places. As Lacey (2010, pp. 102) correctly emphasises, 'not all "late modern" democracies have plumped for "neoliberal" politics' in the domain of crime and punishment, and—as discussed in the previous chapter—not all advanced industrial democracies have public opinion demanding harsher sentencing or more severe punishment. Moreover, short of arguing that Americans, say, have different psychological dispositions from Swedes or the Dutch, the same conclusion

also holds true for individual-level theories stressing the internal, psychological functions of punitiveness (for a discussion, see Brown, 2006).

To explain these patterns, therefore, it is arguably necessary to look elsewhere. One possibility lies in instrumental theories of punitiveness. Unlike the emotive model discussed above, theories in this literature start from the assumption that support for punishment is, fundamentally, a response to crime. Consequently, studies approaching punitive preferences from this perspective have emphasised the relationship between punitivity and perceptions of, or experiences with, crime itself—whether conceptualised as victimisation, fear of crime, or some more general 'concern about crime'. The logic behind this argument is admittedly intuitive: from a starting point of individual rationality, it seems reasonable to expect those who are more concerned, more afraid, or have a higher probability of criminal victimisation to also be more supportive of tougher measures aimed at curbing crime. Despite this appeal, however, empirical support for these instrumental expectations is, if not weak, so at least inconsistent. A regular finding in the literature is that victimisation, however operationalised, does not accurately predict punitive preferences (Blumstein and Cohen, 1980; Stinchcombe et al., 1980; Cullen et al., 1985; Taylor, Scheppele, and Stinchcombe, 1979; Applegate et al., 2000; Unnever, Cullen, and Fisher, 2007). The results of studies investigating the relationship between fear of crime and punitiveness, on the other hand, are more mixed. On the individual-level, while some studies find support for the expected positive relationship between fear of crime and support for harsher punishment (Langworthy and Whitehead, 1986; Costelloe et al., 2002; Costelloe, Chiricos, and Gertz, 2009; Dowler, 2003), others arrive at the opposite conclusion (Tyler and Boeckmann, 1997; King et al., 2004; Kleck and Jackson, 2016; Barkan and Cohn, 1994). To date, little consensus exists as to what explains these divergent results.

Building on these results, the argument of this dissertation suggests that in order to understand the association between fear of crime and preferences over alternative means of reducing crime—whether that is redistribution, on the one hand, or public and private security, on the other—it is necessary to consider the relationship between social identity and causal attribution. Identity matters, I argue, because it affects how crime is understood and interpreted, and hence comes with consequences for views about how crime as a social problem should best be solved. In terms of causal attribution more generally, a broad distinction can be made between dispositional (or 'internal') attribution, which roots a

particular act or behaviour in a given person's character or 'type', and situational ('external') attribution, which sees it as the product of an individual's environment and context (Heider, 1958; Ross, 1977). Criminologists have long understood that this distinction comes with important consequences for preferences over alternative means of reducing crime—as George Vold wrote in the late 1950s, 'there is an obvious and logical interdependence between what is done about crime and what is assumes to be the reason for or explanation of criminality' (1958, pp. 258)—but less is known about where such attributions come from, or why they would vary in such a way as to explain cross-sectional differences in support for redistribution and alternative law enforcement policies.

Borrowing insights from social psychology (Pettigrew, 1979; Tajfel et al., 1971; Tajfel and Turner, 1986; Chen and Li, 2009; see also Hewstone, 1990), I suggest that an important piece of this puzzle lies in the social identity of those concerned about crime relative to that of potential perpetrators: while the former are more likely to rely on situational attributions of crime, including inequality, when the latter are in-group members, they are more likely to attribute crime to dispositional factors when criminals belong to an out-group. The connection between these distinctions and the discussion developed earlier in this chapter is not hard to see. Given that the argument from crime to redistribution preferences presupposes a causal inference from inequality to crime, I expect this association to weaken as those most likely to engage in crime—which in practice means the relatively poor—become more heterogenous relative to the affluent. In such settings, the affluent are more prone to view criminals as 'bad' people, leaving them with little reason to demand redistribution as a solution to crime, but all the more reason to prefer punitive alternatives. Hence, support for punitive forms of criminal justice policy will increase. In homogenous contexts, by contrast, I expect the relationship between fear of crime and support for redistribution to remain significant. The paragraphs below develop this argument in greater detail.

There are three main steps to this causal logic: first, that the preferences of the rich over alternative solutions to crime depend on their perceived efficiency; second, that for redistribution, this is conditional on whether the affluent view crime as causally related to inequality; and third, that the extent to which they do so is affected by the level of population heterogeneity among those with low incomes. The first and second points are reasonably straightforward. On the logic of the argument developed over the previous pages, crime has the

potential to matter for redistribution preferences only because (i) crime and fear of crime are economic 'bads', and (ii) redistribution is seen as an appropriate means through which crime and its effects can be limited. Through its effect on crime, redistribution thus allows the affluent to pay in order to avoid crime from affecting them. Implicit in this argument, therefore, is the assumption that preferences over alternative means of reducing crime are indirect, and conditional on their expected effects of crime. If the affluent do not expect redistribution to affect crime, there is little reason for thinking that they will demand redistribution as a solution to it. The same holds true for the argument linking crime to demand for public and private security: the affluent do not have preferences over either of these policy alternatives directly, but only demand them because of their expected impact on crime. In order to understand the relationship between crime and preferences over alternative policies aimed at reducing crime, therefore, it is necessary to consider how such assessments are shaped and formed.

Of course, in a perfectly rational setting with complete and perfect information, 'beliefs' and 'perceptions' of this kind would have no place in my argument. Under such circumstances, the affluent would simply be aware of the relative productivities and costs of redistribution and security with respect to crime, and subsequently maximise their welfare by allocating resources accordingly between the different alternatives (for theoretical models of this scenario, see Imrohoroglu, Merlo, and Rupert, 2000; Benoit and Osborne, 1995). In the real world, however, matters are arguably less certain. As Rueda and Stegmueller (2016) also emphasise, a number of concern make the relative costs and benefits of these alternatives hard to quantify, both from a social welfare perspective and from the point-of-view of individual voters and agents. As the previous discussion has shown, estimating the marginal products of redistribution and security on the crime rate is a far from straightforward endeavour: even in the academic literature, there is little consensus about which policies work, and even less agreement about the relative magnitude of their effects on crime. The same holds true on the cost side. From a social welfare perspective, moreover, evaluating the more long-term, indirect costs and benefits of these alternatives—the cost of the criminal justice system, accounting for future re-entry and employment prospects of convicted criminals, the direct and indirect costs of incarceration, labour disincentive effects associated with redistribution, or the relationship between public and private security investments, for example—is if anything even more challenging,

both within and outside of academia (see Vining and Weimer, 2010; Roman and Visher, 2009; Western, 2002; Aos, Miller, and Drake, 2006; Cohen, 2005). Given this complexity, uncertainly is likely to influence how the rich decide between redistribution and different forms of security as alternative solutions to crime.

A more reasonable argument, therefore, is to suggest that what matters for this choice are subjective beliefs and expectations about the ability of these options—redistribution, on the one hand, and public or private security, on the other—to reduce crime, which may or may not be accurate *ex post facto*. For redistribution, again, a key question in this context concerns the extent to which the affluent internalise the empirical, causal relationship between inequality and crime. If they do, redistribution should be a plausible means for solving their concern about crime. If they do not, however, the same outcome is unlikely to follow.

There are several reasons for expecting social identity—and by extension, the level of population heterogeneity among the poor—to matter for this distinction. Empirically, for one, an extensive but highly US-centric literature has established that racial attitudes, most often conceptualised as racial stereotypes or prejudice, form a highly salient determinant not only of law enforcement preferences but also for beliefs about the causes of crime. Gilliam and Iyengar (2000), for example, manipulate the racial identity of an alleged perpetrator of murder in a fictional local news report. In line with this reasoning, they find then when the suspect shown is black, white respondents were significantly more likely to attribute crime to dispositional (that is, internal) traits—as the view that crime is the product of 'the failure of some groups in society to instil proper morals and values in their children' and the 'breakdown of the family structure', or accept that 'some people are just born criminals'—rather than to inequality and economic disadvantage. Similarly, Hurwitz and Peffley (2005) find that white respondents primed on the identity of criminals as black were significantly more likely to support punitive means of crime control—in this case, 'building more prisons'—over broadening anti-poverty programmes as a means to reduce crime, compared to the unprimed control group. Observational studies have largely arrived at the same conclusion: using data from the 2000 American National Election Survey (ANES), for instance, Unnever, Cullen, and Jones (2008) corroborate the finding that racial attitudes significantly condition beliefs about the causes of crime as well as preferences over alternative crime prevention policies. These are but some examples: Peffley, Hurwitz, and Sniderman (1997), Hurwitz and Peffley (1997),

Unnever and Cullen (2010), and Peffley and Hurwitz (2002) also report similar results, amongst many others (for a review, see Huddy and Feldman, 2009).

These findings are typically interpreted with specific reference to the history of racism and race-relations in the United States. Echoing the logic of Gilens' (1996) argument about race and redistribution, a common contention in this literature is that crime—much like 'welfare'—has become 'racialised'. Despite being an ostensibly 'race-neutral' policy domain (unlike affirmative action, for example), preferences regarding crime thus are strongly influenced by racial attitudes and specific forms of racial prejudice. Other authors place greater explanatory emphasis on the supply-side, arguing that crime has become a 'dog-whistle' issue: in a time where explicitly racist positions are considered if not unacceptable so at least unpopular, political actors resort to seemingly race-neural but fundamentally racialised issues—that is, they 'play the race card'—in order to tap into existing racial fears and biases among the electorate, reinforcing the connection between race the issue in question in the process. The 'Willie Horton' television ad, run by the National Security Political Action Committee against Michael Dukakis (then governor of Massachusetts) during the 1988 presidential campaign, is arguably the most infamous example of this logic. The narrator of the ad explains that Horton, an African-American convicted for murder in Massachusetts, received several furlough passes from prison, during the last of which he 'fled, kidnapping a young couple, stabbing the man and repeatedly raping his girlfriend'. As the story is told, a menacing photo of Horton is shown over the narrative. These different approaches thus differ in the details, but all share the contention that crime and race are closely intertwined in the United States.

There is no reason, however, for not expecting a similar but more general logic—whereby the identity of the poor and intergroup dynamics affect judgements about the nature of crime, and hence comes with consequences for preferences over alternative crime control policies—to be at work elsewhere. Indeed, plenty of related research give good grounds for thinking that the US is not unique in this context. An extensive literature in social psychology, first, has demonstrated the effects of group boundaries and distinctions in a wide range of social interactions and settings (Tajfel et al., 1971; Tajfel and Turner, 1986; Brown, 2000; Hornsey, 2008; Chen and Li, 2009; for an important and related political economy treatment, see Shayo, 2009). Seeking to explain these patterns, a central tenet of social identity theory so-defined is that individuals 'strive to maintain

or enhance their self-esteem: they strive for a positive self-concept' (Tajfel and Turner, 1979, pp. 40), which in turn derives largely from favourable comparisons between an individual's own social group and relevant out-groups. As a result, group members are motivated to think and behave in ways that enhance in-group status and differentiation, resulting in positive in-group bias and discrimination towards members of out-groups. A large and predominantly experimental literature has shown, moreover, that social categorisation itself is often sufficient to trigger these tendencies: even when group membership is substantively arbitrary or even explicitly random, powerful group-based effects are still observable. Importantly, these effects are also not limited to distributive decisions, but also affect how others are perceived and judged (for a useful review, see Chen and Li, 2009).

More recently, these insights have been combined with research on the psychological process of causal attribution. In this context, attribution refers to the way in which individuals understand and consequently explain the causes of events and, importantly, the behaviour of others around them. It is thus a psychological concept—about how we observe and interpret events around us—rather than a metaphysical one about the nature of causation itself. Again, following Heider's (1958; see also Shaver, 1975) seminal work, a fundamental distinction can be made between situational (or 'external') and dispositional (or 'internal') attribution. Situational attribution, first, roots the cause of a given act or a given behaviour in the external circumstances an individual finds him or herself in. Examples include a punctured tire explained with reference to a pothole, or a spell of unemployment explained by invoking the state of the economy. Dispositional attribution, on the other hand, shifts focus to causal factors that are inherently internal to the agent: his or her personality, mood, attitude, effort or ability. The punctured tire, for example, may be explained by poor driving, and unemployment by laziness or bad decisions on part of the unemployed.

Where do such causal attributions come from? For one, there is considerable evidence to suggest that they are biased in favour of the in-group at the expense of out-groups. In his original formulation, Pettigrew (1979, pp. 464) referred to this as the 'ultimate attribution error', which he defined as 'a systematic patterning of intergroup misattributions shaped in part by prejudice'. In its most straightforward form, this theory of intergroup attribution bias suggests that individuals will tend to attribute positive in-group behaviours (that is, those that are considered prosocial or desirable) to dispositional causes, but

negative in-group behaviours (considered antisocial or undesirable) to external causes. The opposite, in turn, holds true for out-groups: negative acts are disproportionally likely to be attributed to internal causes, while positive acts will be understood situationally (see Taylor and Jaggi, 1974; Stephan, 1977; also Hewstone, 1990). Using the language of social identity theory, these patterns can again be explained by the desire to maintain a positive social identity, and consequently, a positive self-image. In attributing negative in-group behaviours to circumstances, no immediate blame needs to be placed on the group or any of its constituent members. Conversely, attributing negative out-group behaviours to internal characteristics helps to reinforce out-group stereotypes, thus maintaining a sense of group differentiation. Numerous empirical studies, in turn, have found support for these basic predictions (Taylor and Jaggi, 1974; Stephan, 1977; Lau and Russell, 1980; Hewstone and Ward, 1985; Islam and Hewstone, 1993; Guerin, 1999; Cox and Beier, 2014; Tarrant and North, 2004).

Going back to the main concern of this dissertation, these arguments and findings provide good grounds for expecting the level of population heterogeneity among the poor to come with important consequences for how the affluent think about crime, and hence matter for which policies they support in order to deal with crime. As an increasing share of the poor are out-group members—of a different race, for example, or immigrants—the relative importance of internal causal explanations for criminal behaviour should increase. On a first look, it is notable that real-world examples of this phenomenon do not seem hard to come by: a common contention in the US-focused literature on punitiveness, for example, is that many white Americans believe that the over-representation of young, black men in most crime statistics stems from the fact that they are inherently violent and aggressive, and in Europe, far-right parties across the continent have campaigned using an argument linking 'immigrant culture' to increases in crime. These points aside, it follows that increasing population heterogeneity should weaken the relationship between fear of crime and demand for redistribution: again, if the affluent attribute crime to dispositional traits, they have little reason to demand redistribution as a solution to crime as a social problem. Instead, I argue, punitive alternatives should become more important. When a large share of the poor are in-group members, on the other hand, the opposite outcome should follow. As external explanations of crime—which place the locus of control, and hence culpability, beyond the individual—become more

prevalent, non-punitive forms of crime control, including redistribution, should become more attractive. Hence the 'dismal' aspect of this dissertation's argument: while the affluent, through fear of crime as a causal mechanism, become more supportive of redistribution as the level of inequality increases when the poor are similar to themselves, they become more punitive—that is, they support harsher punishment, longer prison sentences, and more policing as protection against the consequences of crime—when the poor are more different.

## 2.6 Private security, sorting and other options

The argument developed over the previous pages can be summarised in a few sentences. Crime is a negative externality of inequality, and because of differences in time preferences, it matters more—relative to material self-interest concerns—to the affluent than it does for those with lower incomes. How the affluent respond to crime, in turn, depends on the social identity of those most likely to engage in crime, which in practice means that it depends on the social identity of the poor. Shared group membership facilitates situational explanations of criminal behaviour, increasing the importance of redistribution as a means of crime control. Divergent group membership, on the other hand, promotes dispositional explanations of crime, and hence increases the demand for security. On the macro-level, therefore, this argument emphasises the level of population heterogeneity among the poor as key to understanding the political economy of crime and punishment.

So far, however, the argument has ignored two potentially important further issues. The first concerns the question of how demand for public security relates to individual investments in private security. Do private security investments crowd out support for publicly provided policing and punishment, and if so, what determines the choice between the two on the individual-level? The second issue relates to the role of residential sorting, and the possibility that both redistribution and security can be viewed not as public but as locally provided club goods. If individuals are geographically mobile, for one, it is possible that a further strategy in response to crime—in addition to redistribution and security, whether public or private—is to move to an area that is perceived as being more safe. And likewise, if local jurisdictions differ in the measures they supply for the purpose of reducing crime, it is possible that sorting also happens on the basis of such differences. What are the implications of these options, and how do they

affect my argument about a trade-off between redistribution and security? The remainder of this chapter looks at each of these questions in turn.

As discussed in some detail earlier in this chapter, public security includes the different measures provided by the government for the purpose of deterring crime: policing, courts, prisons, and related parts of the criminal justice system. In its ideal form, public security thus approximates a public good: within a given area or district, it is non-rivalrous and, generally, non-excludable. Private security, on the other hand, refers to individual actions aimed at self-protection from crime. Following Cook (1986), such actions can be roughly grouped into three, partially overlapping, categories. Some types of self-protection—storing valuables in a safe, for example, or installing window bars, avoiding certain areas at night, and carrying a gun—serve to make crime more costly (and hence less profitable and attractive) to potential criminals. Other types of self-protection increase the probability that a criminal is caught and convicted if a crime does occur. Examples include home alarm systems, surveillance cameras and different types of tracking devices. Yet other self-protection measures, finally, work by decreasing the payoff from successful crime: examples include dye packs, which automatically taint stolen bills, or deciding not to carry cash at night. What these different actions have in common is that they either decrease the probability of victimisation, or reduce the cost of victimisation should it occur.

A large body of empirical research supports the expectation that both public and private security can reduce crime (see the discussion in Section 2.4). Less is known, however, about whether these alternatives are complements or substitutes: does demand for private and public security increase together, or do individuals substitute one type of security for the other? And if so, what determines the trade-off between the two? These questions come with important implications for the argument developed above, since they determine what outcomes we should observe in contexts of high population heterogeneity. If private and public security are complements, fear of crime should give rise to a simultaneous increase in support for public security and demand for private protection. If they are perfect substitutes, the same conclusion does not necessarily follow.

There are good reasons, I argue, for expecting demand for public and private security to increase together. A simple reason for this is that their respective marginal products (with respect to reducing crime or preventing victimisation) are likely not to be independent. Consider the case of home alarm systems, for ex-

ample. At some point, spending more on alarms may be a more efficient means to deter crime than spending more on policing, yet home alarms alone are likely to have little effect. The same logic holds true in the opposite direction. More policing may reduce crime, for instance, but it is reasonable to expect the magnitude of this effect to depend—in no small measure—on actions taken by private individuals, such as the decision to report a crime to the police, testify in court, or invest in some form of surveillance system. Even if this is not the case, however, both public and private security likely exhibit diminishing marginal returns. Having some policing and some law enforcement should make more of a difference than having more policing and more law enforcement, at least beyond a certain level. Similarly for private security: while the first units of self-protection can 'pick the low hanging fruit'—locking the front door, for example—such opportunities likely diminish with increasing investments. Issues of cost aside, this suggests that public and private security are at best imperfect substitutes, implying that demand for both should increase following a rise in concern about crime. This point can be made more formally using the framework proposed by Clotfelter (1977). In this model, individuals value the absence of crime, which in turn is 'produced' using public and private security as inputs. Under the above assumptions, it follows that this production function will exhibit a diminishing marginal rate of substitution between private and public and private inputs. This suggests, again, that demand for either input will increase together following an uptick in demand for security. Unfortunately, very limited empirical work exists on this topic.

In the context of this dissertation, therefore, I expect fear of crime to be associated with increased demand for both public and private security in settings where the poor do not share a salient identity trait with the affluent. While properly analysing the determination of the relative importance of public and private alternatives is beyond the scope of the argument and empirical analysis developed here, it is nonetheless interesting to note that this framework, like any analysis of production, directs attention to two important variables: the relative marginal cost of public and private security, and their relative marginal productivities for individual voters. To the extent that private and public security are imperfect substitutes, this can be used to generate some familiar comparative statics: as the cost of one alternative increases, for example, so should demand for the other. Similarly, holding relative prices constant, demand for one option should increase following a decline as the other becomes less efficient.

Before moving on, it is perhaps interesting to note that related research has produced a range of findings that are largely consistent with these expectations. A clear example is the literature on so-called 'private protection organisations'—more commonly known as the mafia. According to Gambetta's (1993) influential treatment, the 'mafia is a specific economic enterprise, an industry which produces, promotes and sells private protection' (pp. 1). On this account, violence and engagement in other types of criminal endeavours are means rather than ends: the mafia sells protection, and may or may not use violence for that purpose. Tracing the historical emergence of the Sicilian mafia back to the 19th century, Gambetta argues that a key factor facilitating this process was the absence of efficient and sufficiently credible public law enforcement: for both intentional and unintentional reasons, the newly formed Italian state was unable to effectively enforce either contracts or property rights. The mafia consequently emerged to fill this void, at least in part fuelled by public demand. Others have generalised this argument, linking weak state institutions to demand for private protection in a range of different settings (Hay and Shleifer, 1998; Volkov, 2010; Milhaupt and West, 2000). As Varese (2001, pp. 55) notes when discussing the former Soviet and now Russian mafia, for example, 'if the demand for protection that accompanies the spread of market transactions is not met by the state, a demand for alternative sources of protection is then expected to arise'. Superficially at least, this reasoning is consistent with the argument developed above: to the extent that public and private security are imperfect substitutes, demand for private protection should increase as the perceived efficiency of public alternatives declines. Elsewhere, a similar theoretical logic has been used to explain a wide range of phenomena: from the emergence of a large legal private security industry across much of the developing world (see for example, Duffield, 1998; Lemanski, 2004) to the rapid growth of gated communities in many Latin American cities.

It is important, moreover, to be explicit about why I expect a trade-off between redistribution and security, on the one hand, but not between private and public security, on the other. In particular, a critical reader may ask why, if demand for public and private security tend to increase together, should not the same hold true for demand for redistribution and security in general? The reason, again, has to do with causal attribution. As discussed above, I argue that the level of population heterogeneity among the poor significantly affects how criminal behaviour is interpreted and explained: shared group membership between po-

tential criminals and potential victims of crime facilitates situational explanations of crime, including inequality, while divergent identities promotes dispositional explanations of crime, unrelated to inequality. This implies a trade-off, since individuals who view crime as independent of inequality have little reason to demand redistribution as a solution to fear of crime, while the same does not hold true for those who view crime and inequality as causally related. The same logic, however, is not applicable to the relationship between public and private security alternatives: as I have said above, they both feature in my argument as responses to fear of crime when crime is explained dispositionally, implying that the logic giving rise to the binary choice between redistribution and security—differential causal attribution—does not apply to that between public and private security options. Hence my expectation about a trade-off in the former case, but not in the latter.

Moving on, it is also necessary to consider the potentially important role of residential sorting. The argument developed above implicitly relies on two key assumptions: first, that both redistribution and public security can be thought of as pure public goods (that is, as both non-rivalrous and non-excludable), and second, that individuals are not geographically mobile. In theory, either of these assumptions can be relaxed. To the extent that both redistribution—as in the case of many public services, such as eduction—and public security are administered and provided locally, for example, it can be argued that they approximate club rather than public goods: while non-rivalrous, they allow for the exclusion of outsiders. Similarly, it is perhaps not only possible but even probable that a given individual's choice to reside in a particular neighbourhood, area or even city is not independent from perceptions of crime or fear of crime, and, if the former point holds true, from the locally provided bundle of club goods (and taxes).

Although largely bracketed in the empirical analysis developed below, these considerations open up for a number of interesting extensions of my argument. I return to these possibilities in some detail in Chapter 6, but mention them briefly here. In the simplest sense, acknowledging that individuals segregate into localities based directly or indirectly on characteristics and factors related to crime, first, opens up the possibility of residential segregation as an additional, second alternative to redistribution as a solution to fear of crime. On this logic, rather than attempting to reduce crime by supporting increased public spending on some bundle of redistribution and security, individuals can reduce their fear of crime by moving to an area that is perceived as being more safe. On its own,

this argument does not require that the provision of either redistribution or security are locally heterogenous: all that is required is some exogenous variation in the crime rate. This possibility in turn shifts attention towards the factors that drive crime-motivated residential segregation, and the extent to which these correlate with or are orthogonal to population heterogeneity.

Perhaps even more fruitful, however, are the theoretical implications of additionally viewing certain forms of redistribution and public security as club rather than as public goods. Explicitly modelling the supply of these policies aimed at reducing crime as geographically heterogeneous opens up a number of theoretical extensions. First, and in line with Tiebout's (1956) general theory of local public expenditure, one possibility is that individuals sort into different and distinct geographical areas based on their demand for local public services (and conversely, their willingness to pay for such services), including redistributive forms of public spending—public education being the most obvious example—and the level of policing. If this is the case, moreover, residential sorting in the context of my argument thus works through two distinct channels: sorting based on crime, and sorting based on the provision of alternative solutions to crime, and their costs.

Second, this model can be made richer by considering the likely bi-directional and dynamic relationship between sorting and patterns of both crime and fear of crime, and its effect on housing prices. To see this on an intuitive level, consider the case where local jurisdictions differ in the provision of redistribution and policing, and individuals are differentiated by income but share a preference for less crime. If individuals can move, demand for housing in low crime jurisdictions should increase, leading to a rise in property prices. The result is geographic stratification according to income, where the affluent can afford to live in places where crime is not a social problem but the poor cannot. What is more, given the relationship between income and the propensity to engage in crime—as discussed earlier in this chapter, in the context of the Becker model of criminal behaviour—higher property prices should in turn result in less crime. In other words, the housing market allows residential sorting based on crime to translate into stratification based on income, which in turn shifts crime away from more expensive to less affluent jurisdictions. As such, there is a clear parallel between this argument and the large literature on residential sorting in the context of public education.

It is important to stress, however, that these possibilities do not contradict the argument developed over the previous pages. As discussed earlier in this

chapter, for example, it is well established that perceptions of fear of crime are more reflective of aggregate level crime rates than individual victimisation risk, implying that a local reduction in crime as the result of sorting—where crime shifts from affluent to poor neighbourhoods—does not necessarily translate into less fear of crime among the affluent. Likewise, to the extent that beliefs about likely perpetrators of crime are shaped not only by the level of heterogeneity in an individual's residential neighbourhood but also by that in the wider geographical area—the city, for instance—there is no reason for not supposing that my argument should apply in segregated and non-segregated geographies alike. As I return to in some detail in Chapter 6, however, these extensions—and their relationship to the role of population heterogeneity and social identity in shaping preferences over redistribution, on the one hand, and public security, on the other—provide a number of interesting and promising avenues for further work.

## 2.7 Summary and expectations

These concerns aside, this chapter has presented the argument that forms the basis of this dissertation's theoretical contribution. More succinctly, this reasoning can be used to generate the following set of hypotheses, together with a number of background premises. First, I have argued that inequality and fear of crime are positively related. This expectation follows since crime becomes more profitable for the poor as inequality increases, and fear of crime depends, in part, on actual crime.

**Premise 1:** As inequality increases, so does fear of crime.

Turning to preferences, my baseline expectation, first, is that support for redistribution declines with relative income, defined as the difference between the market income of a given individual and the mean. The intuition comes directly from the Meltzer-Richard model, and is based on a pure tax-and-transfer logic.

**Premise 2:** As relative income increases, support for redistribution declines.

Next, the argument implies that the importance of fear of crime for preferences, whether for redistribution or security, increases in income. This follows from my argument about time-preferences: because the poor have shorter time horizons relative to the affluent, current tax-and-transfer motivations will matter more for than longer-term concerns related to inequality. For the affluent, on the

other hand, fear of crime can become a more important driver of behaviour. This implies that the effects of fear of crime on preferences will be income-dependent.

**Premise 3:** The effect of fear of crime on demand for both redistribution and security increases with relative income.

Most importantly, I expect the relationship between fear of crime and demand for both redistribution and security to depend on the level of population heterogeneity among the poor. As the poor grow more heterogenous relative to the affluent, I expect fear of crime to become increasingly associated with support for security, and conversely, less important as a determinant of preferences for redistribution. Empirically, this means that I expect to find significant interaction effects between fear of crime, preferences and population heterogeneity: as the latter increases, the effect of fear of crime on support for redistribution will diminish, while its relationship to demand for security will increase in magnitude. Hence the expectation that fear of crime matters differently in different places.

**Hypothesis 1:** Fear of crime has a positive effect on demand for redistribution, and the magnitude of this effect declines as the level of population heterogeneity among the poor increases.

**Hypothesis 2:** Fear of crime has a positive effect on demand for security, and the magnitude of this effect increases as the level of population heterogeneity among the poor increases.

The subsequent chapters proceed to test the empirical validity of these predictions. As I have said above, Chapter 3 and Chapter 4 do so using cross-sectional survey data from the ESS and GSS respectively, while Chapter 5 presents an experimental test of the micro-level mechanism linking the social identity of potential criminals to distinct crime control preferences among potential victims. Taken together, and with some caveats, these chapters together lend strong support to my central expectations: inequality has a large and highly robust positive effect on fear of crime, and support for redistribution steadily declines with relative income. Perhaps most importantly, I also find that, in the expected directions, that social identity concerns—including the level of population heterogeneity—significantly condition the relationship between fear of crime and preferences for redistribution and security. Chapter 6 concludes by summarising the findings from the dissertation, and draws out their implications.

# Chapter 3

# Demand for Redistribution and Punishment in Europe

The objective of this chapter is to present an initial empirical assessment of the theoretical model developed above, using individual-level cross-sectional data from the European Social Survey, for a set of 16 advanced industrial democracies in Western Europe. As I have discussed at length over the previous pages, the theoretical model proposed in this dissertation generates a number of empirical hypotheses about, first, the effect of inequality on fear of crime, and second, the relationship between fear of crime, population heterogeneity, and demand for redistribution, on the one hand, and support for punitive criminal justice policies, on the other. Leaving aside more complex empirical issues for later chapters—particularly the experimental results in Chapter 5—assessing whether these expectations regarding outcomes are borne out in a relatively straightforward, cross-sectional analysis forms a useful starting point for the investigation.

Applying the theory developed above to the West European democracies is also substantively interesting, for several reasons. Issues to do with crime in general, and the possible relationship between immigration and crime in particular, have recently emerged at the top of the political agenda in several European countries—perhaps most notably in Germany, Italy and Scandinavia, especially Sweden. Across these countries, the pattern has been broadly similar: against the

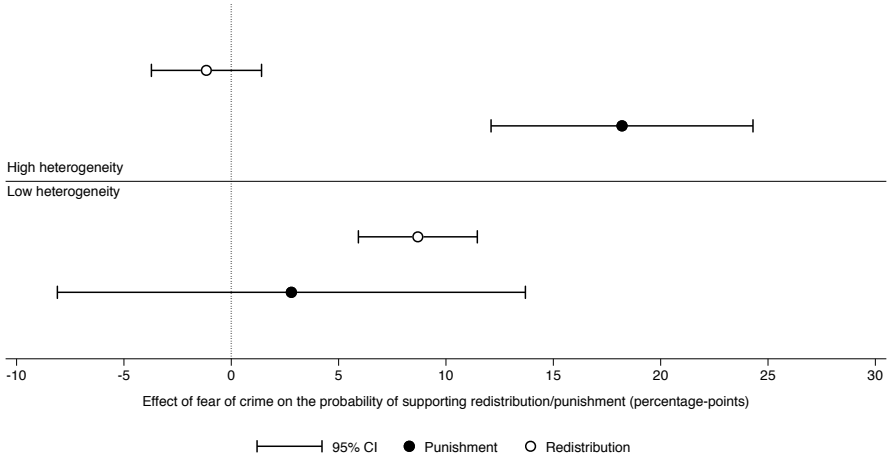
backdrop of increased immigration, parties on the far-right have emphasised an alleged causal link between immigration and crime, as well as called for 'tougher' and more punitive forms of crime control. In Germany, for example, the Alternative for Germany (AfD) recently campaigned using the slogan 'Crime Through Immigration', and in Sweden, the Swedish Democrats (SD) warned of a looming 'rape epidemic caused by immigration' ahead of the 2018 parliamentary elections. Casual observation suggests, in turn, that given the apparent popular appeal of this argument, more mainstream and more traditionally centrist parties have responded either by challenging or by co-opting part of this logic, with varying degrees of electoral success (for a theoretical treatment, see Meguid, 2005). Strategic and partisan dynamics aside, the argument of this dissertation provides a possible explanation for the demand side of these developments: as the level of population heterogeneity among the poor increases, support for punitive forms of criminal justice policy goes up while demand for redistribution as a means of reducing crime falls. In this case, where the poor are immigrants, crime is increasingly explained with reference to dispositional traits—as the AfD writes in their manifesto, the view that crime is embedded in 'family, clan and cultural structures, and aided by language barriers' (AfD, 2017, pp. 63)—rather than seen as the outcome of situational factors, such as socioeconomic disadvantage or inequality.

To foreshadow the results derived below, the analysis developed in this chapter lends a great deal of support to my central expectations. First, in line with the previous literature, I show that the local level of inequality has a highly significant and substantively large positive effect on fear of crime. More importantly, however, the results also demonstrate that the level of population heterogeneity in the lower half of the income distribution has an important mediating effect on the relationship between fear of crime and preferences, both for redistribution and for punishment. As the poor grow more heterogeneous relative to the affluent, fear of crime becomes an increasingly important positive determinant of support for harsher punishment, but matters less and less for redistribution preferences. At low levels of heterogeneity, on the other hand, these relationships are reversed: fear of crime has a positive effect on demand for redistribution, but conversely, has no discernible impact on punishment preferences—at least not for the affluent. In both cases, the effect of fear of crime increases in relative income, in line with the argument that crime matters more for the preferences of the rich than it does for the preferences of the poor. As such, and while keeping in mind that these purely

cross-sectional results alone are not sufficient to establish that these relationships are indeed causal, the analysis developed in this chapter nevertheless provides strong, albeit preliminary support for the argument proposed in Chapter 2.

Additionally, the results suggest that these effects are comparatively large in magnitude—and therefore potentially important. Figure 3.1 below attempts to provide a first illustration of this: it plots the average marginal effect of fear of crime on support for redistribution, on the one hand, and support for harsher punishment, on the other, for an individual with an income $45,600 above the mean (95th percentile), at low (5.0% immigrants, 10th percentile) and high (16.7%, 90th percentile) levels of population heterogeneity among the poor. In the former case, fear of crime is associated with a highly significant 8.7 percentage-point increase in the probability of supporting redistribution, but has no significant effect on the probability of agreeing that criminals should receive harsher pun-

**Figure 3.1:** Marginal effect of fear of crime on the preferences of the affluent




*Note:* The 'dots' represent the average marginal effect of fear of crime, together with 95% confidence intervals, calculated using the results from Model 4 in Table 3.4 (for redistribution, see page 103), and Model 4 in Table 3.5 (for punishment, see page 115). Both are mixed-effects models with random country-level random-effects and year fixed-effects. The marginal effects can be interpreted as the change in the probability of supporting redistribution or harsher punishment associated with going from not being afraid of crime to being afraid of crime. 'High heterogeneity' implies that 16.7% (90th percentile of the sample distribution) of those with incomes below the country-year mean are either first- or second-generation immigrants, while 'low heterogeneity' implies that 5.0% are so (10th percentile). All estimates hold income constant at $45,600 above country-year mean income. See Sections 3.4.2 and 3.4.3 for details.

ishment. When the level of heterogeneity is high, in contrast, these relationships are transposed: fear of crime has no effect on support for redistribution, but is instead associated with an all but negligible 18.2 percentage-point increase in the likelihood of demanding harsher punishment. These results thus suggest that identity has important consequences for the political economy of crime and punishment. They also point to a somewhat dismal conclusion: in Europe, the affluent respond to fear of crime by supporting more redistribution when the poor are similar to themselves, but respond by demanding harsher and more punitive sanctions for criminals when the poor are, in some salient way, different from themselves. For the poor, on the other hand, these relationships are very different: while fear of crime never has more than a substantively small effect on their demand for redistribution, it does have a significant positive effect on their punitive preferences at low—but not at high—levels of population heterogeneity.

## 3.1 Data

The analysis developed in this chapter relies primarily on individual-level survey data from all currently available waves of the European Social Survey (ESS), carried out in 2002, 2004, 2006, 2008, 2010, 2012, and 2014, respectively. Similar datasets centred around individual-level data from the ESS have been used extensively in the previous comparative political economy literature on the determinants of redistribution preferences (Rueda and Stegmueller, 2016; Rueda, 2018; Finseraas, 2009; Rehm, 2009; Cusack, Iversen, and Rehm, 2006; Häusermann, Kurer, and Schwander, 2015; Luttmer and Singhal, 2011; Senik, Stichnoth, and Straeten, 2009, as well as many others). There are several reasons for this: the ESS contains measures of most concepts of interest to this literature, includes a large number of respondents from across both Western and Eastern Europe, and is widely considered to be of a rigorous social scientific and statistical standard. As Stegmueller et al. (2012) write, for example, the ESS 'ensures exceptionally high standards regarding the comparability of questionnaires, sampling designs and population coverage, making it an ideal tool to analyse a large number of countries simultaneously' (pp. 485). Hence, I take it that the choice to use ESS data for the analysis developed in this chapter is comparatively uncontroversial.

Given the specific objectives of this chapter, moreover, the ESS also comes with a number of more narrow merits. Most importantly, it is—to the best of my

knowledge—the only available, reasonably large-scale cross-national survey that includes the full range of measures necessary to test the central empirical implications of the argument developed in Chapter 2: a consistent and cross-nationally comparable measure of income, together with valid and reliable items tapping into fear of crime, preferences for redistribution, and respondents' punitive attitudes. Although several other large, well-known survey programmes include some of these indicators, only the ESS covers all of them. The surveys fielded as part of the International Social Survey Program (ISSP), for example, not only have an income definition that varies between countries within waves (and often, additionally within countries across waves), but also do not contain measures of either fear of crime or of punitive preferences—both of which are central to the theory examined here. A similar conclusion likewise holds true for the data provided by the World Values Survey, which again includes neither a measure of fear of crime nor a consistent income variable. Hence, for the purposes of the analysis developed here, the ESS is arguably a reasonable choice. Finally, I address the perhaps most significant disadvantage with the ESS—that it does not include the US—in Chapter 4, where I develop a similar empirical analysis using American data from the General Social Survey (GSS).

More practically, the analysis is divided into three main parts: the first looks at the relationship between inequality and fear of crime, the second at the effect of fear of crime on redistribution preferences, and the third at demand for harsher punishment. In the former two, the investigation relies on data from all seven currently available rounds of the ESS. After accounting for missing data, this allows me to include approximately 145,000 individual-level observations in the main regression models, drawn from 149 regions in 16 countries, and a total of 93 distinct country-years (not all countries are included in every wave of the survey, see Appendix A and Table A.3 for details). The third part of the analysis is somewhat smaller in scope, since the ESS only includes the items necessary to test the punishment side of the theoretical argument—most importantly, the main dependent variable measuring support for harsher punishment—in the surveys carried out in 2008 and 2010. This restricts the number of individual-level observations to around 35,000, and the number of country-years to 24. This reduced sample size is not ideal, but still sufficient for an initial assessment of the relationships of interest. In order to have a simple and unambiguous operationalisation of heterogeneity, moreover, the sample used below includes native-born

respondents only. As discussed previously, finally, I limit the scope of the analysis to advanced industrial democracies with 'mature' welfare states: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, and the United Kingdom.

### 3.1.1 Dependent variables

**Redistribution preferences** To measure support for redistribution, the analysis relies on a standard item asking respondents if they 'strongly agree', 'agree', 'neither agree nor disagree', 'disagree', or 'strongly disagree' with the statement that 'the government should reduce differences in income levels'. The same question is used by nearly all previous studies employing ESS data to analyse demand for redistribution (for example, Rueda and Stegmueller, 2016; Rueda, 2018; Finseraas, 2009; Rehm, 2009; Cusack, Iversen, and Rehm, 2006; Stegmueller et al., 2012; Stegmueller, 2013; Jaeger, 2006; Häusermann, Kurer, and Schwander, 2015; Luttmer, 2001), and is similar to those included in other comparable surveys, including the GSS. In order to keep both estimation and interpretation as simple as possible, the regressions below use a binary transformation of this variable, coded as 1 if a respondent 'agrees' or 'strongly agrees' with the statement, and as 0 for all other non-missing responses. Using a binary dependent variable allows for the regression results to be presented as simple predicted probabilities—that is, as the probability supporting redistribution, rather than the probability of 'strongly agreeing', 'agreeing', and so on—hence permitting me to avoid the complexities involved in deciphering ordinal regression results. As a final robustness test, however, I replicate the main analysis using the untransformed five-point ordinal variable, and show that this does not main alter the substantive findings (for details, see Table 3.6 in Section 3.5.1 below). Finally, with this and all other variables used in the analysis, 'don't know' and other non-responses are treated as missing.

As a measure of redistribution preferences, this question has a number of strengths, accompanied by some clear weaknesses. Many of these have been noted by previous authors using the same item for similar purposes (for an overview, see Finseraas, 2009). On the upside, the question explicitly identifies the government as the agent of redistribution, and hence taps directly into views about the role of the state in making incomes more equal. This aligns it closely with my theoretical concerns, which relate income inequality and crime to preferences for redistri-

bution, rather than to preferences over alternative means—efforts by individuals themselves, organised labour and corporatism, etc.—through which greater equality may also be achieved. Moreover, the question is arguably very clear: the wording is simple and straightforward, and it does not require respondents to have any particular or specific policy knowledge in order to answer it intelligently.

On the other hand, this item has also been criticised from several angles. One potential problem concerns its generality, and the fact that the statement does not specify the type of policy instruments that may be employed to reduce inequality. To the extent that attitudes towards progressive taxation are qualitatively different from preferences over social insurance or cash transfers, for example, this induces some ambiguity as to what the item is ultimately measuring. Another common line of argument points out that the question does not remind respondents of the government's budget constraint, and emphasises the possibility that responses might have differed had the statement included some reference to higher taxes, or any of the other trade-offs that 'reducing differences in income levels' might imply. As with all survey items measuring preferences or an individual's willingness to pay, finally, a more general concern relates to the possibility of cheap talk: that is, the common observation that answers to real and hypothetical valuation questions can be quite different (Cummings and Taylor, 1999; List, 2001). Although there is little that can be done about this latter possibility, neither of the other issues is unambiguously negative (for a useful discussion, see Finseraas, 2009). Given that the theoretical model developed above concerns demand for redistribution in general, the lack of policy specificity may well be seen as an advantage. Likewise, the absence of an explicit reference to the government budget constraint also has ambiguous implications, since it is unclear whether such references would prime respondents any less than what is currently the case. Moreover, even accepting that these criticisms may carry some validity, the fact remains that out of the possible alternatives available in the ESS, this variable remains by far the least problematic.

For reference, Table 3.1 presents the distribution of responses to this item, using the untransformed five-point ordinal survey variable. These simple descriptive statistics show that redistribution enjoys thoroughly widespread support in Western Europe: when combined, the 'agree' and 'strongly agree' categories account for more than 70% of all observations. In contrast, less than 15% of respondents explicitly oppose redistribution, while a similar share remain neutral.

**Table 3.1:** Support for redistribution in Western Europe

| | % | Cumulative % | *N* |
|---|---|---|---|
| Strongly agree | 26.93 | 26.93 | 57,184 |
| Agree | 43.37 | 70.29 | 92,099 |
| Neither agree nor disagree | 15.04 | 85.33 | 31,937 |
| Disagree | 12.08 | 97.41 | 25,658 |
| Strongly disagree | 2.59 | 100.00 | 5,502 |

As the analysis below reveals, however, these aggregate level figures hide a considerable amount of variation, both between countries, and even more so, between different groups of individuals within countries. Additional and more detailed descriptive statistics are presented in Section 3.3, and summary statistics—for this and for all other variables discussed below—are included in Appendix A.

**Punitive preferences** Given the theoretical argument motivating the analysis developed in this chapter, the 2008 and 2010 rounds of the ESS include useful item measuring support for harsher punishment for those found guilty of crime. The survey question prompts respondents with the statement that 'people who break the law should be given much harsher sentences than they are these days', and records responses on the same five-point ordinal scale discussed above, ranging from 'strongly agree' and 'agree', to 'neither agree nor disagree', 'disagree', and 'strongly disagree'. Although relatively new to the ESS (and consequently, only sparsely used by the previous literature on punitive attitudes in Europe) this question closely resembles an item included in all waves of the General Social Survey (GSS) since 1972, which has been used extensively in previous work on public punitiveness in the Unites States (for example, Barkan and Cohn, 2005; Johnson, 2001; Taylor, Scheppele, and Stinchcombe, 1979; Unnever, Cullen, and Applegate, 2005; Wilson and Dunham, 2001; Brown, 2006). As with the measure of redistribution preferences—and for the same reasons—the analysis below relies on a binary transformation of this variable, with 'agree' and

'strongly agree' responses coded as 1, and all other non-missing responses coded as 0. To demonstrate that this decision does not drive my results, however, I use the untransformed ordinal variable as a robustness test in Section 3.5.

Several arguments can, again, be made both for and against the validity of this item. One concerns relates to the 'much harsher' part of the statement: in theory, it is possible to think that punishment should be made harsher, but disagree that it should be made much harsher. Additionally, an important set of issues again stem from the generality of the statement: it does not specify what type of crime it is referring to, what type of punishment it is asking about, or how 'harsher' should be interpreted. To the extent that punitive attitudes are multidimensional—if, for example, views about the appropriate size of a fine for a minor delinquency are qualitatively different from, say, views about capital punishment or life in prison for the most serious crimes—this is a potential problem. Yet, there are reasons for arguing that this drawback is not fatal. For a start, the theoretical model developed in Chapter 2 relates inequality, fear of crime and identity to punitiveness in general, and does not make an explicit distinction between the potentially more fine grained dimensions of punitive preferences. Consequently, whereas I expect fear of crime to become more strongly related to support for punitive criminal justice policy as the level of heterogeneity increases, it does not matter for the argument whether this takes the form of, for instance, increased support for specific sentencing policies (three-strikes legislation, mandatory sentencing requirements), specific penal sanctions (parole restrictions, longer prison sentences, the death penalty), or expanded law enforcement efforts (for a conceptual discussion of these issues, see Adriaenssen and Aertsen, 2015). Moreover, empirical evidence suggests that these various dimensions of punitiveness tend to be highly correlated in practice (McCorkle, 1993; Gault and Sabini, 2000), as well as relatively stable even when subjects are primed on alternative crimes of varying severity. As with the item used to measure redistribution preferences, finally, even if this question is far from perfect, it remains the best one available for examining the argument cross-nationally.

Table 3.2 shows the distribution of responses to this item, again based on the full pooled sample from all available country-years. As with support for redistribution, the arguably most striking feature of these summary statistics is the very high degree of support for harsher punishment observed in Europe: on average, close to 70% of respondents agree with the statement that 'criminals should be

**Table 3.2:** Support for harsher punishment in Western Europe

| | % | Cumulative % | *N* |
|---|---|---|---|
| Strongly agree | 25.24 | 25.24 | 13,237 |
| Agree | 42.79 | 68.04 | 22,438 |
| Neither agree nor disagree | 20.20 | 88.24 | 10,594 |
| Disagree | 10.34 | 98.58 | 5,423 |
| Strongly disagree | 1.42 | 100.00 | 744 |

given much harsher sentences'. In contrast, only about 10% of respondents express explicit disagreement, and no more than 1.4% strongly disagrees. While this level of support for increasing the level of punitivity in the criminal justice system may appear excessively high, it remains largely comparable with the results from previous studies using alternative datasets (see, for example, Allen et al., 2006; Applegate and Sanborn, 2011; Roberts and Indermaur, 2007). Once again, however, these aggregate level figures hide a considerable amount of lower-level systematic variation, as the formal analysis developed below explores in some detail.

### 3.1.2 Independent variables

Following the logic of the theoretical model, the analysis developed in this chapter includes four substantively important independent variables: on the individual-level, fear of crime and respondents' relative income, and on the macro-level, the level population heterogeneity among the poor and inequality. Again, further descriptive statistics for these variables are provided in Appendix A.

**Fear of crime** The analysis developed in this chapter operationalises fear of crime using an item that asks respondents: 'How safe do you—or would you—feel walking alone in this area after dark?'. Responses are recorded on a four-point ordinal scale, ranging from 'very safe' and 'safe' to 'unsafe' and 'very unsafe'. Given the difficulty of disentangling the substantive difference between feeling 'very safe' and 'safe', or between feeling 'unsafe' or 'very unsafe', the regressions below

use a binary transformation of this variable, with the two 'unsafe' categories coded as 1, and the two 'safe' categories coded as 0. As with the dependent variables, this binary transformation also simplifies interpretation of the results.

Variations on this question have been used extensively in previous research regarding fear of crime, both in Europe and the United States. As Warr (2000, pp. 458) points out, it has also been problematised on several levels: '…it is hypothetical (how afraid would you be), is limited to nighttime, does not mention crime, and only crudely measures intensity' (for similar discussions, see also Ferraro and LaGrange, 1987; Hale, 1996; Ferraro, 1995; Garofalo, 1979; Garofalo and Laub, 1978; Tseloni and Zarafonitou, 2008). On the other hand, empirical evidence suggests that estimated of fear of crime based on this variable tend to be substantively similar to those derived from alternative items not suffering from the above issues, implying that they might be less problematic than what is sometimes argued (see especially Warr, 1995; Vauclair and Bratanova, 2017). As with the measure of demand for redistribution, finally, even if this item is ultimately not perfect, it remains the best one available in the ESS.

As Rueda and Stegmueller (2016) emphasise, however, a more serious criticism concerns the fact that the question can be argued to refer more to violent crime than to property crime. Although this possibility is not necessarily an issue in its own right, it does become problematic to the extent that inequality affects property crime, but does not have any similar effects on violent crime. Nonetheless, and as discussed in some detail in Chapter 2, there are good reasons to remain sceptical about this conclusion. For one, property crime and violent crime are intimately linked in practice—consider, for example, kidnappings, robberies, and similar cases in which violence or the threat of violence is the by-product, intentional or not, of crimes motivated by economic gain. Theoretically, moreover, Bourguignon (2001, pp. 208) develops an extension of the standard economic model of crime from Becker (1968), in which violent crime is 'related to the control of illicit activity like gambling, prostitution, or drug dealing and trafficking'. Since these activities are themselves related to inequality, it follows that the positive relationship between inequality and property crime should hold for violent crime as well (for alternative models with parallel implications, see Imrohoroglu, Merlo, and Rupert, 2000; Ehrlich, 1973). From a sociological perspective, relative deprivation theory also produces similar expectations (Merton, 1938; Stack, 1984). Empirical studies, finally, have tended to confirm the

expectation that inequality matters not only for property crime but also for violent crime. Fajnzylber, Lederman, and Loayza (2002a), for instance, find robust evidence that inequality increases the prevalence of both property and violent crime, and that this conclusion holds true both within countries over time as well as cross-nationally (also Fajnzylber, Lederman, and Loayza, 2002b; Kelly, 2000).

**Relative income** Given the important role of income for the theoretical argument developed above, operationalising it properly is a cornerstone of the empirical analysis developed here. As discussed at length in Chapter 2, income matters for the theoretical model in two distinct but related ways: first, because it determines whether an individual has a self-interested tax-and-transfer motive for demanding (or opposing) redistribution, and second, because it affects the degree to which individuals are willing to substitute spending on crime prevention and security for other consumption through its relationship to time preferences. The former mechanism explicitly concerns relative income, defined as the difference between a given respondent's income and mean income, rather than nominal income. The reason for this is straightforward: what matters from the perspective of material self-interest is not how much individuals earn, but how their incomes compare to those of others in their country of residence. An individual earning € 20,000 in Germany (where mean income is approximately € 44,500), for example, has a stronger self-interested motivation for demanding redistribution than an individual with the same income in Portugal, where mean income is around € 13,900. Therefore, simply using nominal income to capture material self-interest is highly problematic. Given this, the analysis below instead includes a direct and cross-nationally comparable measure of relative income.

The ESS surveys from 2008 and onwards (Round 4-7) record net household income using a survey item that asks respondents 'Using this card, please tell me which letter describes your household's total income, after tax and compulsory deductions, from all sources? If you don't know the exact figure, please give an estimate'. In the surveys from 2002, 2004, and 2006, the question asks about 'net income' rather than income 'after tax and compulsory deductions', but is otherwise identical. Until 2006 (Round 1-3), the card shown to respondents contained twelve income categories with different ranges, but which were the same for all included countries in a given survey-year. Starting in 2008, household income is instead recorded in ten non-equal income bands, roughly corresponding to

the deciles of each country's income distribution. Consequently, the categories not only vary over time but also between countries in the same wave.

To develop a variable that accurately captures the theoretical concept of real relative income, I follow a similar strategy to that proposed by Rueda and Stegmueller (2016, see also Rueda, 2018; Dimick, Rueda, and Stegmueller, 2016). To that end, I first transform each income band to its midpoint, and use the method proposed by Hout (2004) for the GSS to impute a 'midpoint' for the open-ended top category. In short, this approach extrapolates from the boundaries and frequencies of the top and the second-to-last income categories to estimate a midpoint for the top category, based on a modified Pareto distribution. This process is the same for the surveys with ten and those with twelve income bands. In order to derive a direct measure of the income distance to the mean, I then centre each country-year specific income variable on its mean. As such, negative values denote incomes below the mean, and positive values denote incomes above the mean. Finally, given that differences in inflation as well as in relative price levels imply that the substantive meaning of having an income of some given nominal amount above or below the mean varies between countries as well as over time, I convert all income figures to units of 10,000 PPP-adjusted 2010 US dollars, using CPI and PPP rates from the OECD. This means that two respondents with the same relative income will always have the same purchasing power relative to that of someone with mean income, even if they are from different countries in different survey years and have different nominal incomes.

**Population heterogeneity** On the macro-level, one of my main theoretical interests lies in measuring the level of population heterogeneity those most likely to engage in crime. As discussed in some detail in Chapter 2, this means that I am first-and-foremost interested in capturing the level of heterogeneity among the poor. To do so—and since no external measure of income-dependent heterogeneity exists, to the best of my knowledge—the main part of the analysis developed below relies on a survey-based variable: it takes survey responses about self-reported country of birth, parents' country of birth, and income to calculate the percentage of first- and second-generation immigrants below the mean in each country-year. Given the operationalisation of income, this is a reasonably straightforward task. First-generation immigrants are defined as those born abroad, and second-generation immigrants as those with two foreign-born par-

ents. The main advantage of this approach is that it provides a direct and explicit measure of population heterogeneity in the lower half of the income distribution, which is generally not possible with alternative, macro-level or aggregate measures of heterogeneity. In this sense, it forms a valuable component of the analysis, facilitating a comparatively close match between the theory and the empirics.

Nonetheless, a potential issue with this operationalisation of heterogeneity is that the resulting estimates come with a degree of uncertainty. While this problem is somewhat alleviated by the fact that the analysis uses countries (rather than regions, for example) as the macro-level unit of analysis, it remains relevant for some of the smaller countries in the sample, where the number of respondents with a non-native background is correspondingly small. To hedge the analysis against potential idiosyncrasies of this variable, therefore, in Section 3.5 it is substituted with variable that measures the percentage of foreign-born individuals in the population as a whole, based on data from the OECD (2019b). A similar approach to measuring heterogeneity has been used in much of the previous comparative political economy literature. This specification eliminates the uncertainty contained in the survey-based measure of heterogeneity, yet at the same time does not necessarily measure the aspect of heterogeneity that is most relevant for my theoretical logic. As an additional robustness test, I also report the results of a model using a similar measure of heterogeneity in the entire population but derived from survey responses to the ESS. As shown in Section 3.5, however, neither substitution significantly affects the results from the main analysis.

**Inequality** Finally, in the first part of the analysis presented below, my main interest lies in estimating the relationship between inequality and fear of crime. As accurately noted by Rueda and Stegmueller (2016), an important methodological question given this objective concerns the theoretically relevant level of inequality. In my argument—as in theirs—inequality matters because of its relationship to fear of crime: as I have suggested, inequality affects individuals' fear of crime by simultaneously determining the opportunity cost of crime and the relative payoff tied to criminal activities. As Kelly (2000, pp. 530) aptly summarises this reasoning, 'inequality leads to crime by placing low-income individuals who have low returns from market activity in proximity to high-income individuals who have things worth taking'. Through its connection to crime, inequality consequently affects fear of crime. This suggests, in turn, that the level of inequality that matters

to my argument is comparatively local in geographic terms. To see this, consider the following. For an individual living in Marseille, for example, it is reasonable to expect that what matters for the decision whether or not to engage in crime is the relative difference between payoffs from crime and legitimate work in Marseille, not the average difference between these two activities in France as a whole. On the benefit side, the wealth of individuals living in elsewhere—in Paris, for instance, some 770km away—arguably matters less for this decision than the wealth of individuals living nearby, just like the employment opportunities available in Paris matter less than those available locally. Again, the same reasoning applies to the relationship between inequality and fear of crime.

Given these concerns, my analysis of the relationship between inequality and fear of crime employs regions—rather than countries—as the main macro-level unit of analysis. The ESS includes sub-national geographic identifiers for all 16 countries included in the main analysis, allowing me to distinguish 149 regions in total (comprised of a combination of NUTS-1 and NUTS-2 regional divisions). To measure inequality, I derive estimates of regional Gini coefficients using the relative income variable discussed above, after imputing incomes for the top-coded categories using the methodology proposed by (Hout, 2004) and correcting for over-time changes in price levels. To obtain useable sample sizes on the regional level, finally, I follow Rueda and Stegmueller (2016) and pool observations across all survey-years when calculating these estimates, and apply corrections for both non-random sampling and small sample bias. As with my measure of population heterogeneity, an important downside of this approach is that the resulting inequality estimates come with a degree of uncertainty. Again, however, data availability limits what I can do: no ready-to-use measure of regional inequality in Europe is currently available, and to the best of my knowledge no public micro-level dataset exists that would allow me to improve on the estimates derived directly from the ESS.

### 3.1.3 Macro-level control variables

As I discuss in some detail below, the regressions models used in the main part of this chapter's analysis—looking at the interactive effect of fear of crime, relative income and population heterogeneity on preferences for redistribution and punishment—are by default relatively complex. To avoid concerns about degrees

of freedom, therefore, I deliberately keep these models as parsimonious as possible. To that end, in the main analysis, I only include macro-level controls for the existing level of redistribution (in the models estimating the determinants of redistribution preferences) and the existing level of punitiveness (when estimating punishment preferences). Additional macro-level controls variables, as well as country fixed-effects, are instead added as robustness tests in Section 3.5.

**Social expenditure per capita** The analysis of the determinants of redistribution preferences includes a control for existing levels of redistribution. Practically, redistribution is operationalised using data on social expenditure per capita from Eurostat, measured units of 1,000 PPP-adjusted 2005 Euros. The main reason for why controlling for actual levels of redistribution in this type of analysis is important has to do with the possibility that responses to the question of whether or not the government should reduce income differences take into account how much redistribution the government is already providing, at least for some respondents. Hence, agreement with the statement may suggest that an individual wants the government to do more to reduce income differences, rather than simply indicate that the respondent wants the government to do something to that end. An important implication of this is that expressed support for redistribution could mean one thing in Denmark (where the existing level of redistribution is relatively high), for example, but something quite different in Ireland (where it is much lower). The opposite could also hold true: opposition to redistribution in highly redistributive countries may have a different meaning than opposition to redistribution in countries that are less redistributive.

**Punitive criminal justice policy** A similar reasoning holds true for punitive attitudes. Arguably, including a control for actual punitiveness is even more important than controlling for actual redistribution, since the survey item used to measure punitive preferences is explicitly comparative: it asks if criminals should receive harsher sentences that what is currently the case, rather than what type of sentence they should receive simpliciter. Since the level of punitivity in the criminal justice system is relatively difficult to measure directly, however, I use two complimentary variables to control for existing punitivity: first, the total prison population (limited to prisoners who have been convicted of a crime), and second, the number of police officers. Both variables are in units of individ-

uals per 100,000 population, and are taken from the criminal justice statistics database compiled by the United Nations Office on Drugs and Crime (UNDOC, 2019). To save space, once again, the main analysis relies on models using the incarceration rate only to control for existing levels of punitivity, while as a robustness test, Table 3.7 presents results for a model estimated with the police rather than the incarceration rate. As with the two heterogeneity measures, the substantive results are the same independently of this choice.

**Regional level controls** In the analysis of the relationship between inequality and fear of crime—where parsimony is less of a concern, given both the relative simplicity of my model specifications and the larger number of regions—finally, I add controls for regional GDP per capita, the harmonised regional unemployment rate, and the percentage of foreign-born individuals in the region. All variables are taken from the Eurostat Regional Database (Eurostat, 2019).

### 3.1.4 Individual-level control variables

In addition to the above, the analysis also includes a standard battery of individual-level control variables that have been shown in the previous literature to matter for both redistribution and punishment preferences. The same or nearly the same set of controls have been used by Rehm (2009), Iversen and Soskice (2001), Rueda and Stegmueller (2016), Corneo and Grüner (2002), Finseraas (2009), King and Maruna (2009), Costelloe, Chiricos, and Gertz (2009), and others. Additional controls are introduced as robustness tests in Section 3.5.

**Age** Most models below include a control for age, measured in years, together with its square. Previous research has shown that support for redistribution tends to increase with age. Common explanations of this regularity suggest that older people may be more likely to support redistribution because they are disadvantaged in the labour market, especially in terms of re-employment opportunities, or that generations brought up in times of relative economic insecurity tend to prioritise material over non-material values (Inglehart, 2008). The relationship between age and punitive attitudes is less consistent (for example, Costelloe et al., 2002; Payne et al., 2004; Rossi and Berk, 1997; Kury and Ferdinand, 1999).

**Gender** I also include a control for gender, coded as an indicator of whether the respondent is a woman. Most previous studies find that women tend to be more supportive of redistribution than men (for instance, Iversen and Soskice, 2001; Svallfors, 1997; Rehm, 2009; Iversen and Rosenbluth, 2006; Finseraas, 2009), although explanations offered for this observation differ. Once again, the effect of gender on punishment preferences is less systematic, although some studies report that women tend to be more supportive of treatment and rehabilitation and less supportive of punishment relative to men (Applegate, Cullen, and Fisher, 2002; Gault and Sabini, 2000; Blumstein and Cohen, 1980).

**Educational attainment** In line with almost all of the previous literature on demand for redistribution as well as punitive preferences, the analysis developed in this chapter controls for educational attainment. Empirical evidence suggests that education is negatively correlated both with demand for redistribution (Rehm, 2009) and support for punitive criminal justice policies (Dowler, 2003; Payne et al., 2004; Rossi and Berk, 1997). Practically, educational attainment is operationalised using an indicator variable coded as one if the respondent has completed some form of tertiary education, and as zero otherwise.

**Religiosity** The analysis also controls for religious attendance, intended to capture respondents' religiosity. To that end, I include an indicator variable coded as one if the respondent attends a religious service at least once a week, and as zero otherwise. Previous research suggests that religious individuals tend to be both less supportive of redistribution (Scheve and Stasavage, 2006; Stegmueller et al., 2012; Stegmueller, 2013) and more punitive relative to those who are not religious. Within both literatures, moreover, differences between religious and secular individuals tend to be greater than those between individuals belonging to different denominations, or equally, different religions.

**Marital status** The analysis also controls for marital status, using a dummy variable indicating if the respondent is married or in a civil partnership. Although evidence regarding the relationship between civil status and demand for redistribution is mixed, there appears to be a relatively robust relationship between being married and being more punitive (for example, see Dowler, 2003).

**Additional controls in fear of crime equation** Finally, in addition to the above, the main models estimating the determinants of fear of crime also controls for a number of individual-level factors that have been shown to matter for fear of crime (see, for example, Vauclair and Bratanova, 2017): labour force status (an indicator of whether the respondent is in the labour force), unemployment (an indicator for being in unemployment), household size, and prior victimisation (using an item that asks respondents if they or a member of their household has been 'the victim of a burglary or an assault in the last five years').

## 3.2 Statistical model

In what follows, my main objective is to estimate how fear of crime matters for redistribution and punitive preferences, conditional both on relative income (an individual-level variable) and the level of population heterogeneity (on the country-level). Consequently, I use models that allow variables at the country-level to affect relationships on the individual-level, and specifications that include a three-way interaction between relative income, fear of crime, and heterogeneity. The analysis of the effect of inequality on fear of crime, on the other hand, is less complex, since it does not imply any similar cross-level interactions.

More generally, some of the most challenging methodological issues that arise when analysing any form of cross-national survey data have to do with clustering and non-independence: that is, the fact that because observations from the same macro-unit—the same country, for example—share a multitude of unobserved characteristics that vary across but not within such units, the standard assumption of independent and identically distributed errors is often violated. Failure to account for this type of clustering, in turn, risks producing incorrect (and underestimated) variance estimates, leading over-confident inference tests: standard errors that are too small, confidence intervals that are too narrow, test statistics that are too high, and p-values that are too low. Properly accounting for clustering therefore is a necessary cornerstone of the empirical analysis developed below.

To deal with these issues, the analysis below relies on two different types of regression model for categorical dependent variables, both of which are robust to within-unit clustering. The first is a standard pooled logistic regression model, estimated with cluster corrected—this technique goes under a number of names, with cluster robust, modified sandwich, Huber-White, and Eicker-Huber-White

being the most common—standard errors to account for the non-independence between observations from the same country. In this model, point estimates for the regression coefficients are estimated in the same manner as in an ordinary logistic model, but the model based variance estimates are adjusted post-estimation to account for any within-cluster dependencies in the data (for a useful technical treatment, see Hardin and Hilbe, 2012). This approach still assumes that observations are independent across clusters, but allows the independence assumption to be relaxed for observations from the same macro-unit.

More formally, a useful way of thinking about the basic logistic regression model is to derive it as a latent variable model. Both dependent variables used in this chapter—one about support for redistribution, the other about support for harsher punishment—are binary, and measure only agreement ($\omega = 1$) or disagreement ($\omega = 0$) with the respective statements. However, it is plausible to suppose that respondents have, or at least can have, a continuum of attitudes about redistribution and punishment, which are only imperfectly captured by these binary survey variables. This continuum of attitudes, in turn, can be conceptualised as supposing that respondents have different values on a latent and unobserved variable, $\omega^*$, which in turn determines their responses on the observed binary dependent variable $\omega$. Using this framework, for each country $j \in \{C_1, C_2, C_3, \ldots, C_j\}$, individual $i \in j$, and time $t$, a general form of the models estimated below can be written as,

$$\omega^*_{ijt} = \alpha + \beta_1(y_{ijt} - \bar{y}_{jt}) + \beta_2 x_{2ijt} + \beta_3 x_{3jt} + \beta_4(y_{ijt} - \bar{y}_{jt})x_{2ijt} + \beta_5(y_{ijt} - \bar{y}_{jt})x_{3jt} + \beta_4 x_{2ijt}x_{3jt} + \beta_6(y_{ijt} - \bar{y}_{jt})x_{2ijt}x_{3jt} + \mathbf{W_{ijt}b} + \lambda_t + \epsilon_{ijt} \tag{3.1}$$

where $\omega^*_{ijt}$ is the latent (that is, unobserved) variable measuring support for either redistribution or punishment, and that determines the outcome on the observed dependent variable, $(y_{ijt} - \bar{y}_{jt})$ is relative income, $x_{2ijt}$ is fear of crime, and $x_{3jt}$ is the share of first or second-generation immigrants below the mean. The coefficients $\beta_1$ to $\beta_3$ capture the direct effect of these variables, and $\beta_4$ to $\beta_6$ their three two-way and one three-way interactions. $\lambda_t$ are time fixed effects, which capture common time specific shocks affecting individuals in all countries. $\mathbf{W_{ijt}}$ is a matrix of the individual- and macro-level control variables, and $\mathbf{b}$ is their coefficients. As I have said above, the error term $\epsilon_{ijt}$ is taken to follow a

logistic distribution, and is assumed to be independent and identically distributed across countries, but not necessarily so within countries. The link between the observed responses $\omega_{ijt}$ and the latent variable $\omega^*_{ijt}$ is then given by:

$$\omega_{ijt} = \begin{cases} 1 & \text{if } \omega^*_{ijt} > 0 \\ 0 & \text{if } \omega^*_{ijt} \leq 0 \end{cases} \tag{3.2}$$

The model is estimated by maximum likelihood, and the variance estimates are then adjusted to account for the non-independence between observations from the same country. This simple cluster correction approach has two main comparative advantages. The first has to do with the fact that it is comparatively simple, both in terms of estimation and interpretation. Moreover, since I am not interested in disaggregating the country-level effects, it follows that part of the benefit associated with more complex multilevel models does not apply. The second motivation for including simple cluster corrected models in the analysis is that this approach relies on relatively few, possibly unrealistic assumptions. As I discuss below, for example, consistency in standard multilevel logistic models require that the macro-level random effects are normally distributed, that the random effects and the covariates are independent, as well as conditional independence not only between the responses and the covariates but also between the responses and the random effects. In contrast, a standard logistic model with cluster corrected standard errors requires neither of these assumptions.

The second type of regression used in the analysis below is a standard random-intercept mixed-effects logistic model. Rather than simply adjusting the variance estimates from a standard pooled regression, this model accounts for possible within cluster dependence in the observations by adding a separate and random country specific error term in the linear predictor. In simple terms, this implies that the within cluster dependencies in the data are accounted for at the estimation stage, rather than afterwards. This model can be written as

$$\begin{aligned} \omega^*_{ijt} = \alpha + \beta_1(y_{ijt} - \bar{y}_{jt}) + \beta_2 x_{2ijt} + \beta_3 x_{3jt} + \beta_4(y_{ijt} - \bar{y}_{jt})x_{2ijt} \\ +\beta_5(y_{ijt}-\bar{y}_{jt})x_{3jt}+\beta_4 x_{2ijt}x_{3jt}+\beta_6(y_{ijt}-\bar{y}_{jt})x_{2ijt}x_{3jt}+\mathbf{W_{ijt}b}+\lambda_t+\zeta_j+\epsilon_{ij} \end{aligned} \tag{3.3}$$

where interpretation is the same as before, aside from the addition of the country random effects $\zeta_j$. These are assumed to be draws from a normal distribution, $\zeta_j \sim N(0, \psi)$, and independent and identically distributed across countries as well as independent of the covariates. The individual-level residuals $\epsilon_{ijt}$ are assumed to be independent both within and across clusters, as well as conditionally independent of both $\zeta_j$ and the individual-level covariates (Rabe-Hesketh and Skrondal, 2012). As the latter point out, a useful way of thinking about the random intercept term in this model is to view it as the combined effect of all omitted country specific covariates that cause some individuals to be more (or less) likely to have a positive outcome on the dependent variable. Once again, the main disadvantage of this type of random-effects model relative to the simpler pooled model with cluster robust errors is that it requires several additional distributional assumptions, including the requirement that both the within-cluster correlation structure and the between-cluster random effects are correctly specified. On the other hand, the mixed effects model tends to be more efficient, especially when the number of clusters is reasonably large. Moreover, since the previous methodological literature offers little practical guidance as to which of the two approaches to use, the analysis below relies on both options.

## 3.3 Descriptive patterns

Before turning to the more systematic analysis, it is useful to consider some initial but theoretically relevant descriptive patterns that emerge from the data. The aim of this section, therefore, is not so much inference as illustration. As a start, Figure 3.2 plots the macro-level correlation between regional inequality and the share of respondents in each European region that are afraid of crime, averaged across all survey years. Once again, the positive correlation between inequality and fear of crime ($r = 0.58, p < 0.01$) is not surprising (see, for example, Rueda and Stegmueller, 2016; Vauclair and Bratanova, 2017; Hummelsheim et al., 2010), but nonetheless remains quite notable. Although there are a handful of notable outliers, the general pattern is clear: on average, the share of respondents who are afraid of crime increases with inequality, in line with expectations. I return to analyse this relationship more systematically in Section 3.4.1 below.

Moving on to preferences, Figure 3.3 shows the share of respondents in each country that either 'agree' or 'strongly agree' with the statement that the govern-

**Figure 3.2:** Inequality and fear of crime by region

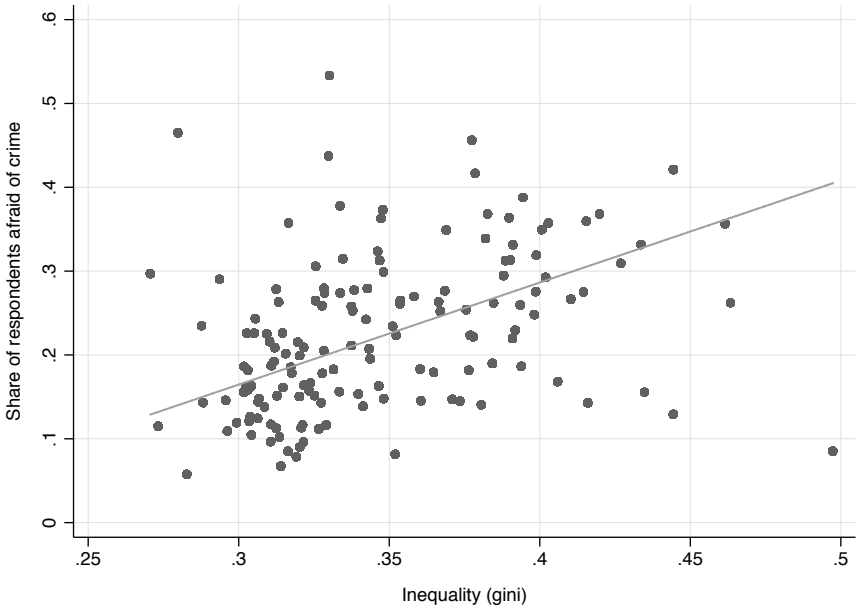
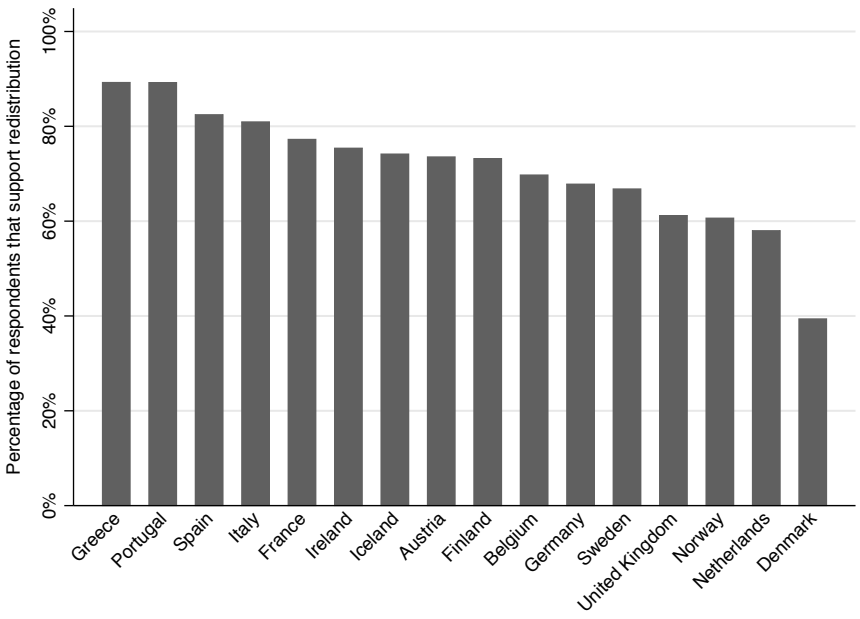


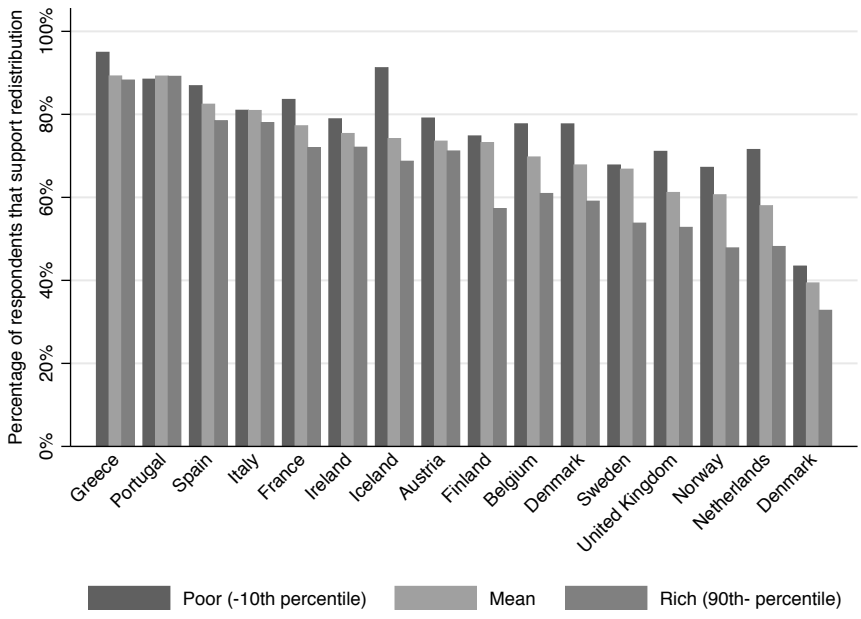

**Figure 3.3:** Average support for redistribution by country



ment should reduce differences in income levels. Although support for redistribution is remarkably widespread in Western Europe—as already noted in Table 3.1, more than 70% of respondents agree with the statement—Figure 3.3 makes it clear that this aggregate-level figure hides a considerable amount of cross-national variation: average support for redistribution varies from close to 90% of respondents in Portugal and Greece to less than 40% in Denmark. As noted above, part of this variation most likely has to do with cross-national differences in existing levels of redistribution, but still, even when accounting for such considerations the degree of between-country variation is arguably all but negligible.

Now, while the data presented in Figure 3.3 is informative, it does not say much about two issues that are central to my theoretical logic: the relationship between relative income and redistribution preferences, and the association between fear of crime on demand for redistribution. To touch on the first of these points, Figure 3.4 plots the average level of support for redistribution among the poor (respondents in or below the 10th percentile of the country-year income distribution) and the rich (90th percentile or above). Two important points should be noted about these summary statistics. The first is that in all countries

**Figure 3.4:** Average support for redistribution by income



but Portugal, individuals with low incomes are more supportive of redistribution than those with high incomes, which is perhaps no surprise. Less obviously and more interestingly, however, there is considerable cross-national variation in how far apart the redistribution preferences of the poor are from those of the rich: in some countries, support for redistribution appears to be almost to unaffected by relative income, while elsewhere the preferences of the rich and the poor are much further apart. In Austria, for example, the difference in support for redistribution between the rich and the poor is only around 7.9 percentage-points, while in the Netherlands the same difference is 23.3 percentage-points. What is more, this variation appears to be driven less by the poor and more by the rich: whereas more than two-thirds of the poor in all countries but Denmark support redistribution, the preferences of the rich exhibit much greater variation between countries.

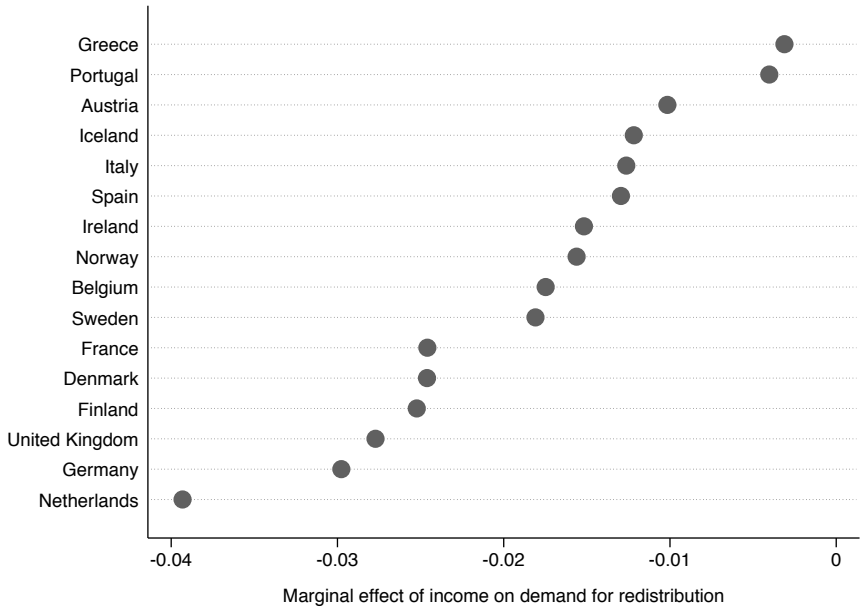
More systematically, Figure 3.5 plots the predicted average marginal effect of relative income on support for redistribution, controlling for the influence of age, gender and educational attainment. These coefficients carry a reasonably straightforward interpretation as the estimated change in the probability of supporting redistribution associated with a unit (that is, $10,000) increase in relative income, holding all variables constant at their observed values. As expected given the discussion above, the effect of income on support for redistribution is consistently negative, and remains so even when these basic demographics are controlled for. What is more striking, however, is the degree of cross-national variation in the magnitude of this effect. In Greece and Portugal, income does not seem to matter at all, while in the Netherlands, each unit increase in relative income lowers the likelihood of supporting redistribution by nearly 4 percentage-points.

Moving on, a key implication of argument proposed in Chapter 2 is that while concern about crime can give rise to increased support for redistribution, this effect is continent. It is indeed this variation that the empirical analysis developed below attempts to explore, and ultimately explain. To provide an illustration of this point, however, Figure 3.6 plots the difference (in percentage-points) between the share of respondents who are concerned about crime that supports redistribution, and the share of respondents who are not concerned about crime that supports redistribution. Higher values thus indicate a greater difference in demand for redistribution between those who are afraid of crime and those who are not afraid, and positive values mean that the former are comparatively more in favour of redistribution than the latter. Again, two points are noteworthy.

First, and as expected, in all countries in the sample but Greece, support for redistribution is higher among those who feel unsafe than it is among those who feel safe. Second, and importantly, the relationship between fear of crime and demand for redistribution is all but constant across countries: in Germany and the Netherlands, for instance, the difference in support for redistribution between the two groups is close to nine percentage-points, compared to 1.2 percentage-points in Portugal and negative 0.8 points in Greece. These numbers do little to explain this variation, but they show that the relationship between fear of crime and redistribution preferences is far from homogeneous.

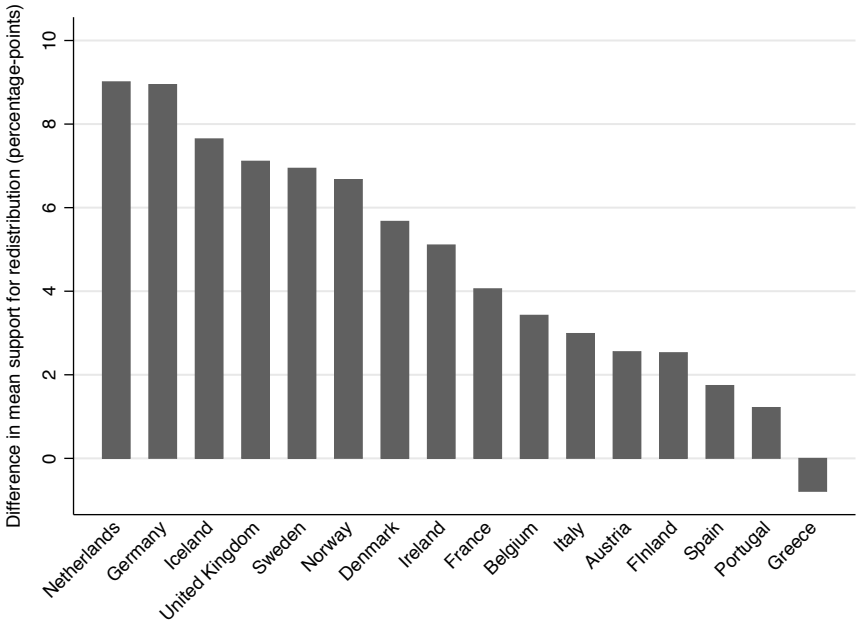
Turning to punitive attitudes, Figure 3.7 shows the share of respondents in each country that either 'agree' or 'strongly agree' with the statement that 'people who break the law should be given much harsher sentences than they are these days'. Once again, support for harsher sentences is remarkably high in this

**Figure 3.5:** Marginal effect of income on redistribution preferences




*Note:* Average marginal effects of relative income on support for redistribution, estimated from a logit model with individual-level controls for age, gender and education. In this context, these effects can be interpreted as the predicted average change in the probability of supporting redistribution associated with a $10,000 (PPP-adjusted) increase in relative income.

**Figure 3.6:** Fear of crime and support for redistribution




sample: as shown Table 3.2, on average close to 70% of respondents think that punishment should be made 'harsher'. It is important to emphasise, however, that although this degree of punitiveness may seem high, it is generally consistent with estimates from the previous literature, including those based on questions with alternative wordings, or those derived from surveys using different sampling methodologies. Allen et al. (2006), for example, use data from the British Crime Survey (now, the Crime Survey for England and Wales) to find that 76% of respondents thought that current sentencing was too lenient. Outside of Europe, both Pastore and Maguire (2005) and Applegate and Sanborn (2011) report similar figures for the U.S. in the last decade, and Roberts and Indermaur (2007) do the same for Australia. Still, the aggregate-level figures again brush over a substantial amount of variation between countries: the share of respondents that support harsher sentences varies from around 80% in Spain, Ireland, the UK, and Portugal, to about 55% in Sweden and less than 50% in Denmark. There also appears to be some regional clustering, with relatively low levels of punitiveness in the Scandinavian countries, and higher levels in Southern Europe.

Finally, it is useful to briefly look at the relationship between fear of crime

**Figure 3.7:** Average support for harsher punishment by country

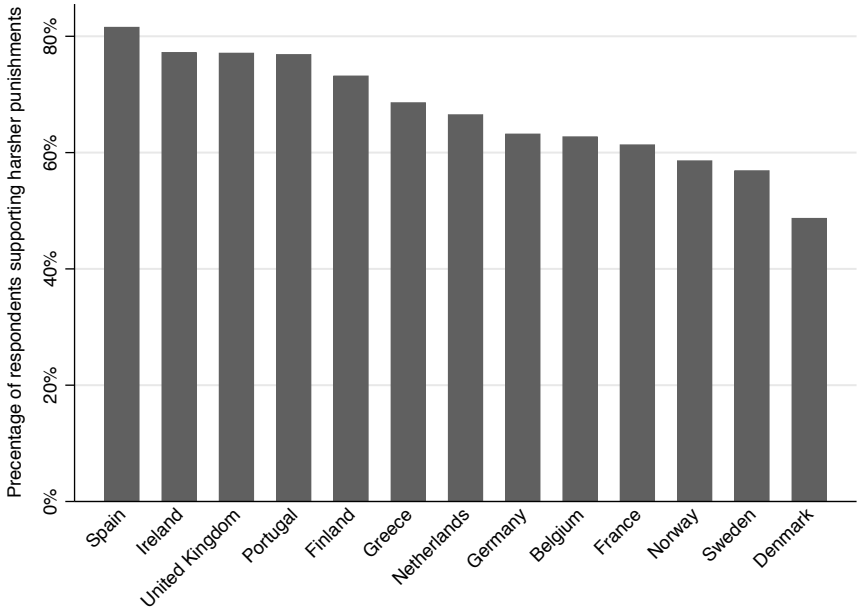
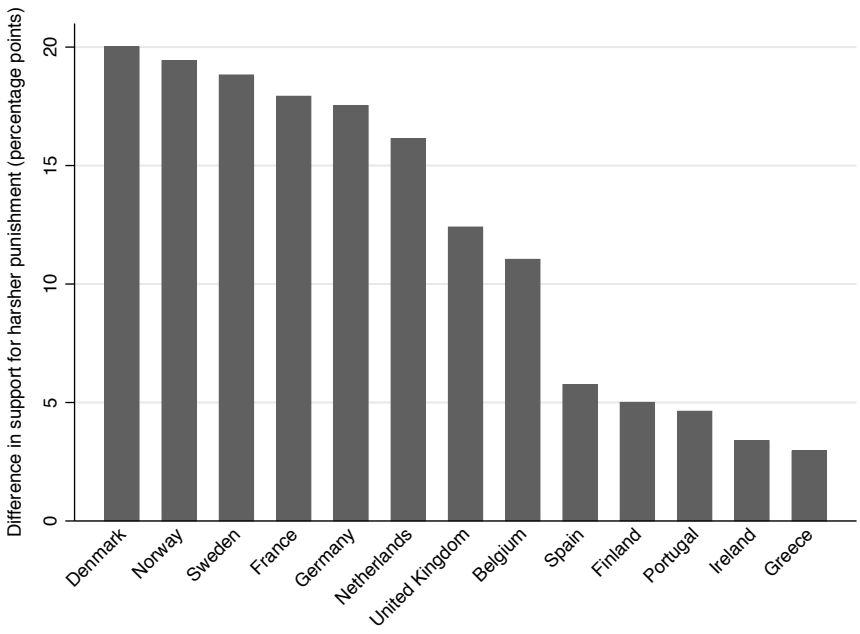



**Figure 3.8:** Fear of crime and support for harsher punishment



and punitiveness. As with the effect of fear of crime on redistribution preferences, the theoretical model developed in Chapter 2 implies that although there should be a positive relationship between fear of crime and punitivity, the magnitude of this effect will not be constant across contexts. In other words, although I expect those who are afraid of crime to be more supportive of harsher sentences than those who feel safe, I expect the difference in punitivity between the two groups to be small in some countries but larger in others. To that end, Figure 3.8 plots the difference (in percentage-points) in the proportion of respondents that support harsher punishment between those who are are afraid and those who are not afraid of crime. As such, Figure 3.8 can be interpreted in a similar way to Figure 3.6, with higher values indicating a greater difference in punitive attitudes conditional on fear of crime (with those who are afraid being comparatively more punitive), and smaller values suggesting that the punitive preferences of the two groups are more similar. Although descriptive, the data presented in Figure 3.8 shows that the relationship between fear of crime and support for punitive criminal justice policy is far from ubiquitous. The difference between the two groups range from just short of 20 percentage-points in the Scandinavian countries, to less than 5 percentage-points in Greece, Ireland, and Portugal. Over the pages that follow, I explain these differences in a more systematic way.

## 3.4 Results

Although the descriptive patterns discussed above are illustrative, they contribute little in terms of inference. To that end, the remainder of this chapter develops and presents a more formal and systematic empirical analysis. In the next section, I begin by briefly examining the relationship between regional inequality and fear of crime. As expected, I find that respondents living in more unequal regions are more likely to be afraid of crime. With this result established, the subsequent sections turn to the effect of fear of crime on preferences. To that end, I start by considering the relationship between fear of crime, population heterogeneity, and demand for redistribution. The following section, in turn, proceeds to assess the importance of these variables for punitive attitudes. Using a variety of statistical techniques and modelling approaches, the empirical findings lend strong support to both sides of the theoretical argument. Fear of crime is associated with a substantially large and statistically significant positive effect on

demand for redistribution, but only in contexts where the poor are relatively homogenous. Where first- and second-generation immigrants comprise a greater share of those with incomes below the mean, in contrast, fear of crime has little or no effect on redistribution preferences. As expected, the opposite relationship holds true for punitive attitudes: in heterogeneous settings, high-income individuals who are afraid of crime tend to be more punitive, but this effect diminishes and eventually disappears as the level of heterogeneity among the poor declines. These findings are highly robust, and provide solid initial evidence in favour of the argument and expectations presented in Chapter 2.

### 3.4.1 Inequality and fear of crime

In Chapter 2, I have argued that the importance of fear of crime to comparative political economy stems from its relationship to inequality. Although this contention is by no means new—either empirically or theoretically—assessing whether the expected positive relationship between inequality and fear of crime is borne out in the dataset used here nonetheless forms a useful starting point for the analysis. To that end, Table 3.3 presents the first set of regression results. As discussed above, the dependent variable in these models is a binary variable measuring fear of crime, based on the ESS item asking respondents how safe they feel 'walking alone in this area after dark'. Model 1 is a baseline specification, including the level regional inequality as the only independent variable. Model 2 in turn adds a battery of individual-level controls (age, gender, education, marital status, unemployment, an indicator of whether the respondent is in the labour force, household size, relative income, and actual crime victimisation). Model 3 again includes the full set of individual-level controls together with measures of regional GDP per capita, population heterogeneity, and the regional unemployment rate. Model 4, finally, has the same specification as Model 3 but adds an interaction term between relative income and inequality. All models are mixed-effects logistic regressions with regional random-effects and year fixed-effects.

Turning to these estimates, the main results in Table 3.3 largely confirm what we perhaps already knew. In line with previous work on the so-called vulnerability hypothesis—as the argument that 'socio-demographic factors such as age, gender, ethnicity, and physical disability are predictors of fear of crime since people who belong to these social groups feel physically or socially vulnerable' and therefore

**Table 3.3:** Determinants of fear of crime

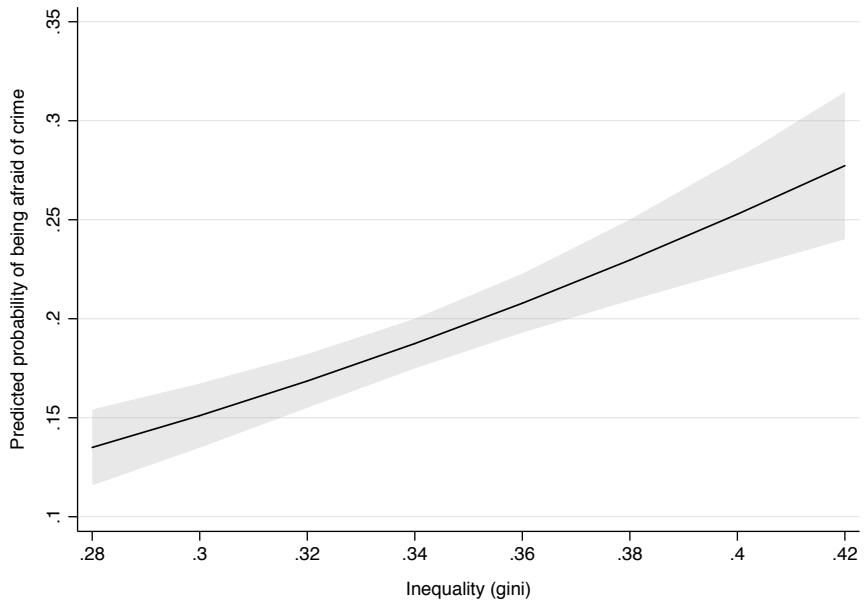
| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| Inequality | 4.855*** | 3.962*** | 7.091*** | 7.249*** |
| | (0.956) | (1.072) | (1.244) | (1.247) |
| Relative income | – | -0.077*** | -0.077*** | -0.191*** |
| | | (0.004) | (0.005) | (0.034) |
| Inequality × Relative income | – | – | – | 0.322*** |
| | | | | (0.094) |
| Age | – | 0.009*** | 0.009*** | 0.009*** |
| | | (0.001) | (0.001) | (0.001) |
| Gender | – | 1.202*** | 1.218*** | 1.217*** |
| | | (0.016) | (0.019) | (0.019) |
| Education | – | -0.534*** | -0.555*** | -0.551*** |
| | | (0.031) | (0.037) | (0.037) |
| Married | – | -0.081*** | -0.093*** | -0.092*** |
| | | (0.017) | (0.020) | (0.020) |
| Unemployed | – | 0.213*** | 0.186*** | 0.185*** |
| | | (0.036) | (0.044) | (0.044) |
| In labour force | – | -0.391*** | -0.420*** | -0.418*** |
| | | (0.018) | (0.021) | (0.021) |
| Household size | – | -0.008 | -0.011 | -0.009 |
| | | (0.007) | (0.009) | (0.009) |
| Crime victim | – | 0.648*** | 0.650*** | 0.650*** |
| | | (0.018) | (0.022) | (0.022) |
| Heterogeneity | – | – | 0.003 | 0.003 |
| | | | (0.003) | (0.003) |
| Regional GDP per capita | – | – | 0.006 | 0.006 |
| | | | (0.004) | (0.004) |
| Regional unemployment rate | – | – | -0.006* | -0.006* |
| | | | (0.003) | (0.003) |
| Constant | -2.932*** | -3.177*** | -4.366*** | -4.441*** |
| | (0.334) | (0.378) | (0.480) | (0.481) |
| BIC | 176,020 | 114,496 | 81,967 | 81967 |
| Log-likelihood | -87,956 | -57,142 | -40,863 | -40858 |
| Observations | 174,542 | 128,423 | 93,507 | 93,507 |
| Regions | 149 | 149 | 116 | 116 |

Numbers are coefficients. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

'at a higher risk of victimisation' (Vauclair and Bratanova, 2017, pp. 222)—age, unemployment, gender, not being married and not being in the labour force are significant predictors of fear of crime. Consistent with this logic, both educational attainment and relative income also have highly significant negative effects.

More importantly for the present purposes, however, regional inequality is estimated to have a consistently significant and substantively large positive effect on fear of crime, in agreement with the reasoning proposed in Chapter 2. To gain a more intuitive understanding of this result, Figure 3.9 plots the average predicted probability of being afraid of crime at different levels of regional inequality, calculated using the results from Model 3. Unlike the more common conditional predicted probabilities (which are derived by holding the remaining covariates in the model at some specified value, typically the mean for continuous variables, and the mode for categorical variables), average predicted probabilities are calculated by setting the variable of interest to some given value or range of values, while allowing all other covariates in the model to assume all of their observed values. Separate probabilities are then calculated for each observation, which are averaged to produce a single predicted probability.

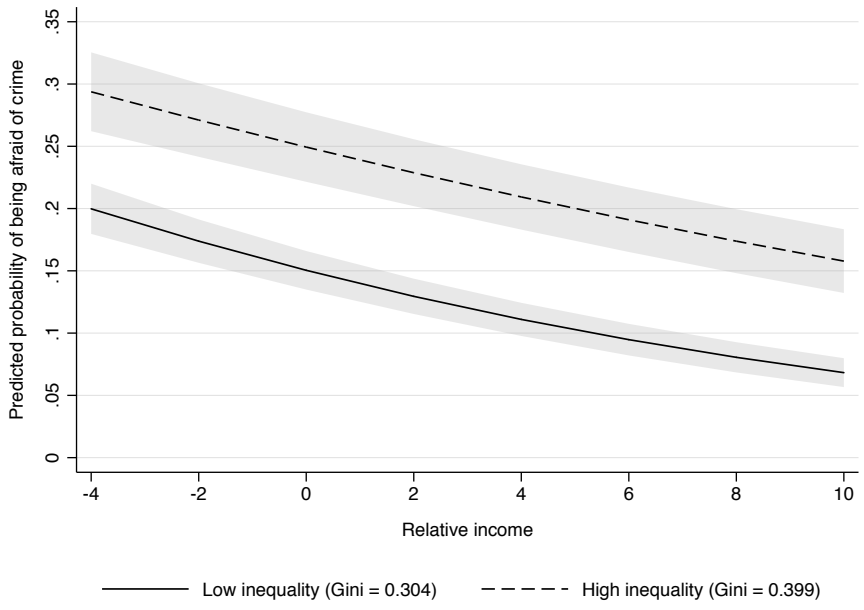
**Figure 3.9:** Predicted probability of being afraid of crime by inequality



The resulting average predicted probabilities shown in Figure 3.9 make it clear that the effect of inequality on fear of crime is far from marginal: as the level of inequality increases, so does the likelihood of being afraid of crime. The magnitude of this effects is furthermore quite large. At the 10th percentile in the distribution of regional inequality (corresponding to a gini coefficient of 0.304, roughly the level observed in the German region of Thuringen) the predicted probability of being afraid of crime is around 15.5%. In a region with an average level of inequality—Stockholm, for example, with a gini of 0.344—it is 19.1%, and in a high inequality region—such as London, with a gini of 0.433—it is no less than 29.5%. Overall, these findings thus lend considerable support for the background expectation—Premise 1—that inequality and fear of crime are positively related, in line with the results from previous studies relying on similar datasets (see especially Rueda and Stegmueller, 2016; Vauclair and Bratanova, 2017).

Before moving on to consider the relationship between fear of crime and preferences, a few words, finally, should be said about the direct effect of relative income on fear of crime. As shown in Table 3.3, the likelihood of being afraid of crime significantly declines as relative income increases. From the perspective

**Figure 3.10:** Predicted probability of being afraid of crime by relative income



of my argument, this is not an issue. What would be problematic, however, is if inequality affects fear of crime among the poor but not among those with higher incomes, since I have argued that fear of crime is a mechanism linking inequality to the preferences of the relatively affluent. To evaluate this possibility, Model 4 in Table 3.3 includes an interaction term between relative income and inequality. The results confirm that this concern is unwarranted. First, the interaction between relative income and inequality is both statistically significant and positive, implying that the effect of inequality increases rather than decreases with income. More importantly, however, the magnitude of this effect remains relatively small. To illustrate this point, Figure 3.10 plots the average predicted probability of being afraid of crime at different levels of heterogeneity, when the level of inequality is low (gini = 0.304, corresponding to the 10th percentile) and high (gini = 0.399, 90th percentile). Looking at these probabilities, it is clear that the differential effect of inequality on fear of crime conditional on relative income is substantively small, especially compared to the direct effect of either variable.

### 3.4.2 Demand for redistribution

Turning to redistribution preferences, Table 3.4 reports the first set of regression results. As discussed above, the dependent variable in these models measures whether respondents either 'agree' or 'strongly' agree with the statement that 'the government should reduce differences in income levels'. Model 1 and Model 2 are standard logistic regression models using cluster robust standard errors (on the country-level). Model 3 and Model 4 are mixed-effects logistic regressions with country-level random effects. All models include year fixed effects. Model 1 and Model 3 are baseline specifications with no individual-level control variables, whereas Model 2 and Model 4 include the controls for age, gender, education, religiosity, and marital status. In all regressions, population heterogeneity is measured using the survey-based variable, and thus gives the percentage of first- and second-generation immigrants in the lower half of the income distribution. To save space, finally, Table 3.4 reports estimates for the substantive variables only, while the full set of results are included in Table A.1 in Appendix A.

Several interesting points emerge from these results. First, a number of the control variables have statistically significant effects on redistribution preferences: age and being female are positively related to demand for redistribution (women

**Table 3.4:** Determinants of redistribution preferences

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| Relative income | -0.103*** | -0.082** | -0.106*** | -0.088*** |
| | (0.037) | (0.032) | (0.007) | (0.008) |
| Fear of crime | 0.169 | 0.066 | 0.252*** | 0.152*** |
| | (0.128) | (0.135) | (0.048) | (0.050) |
| Heterogeneity | 0.004 | 0.006 | 0.005 | 0.011*** |
| | (0.021) | (0.021) | (0.004) | (0.004) |
| Fear of crime × Relative income | 0.093*** | 0.097*** | 0.092*** | 0.097*** |
| | (0.032) | (0.032) | (0.019) | (0.020) |
| Relative income × Heterogeneity | -0.002 | -0.002 | -0.002*** | -0.002*** |
| | (0.003) | (0.003) | (0.001) | (0.001) |
| Fear of crime × Heterogeneity | 0.001 | 0.001 | -0.004 | -0.004 |
| | (0.009) | (0.010) | (0.004) | (0.004) |
| Fear of crime × Relative income × Heterogeneity | -0.006** | -0.007** | -0.007*** | -0.008*** |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| Constant | 1.915*** | 0.979*** | 1.671*** | 0.610*** |
| | (0.338) | (0.375) | (0.126) | (0.135) |
| Controls | No | Yes | No | Yes |
| Year fixed-effects | Yes | Yes | Yes | Yes |
| Country random-effects | No | No | Yes | Yes |
| BIC | 178,313 | 168,193 | 173,640 | 163,809 |
| Log-likelihood | -89,067 | -84,007 | -86,725 | -81,774 |
| Observations | 152,440 | 145,196 | 152,440 | 145,196 |
| Countries | 16 | 16 | 16 | 16 |

Numbers are coefficients. Standard errors in parentheses.
Two-tailed significance tests: *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Full set of estimates included in Table A.1 in Appendix A.

are around five percentage-points more likely than men to support redistribution), while education is associated with lower demand for redistribution (having a higher education degree lowers the probability of support by approximately seven percentage-points). These results are largely consistent with previous findings. Religiosity, on the other hand, has a significant negative effect only in the multi-level specification, but is insignificant in the pooled model. More interestingly, the effect of the existing level of redistribution is both consistently negative and highly significant. The magnitude of this effect is furthermore reasonably large: everything else equal, moving from a country-year in the 25th percentile of the distribution of social expenditure to one in the 75th percentile is associated with an approximately 6.2 percentage-point decline in the probability of supporting redistribution. This suggests that responses should be understood contextually.

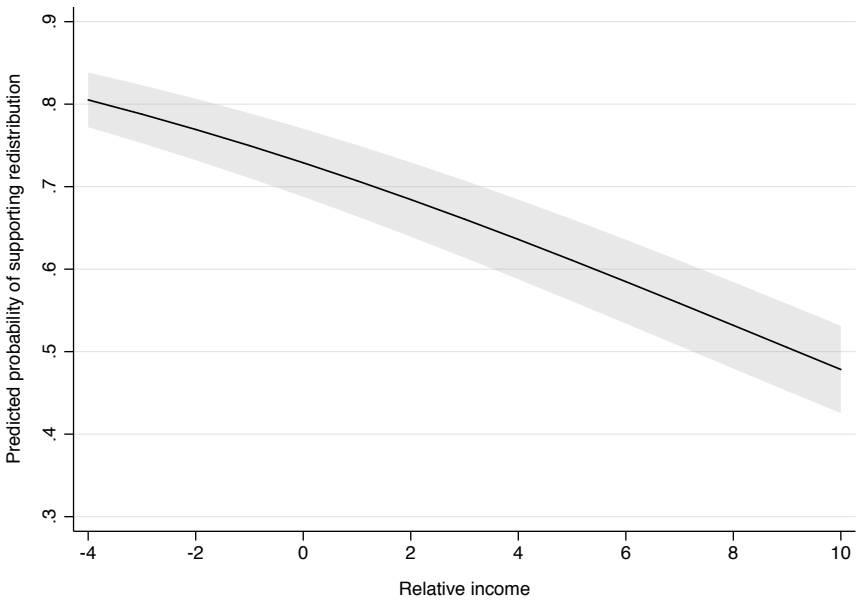
Moving on to more substantive ground, Premise 2 above stated that support for redistribution should decline with relative income: the higher an individual's income is relative to the mean, the less likely he or she is to support redistribution. Conversely, the poorer an individual is relative to the mean, the more likely he or she is to support redistribution. Going back to the theoretical argument, this expectation follows from the fact that relative income captures the impact of short-term material self-interest—in other words, post tax-and-transfer income maximisation—on redistribution preferences: to the extent that redistribution, if imperfectly, works around the mean of the income distribution, then it will provide a net benefit to those below the mean, but form a net cost for those above the mean. Hence, if material self-interest matters for redistribution preferences, support for redistribution should decline as relative income increases, controlling for other relevant factors—including externality effects and fear of crime.

The findings reported in Table 3.4 lend considerable support to this expectation. Across all four models, the estimated effect of relative income on redistribution preferences is both negative—implying that support for redistribution declines as relative income increases—and statistically significant at the 99% confidence-level. Since logistic coefficients are necessarily an intuitive metric, however, Figure 3.11 plots the average predicted probability of supporting redistribution at different levels of relative income, together with 95% confidence intervals for the predictions. These estimates are based on the results from Model 4, which includes country-level random effects and the full battery of individual-level controls, and is the best fitting model of those reported in Table 3.4.

The predicted probabilities in Figure 3.11 show that relative income has an all but negligible effect on the probability of supporting redistribution: as relative income increases, the likelihood of supporting redistribution steadily diminishes. This result is unsurprising given the demand side logic of the Meltzer-Richard model, and is very much in line with the implications of the argument developed in Chapter 2. At the 5th percentile of the sample relative-income distribution (corresponding to an annual income of approximately $28,000 below the country-year mean), the likelihood that an individual supports redistribution is just above 78.4%. For individuals with mean income, the same probability is 72.9%. Above the mean, the likelihood of supporting redistribution then falls dramatically, especially in the top decile: at the 90th percentile (at $27,300 above the mean) it is 66.7%, at the 95th percentile ($45,600 above the mean) it is 62.2%, and at the 99th percentile ($77,000 above the mean) it is 53.9%. As such, Premise 2 receives clear initial support from the analysis developed in this chapter.

These results thus suggest that a simple tax-and-transfer logic can explain a great deal of the observed individual-level variation in demand for redistribution. The richer an individual is relative to the mean, the more unlikely it is that

**Figure 3.11:** Predicted probability of supporting redistribution



he or she supports redistribution. Conversely, the further below the mean an individual is, the more likely it is that he or she does so. If the Meltzer-Richard account of redistribution preferences is correct, this is the outcome to be expected. Despite this, however, there are good reasons for thinking that tax-and-transfer considerations alone do not account for all the observed systematic variation in demand for redistribution, either cross-nationally or within countries. As illustrated in Figure 3.4, the difference in support for redistribution between the poor and the affluent is large in some countries but much much smaller elsewhere, and as shown more systematically in Figure 3.5, relative income therefore matters more in some places but less in others. As indicated by the predicted probabilities in Figure 3.11, finally, even when ignoring these differences, it is still the case that demand for redistribution from the affluent far from zero, even at the very top of the income distribution. If simple material self-interest and disposable income maximisation is all that matters, these observations are hard to explain.

In Chapter 2, I argued that an important part of the explanation for these patterns—about support for redistribution from the affluent, and variation in the extent to which this is the case—has to do with the positive relationship between fear of crime and redistribution preferences, first, and the conditionality of this relationship on the level of heterogeneity among the poor, second. The pages that follow test this argument directly. This involves assessing Premise 3, as the expectation that the effect of fear of crime on redistribution preferences increases with relative income, and Hypothesis 1, stating that the magnitude of this effect declines with the level of population heterogeneity.

To that end, the regression models reported in Table 3.4 all include a three-way interaction (together with the three necessary two-way interactions, as well as the main effects of the three variables) between fear of crime, relative income, and the share of first- and second-generation immigrants in the lower half of the income distribution. Although these estimates are difficult to interpret directly, it is notable that the main three-way interaction term is both negative—as expected—and statistically significant in all four models. To better illustrate the meaning of this result, Figure 3.12 and Figure 3.13 below plot the average predicted probability of supporting redistribution conditional on fear of crime, at low and high levels of heterogeneity and with continuous variation in relative income. The probabilities in Figure 3.12 hold the share of first- and second-generation immigrants below the mean constant at 5.0% (the 10th percentile

in the sample distribution), whereas those in Figure 3.13 hold the same variable constant at 16.7% (90th percentile). For reference, the former corresponds roughly to the level observed in Denmark in 2004, while the latter is close to that of Belgium in 2010. Similarly to the average predicted probabilities shown Figure 3.11 above, those presented in Figure 3.12 and Figure 3.13 are calculated using the results from Model 4 in Table 3.4, which includes the full battery of controls. The substantive results are the same when they are excluded.

Two important findings emerge from these results. The first is that fear of crime clearly makes a difference to redistribution preferences, provided that the level of heterogeneity among the poor is sufficiently low: whereas the difference in the probability of supporting redistribution between those who feel safe and those who feel unsafe is negligible in the high-heterogeneity setting modelled in Figure 3.13, it is much larger when the level of heterogeneity is low, as shown in Figure 3.12. The second but equally important point to note is that in the low heterogeneity case, the estimated effect of fear of crime on redistribution preferences increases with relative income: although fear of crime makes little difference to the likelihood of supporting redistribution for those in the lower half of the income distribution, it matters much more for the preferences of the rich.

In substantive terms, moreover, these effects are reasonably large in magnitude. At the mean of the income distribution, the probability of supporting redistribution is around 71.2% for those who feel safe and 73.7% for those who feel unsafe when the level of heterogeneity is low, compared to 73.6% for those who feel safe and 75.0% for those who feel unsafe when the poor are more diverse. For the affluent, on the other hand, the corresponding differences are much larger: in the 95th percentile (corresponding to an income $45,600 above the mean) and with the heterogeneity variable at 5.0%, the same probability is 61.5% for those who are not afraid of crime, but 70.2% for those who are afraid. When the level of heterogeneity is high, by contrast, the probability of supporting redistribution is 61.8% for those who feel safe, but 60.7% for those who feel unsafe. Taken together, these results thus lend initial support for the argument that fear of crime has a positive effect on support for redistribution, and that this effect increases in income but is conditional on heterogeneity.

An alternative way to illustrate this result is to instead estimate the predicted probability of supporting redistribution conditional on fear of crime for the rich and the poor separately, but with continuous variation in the level of population

**Figure 3.12:** Predicted probability of supporting redistribution (low heterogeneity)

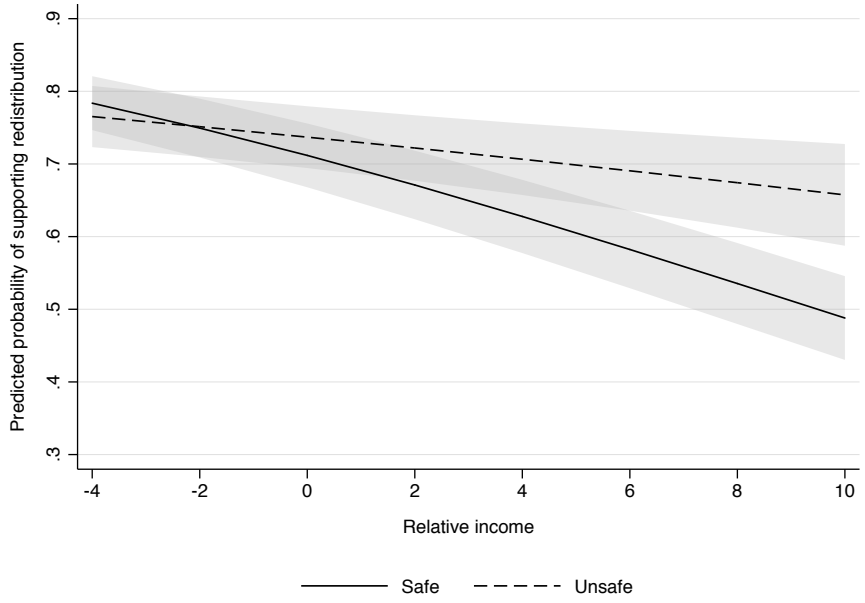
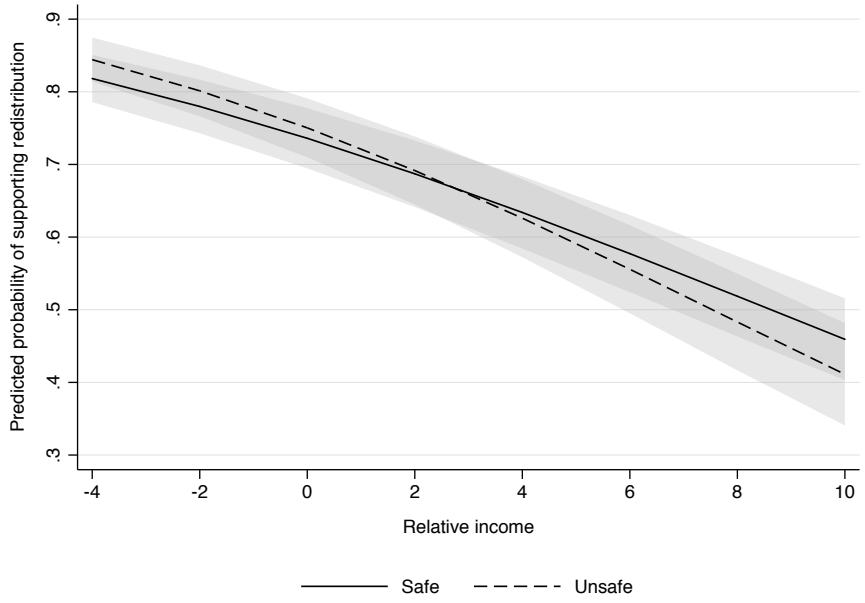



**Figure 3.13:** Predicted probability of supporting redistribution (high heterogeneity)



heterogeneity. To that end, Figure 3.14 shows the average predicted probability of supporting redistribution for an individual in the 95th percentile of the income distribution (once again, earning $45,600 more than the mean), while Figure 3.15 plots the same probabilities for an individual in the 5th percentile (making $28,000 less than the mean). As before, the solid lines give the probabilities for those who feel safe, and the dashed lines give the probabilities for those who feel unsafe. In both figures, the share of individuals with a non-native background in the lower half of the income distribution varies continuously across the $x$-axis. Thus, whereas the probabilities shown in Figure 3.12 and Figure 3.13 are derived for discrete levels of population heterogeneity but continuous variation in relative income, those presented in Figure 3.14 and Figure 3.15 are calculated by holding relative income constant while simultaneously varying the level of heterogeneity.

The predicted probabilities shown in Figure 3.14 and Figure 3.15 help to clarify the patterns discussed above. They also highlight the take-home result from a different angle: for the rich, fear of crime has a reasonably large and positive effect on the probability of supporting redistribution at low levels of population heterogeneity, but the magnitude of this effect steadily declines as the poor grow more diverse. Among those of the rich who are not afraid of crime, first, support for redistribution remains largely unaffected by heterogeneity. Among those who are afraid of crime, on the other hand, the predicted probability of supporting redistribution declines from around 73.0% at the lowest observed level of heterogeneity, to 60.7% when 16.7% of those with incomes below the mean are first- or second-generation immigrants. In stark contrast to these patterns, Figure 3.15 shows that among the poor themselves, the situation is very different. Although the less affluent—as discussed above—tend to be more supportive of redistribution compared to the rich, their redistribution preferences are never significantly affected by fear of crime at any level of heterogeneity. Taken together, these findings thus closely parallel the empirical implications of my theoretical logic, and the expectations stated in Premise 3 and Hypothesis 1 above.

Moving on, since it is not strictly correct to infer that these effects—that is, the effect of fear of crime on redistribution preferences, and the conditionality of this relationship on both the level of population heterogeneity and relative income—are statistically significant based on the non-overlapping confidence intervals of the predicted probabilities (or the opposite: that an effect is not significant because two confidence intervals overlap), I proceed to estimate correspond-

**Figure 3.14:** Predicted probability of supporting redistribution (high income)

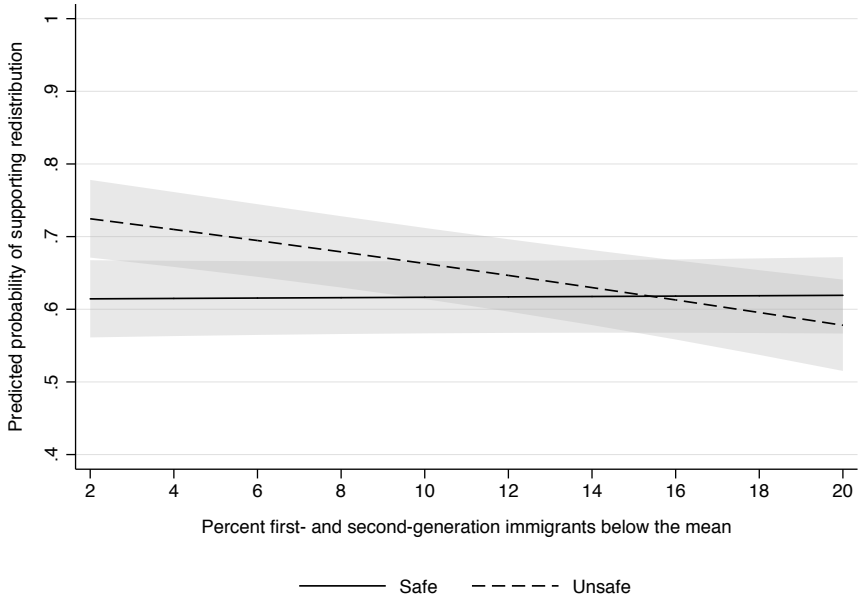
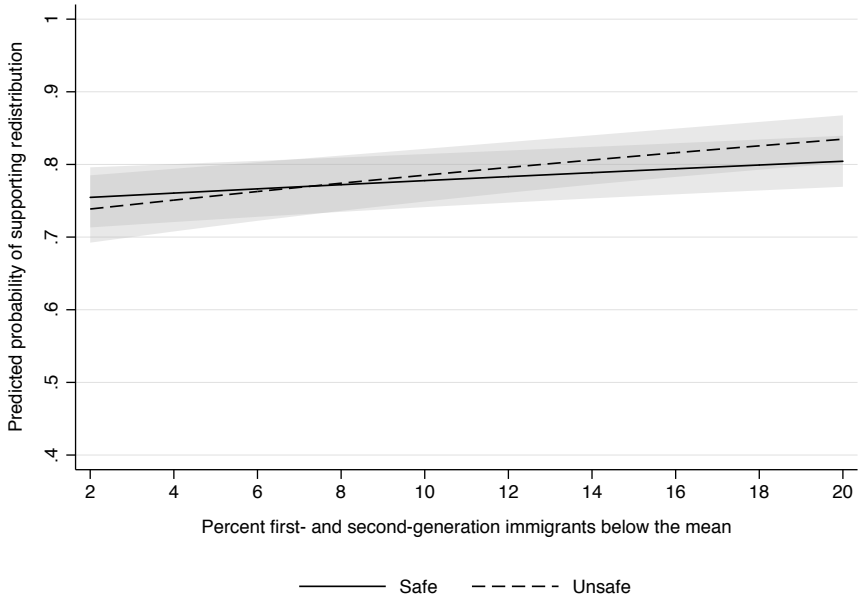



**Figure 3.15:** Predicted probability of supporting redistribution (low income)



ing series of average marginal effects. Similarly to the procedure used to derive the average predicted probabilities discussed in the previous paragraphs, average marginal effects are calculated by first estimating the marginal effect of the variable of interest for each observation while keeping all remaining variables in the model at their observed values, and then averaging those to produce single average marginal effect. Since the measure of fear of crime is dichotomous, moreover, the resulting marginal effects have a straightforward interpretation as the average percentage-point change in the likelihood of supporting redistribution associated with moving from feeling safe to feeling unsafe. Positive marginal effects thus imply that fear of crime has a positive effect on demand for redistribution, negative marginal effects suggests that it has a negative effect, and marginal effects that include zero mean that the effect of fear of crime on preferences is not significant.

To that end, Figure 3.16 plots the average marginal effect of fear of crime on support for redistribution, under low (10th percentile, 5.0% immigrants below the mean) and high (90th percentile, 16.7%) levels of heterogeneity, at different levels of relative income. These results confirm the significance of the points discussed above. First, the absolute magnitude of the effect of fear of crime on redistribution preferences significantly increases with income: put differently, fear of crime matters little to the redistribution preferences of the poor, but makes much more of a difference to the preferences of the affluent. Although the effect of fear of crime is significant and positive for those with the lowest incomes under high heterogeneity, this effect remains comparatively small. Second, the effect of fear of crime on demand for redistribution varies with the level of heterogeneity among the poor. In contexts of low heterogeneity, the magnitude of the positive relationship between fear of crime and support for redistribution increases with income, and is statistically significant for incomes just below the country-year mean and above. The size of this effect then grows significantly as income increases. At the very top of the income distribution, fear of crime increases the probability of supporting redistribution by between 8.7 (in the 95th percentile) and 13.5 percentage-points (in the 99th percentile). When the level of population heterogeneity is high, on the other hand, fear of crime has no parallel positive effect on demand for redistribution: the interaction with income is negative, and statistically insignificant for all income levels above $3,000 more than the mean.

Finally, Figure 3.17 complements Figure 3.16 by plotting the average marginal effect of fear of crime on demand for redistribution, for the afflu-

**Figure 3.16:** Marginal effect of fear of crime on redistribution preferences (by income)

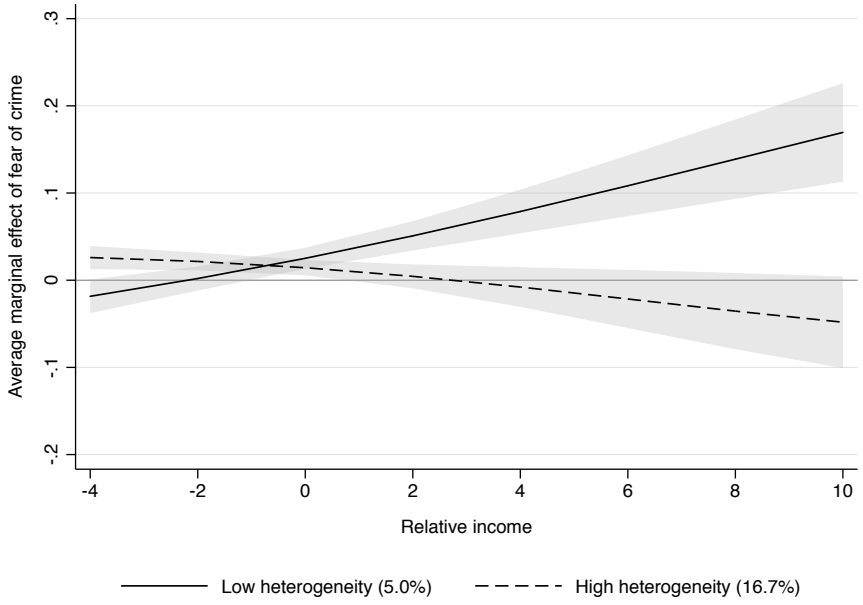
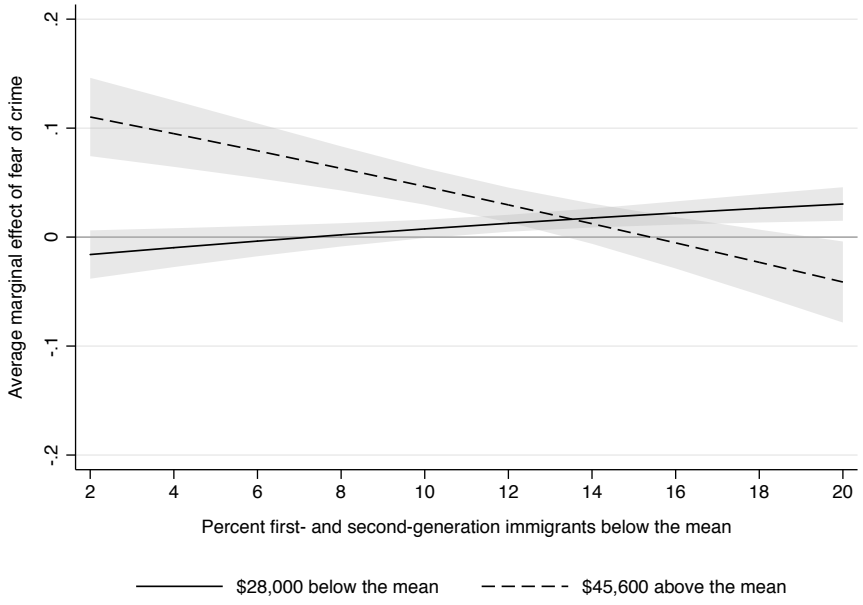



**Figure 3.17:** Marginal effect of fear of crime on redistribution preferences (by heterogeneity)



ent (95th percentile, $45,600 above the mean) and the poor (5th percentile, $28,000 below the mean) at different levels of heterogeneity. This again helps to highlight the same results from a different perspective. For the poor, the effect of fear of crime is insignificant for most levels of heterogeneity, and positive but—once again—substantively small when heterogeneity is sufficiently high. For the affluent, on the other hand, fear of crime has a large and significant positive effect when the level of heterogeneity is low, but this effect significantly diminishes as the level of heterogeneity increases. It becomes indistinguishable from zero when around 13.6% of those with incomes below the mean are have a non-native background, and both negative and significant at levels of heterogeneity above 19.8% (corresponding to the 98th percentile). This positive effect under low heterogeneity is furthermore not small: by comparison, the effect of fear of crime on the preferences of the affluent when the level of heterogeneity is at 5.0% is similar in magnitude to that associated with moving from the 95th percentile of the income distribution to the mean, when the joint effect of crime and heterogeneity is controlled for. These are very much key expectations: crime matters more for the redistribution preferences of the affluent than it does for those of the poor, but only as long as the level of heterogeneity remains low.

### 3.4.3 Demand for punishment

Taken together, therefore, the analysis in this chapter lends considerable support to the argument that fear of crime has a positive effect on support for redistribution from the rich, yet that this effect is conditional on the level of population heterogeneity in the lower half of the income distribution. To that end, neither Premise 3, as the expectation that the effect of fear of crime on preferences increases with income, nor Hypothesis 1, stating that the positive effect of fear of crime on redistribution preferences declines with the level of heterogeneity among the poor, can be dismissed based on the empirical analysis developed in this chapter. The same holds for Premise 2, about the role of relative income.

With this result in hand, the pages that follow develop an analysis similar to the one presented above, but focused on the relationship between fear of crime and punitive attitudes, rather than that between fear of crime and demand for redistribution. As discussed previously, the theoretical argument developed in Chapter 2 implies that as the level of heterogeneity among the poor increases,

the affluent become less likely to perceive of crime as a social problem related to inequality, but instead more likely to causally attribute crime it to non-economic dispositional traits. Consequently, increasing heterogeneity weakens the link between fear of crime and support for redistribution, but strengthens the extent to which it is associated with support for alternative means of addressing crime, the most prominent of which have to to with punishment and policing. If this argument is correct, therefore, fear of crime should be positively associated with support for harsher punishment when the level of population heterogeneity among the poor is high, but not when the poor are more homogeneous.

As demonstrated below, this second set of expectations also receive considerable empirical support from the analysis developed in this chapter. The effect of population heterogeneity on the relationship between fear of crime and punitive preferences is, in the important respects, the mirror image of the way in which heterogeneity affects the relationship between fear of crime and support for redistribution: in homogeneous contexts, fear of crime has no effect on the punishment preferences of the affluent, whereas in heterogeneous settings, it is associated with a statistically significant and substantively very large increase in punitivity. In line with the argument that fear of crime matters more for the preferences of the affluent than it does for the preferences of the poor, moreover, the results also indicate that the magnitude as well as the significance of this effect increases with relative income. This relationship, nevertheless, is somewhat more ambiguous than it is for redistribution preferences: in particular, fear of crime has a non-negligible positive effect on the punitivity of those with relatively low incomes, especially when the level of heterogeneity remains comparatively low. I discuss some possible explanations for this unexpected result below. In terms of the preferences of the rich, however, my central expectations are clearly borne out: crime increases support for harsher punishment only when the level of heterogeneity is high, but has no similar consequences when the poor are less diverse.

Table 3.5 presents the main set of regression results. As described above, the dependent variable used in these models measures whether respondents 'agree' or 'strongly agree' that 'criminals should be given much harsher sentences than they are these days'. Model 1 and Model 2 are standard logistic regressions estimated with cluster robust standard errors, while Model 3 and Model 4 are mixed-effects models with random country-level effects. Model 1 and Model 3 include only the three substantive variables (fear of crime, relative income, and heterogeneity)

**Table 3.5:** Determinants of punishment preferences

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| Relative income | -0.080*** | -0.060* | -0.082*** | -0.065** |
| | (0.028) | (0.035) | (0.024) | (0.028) |
| Fear of crime | 0.441 | 0.441 | 0.583*** | 0.523*** |
| | (0.311) | (0.376) | (0.134) | (0.147) |
| Heterogeneity | -0.058** | -0.037 | -0.000 | -0.026** |
| | (0.026) | (0.024) | (0.013) | (0.012) |
| Fear of crime × Relative income | -0.171*** | -0.146** | -0.181** | -0.158* |
| | (0.046) | (0.061) | (0.075) | (0.082) |
| Relative income × Heterogeneity | -0.001 | 0.001 | -0.001 | 0.001 |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| Fear of crime × Heterogeneity | 0.012 | 0.005 | 0.001 | -0.000 |
| | (0.027) | (0.032) | (0.010) | (0.011) |
| Fear of crime × Relative income × Heterogeneity | 0.015*** | 0.013*** | 0.016*** | 0.014** |
| | (0.004) | (0.005) | (0.006) | (0.006) |
| Constant | 0.113 | 0.169 | 1.576*** | 0.559* |
| | (0.393) | (0.398) | (0.561) | (0.321) |
| Controls | No | Yes | No | Yes |
| Year fixed-effects | Yes | Yes | Yes | Yes |
| Country random-effects | No | No | Yes | Yes |
| BIC | 43,984 | 39,904 | 43,598 | 39,715 |
| Log-likelihood | -21,939 | -19,890 | -21,742 | -19,769 |
| Observations | 35,654 | 33,169 | 35,654 | 33,169 |
| Countries | 13 | 13 | 13 | 13 |

Numbers are coefficients. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Full set of estimates included in Table A.2 in Appendix A.

and the macro-level control for the incarceration rate, whereas Model 2 and Model 4 add the full battery of controls. All models include year fixed effects.

A number of interesting points again emerge from these estimates. Although the separate and interactive effects of fear of crime, relative income, and population heterogeneity are difficult to decipher directly from the regression results in Table 3.5, it is notable that a number of the individual-level control variables have significant effects on respondents' punitivity, generally in line with results from the previous literature: women are more punitive than men (Hurwitz and Smithey, 1998), and those with more education (in this specification, those who have a higher education degree) are less punitive than those with less education (Dowler, 2003; Payne et al., 2004). The effect of education is particularly large, and likely explains why the effect of income drops both in magnitude and significance in Model 2 and Model 4. It is also noteworthy that the effect of the incarceration rate is consistently both significant and positive, implying that individuals tend to be more punitive in places where more people are incarcerated. Although neither the focus of this chapter nor something that can be meaningfully studied using this cross-sectional dataset alone, a possible interpretation of this is that actual criminal justice policy is responsive to popular preferences.

More directly related to my theoretical interests, the three-way interaction term between the level of heterogeneity among the poor, relative income, and fear of crime is both positive and statistically significant in all models in Table 3.5. To illustrate the meaning of this result, it is again useful to calculate a set of average predicted probabilities. To that end, Figure 3.18 plots the predicted probability of supporting harsher punishment conditional on fear of crime when 5.0% of those with incomes below the mean are first- or second-generation immigrants (10th percentile), while Figure 3.19 plots the corresponding probability when heterogeneity is held constant at 16.7% (90th percentile). In both graphs, relative income varies across the $x$-axis. Aside from the dependent variable, these plots are thus analogous to Figure 3.12 and Figure 3.13 in the previous section.

Several relevant conclusions can be drawn from these predictions. For the affluent, first, heterogeneity has a strong mediating effect on the relationship between fear of crime and punitive preferences. When the level of heterogeneity is low, as estimated in Figure 3.18, the probability that an individual earning $45,600 more than the mean is in favour of harsher punishment is nearly the same independently of fear of crime—63.5% compared to 66.2%. When the

**Figure 3.18:** Predicted probability of supporting harsher punishment (low heterogeneity)

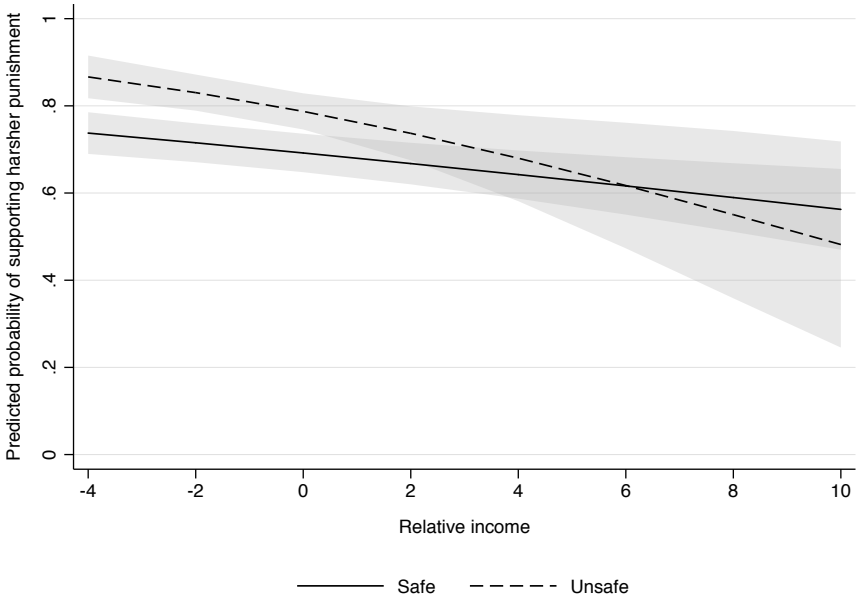
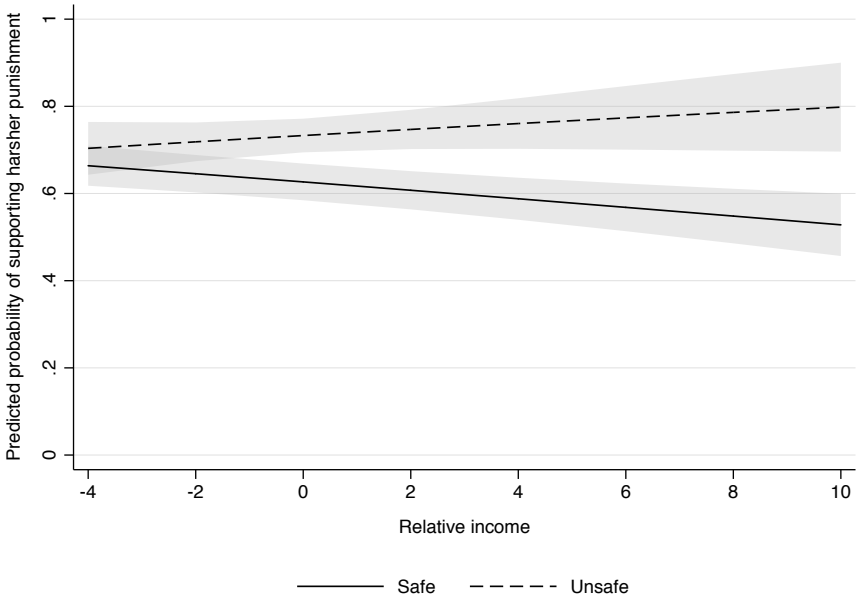



**Figure 3.19:** Predicted probability of supporting harsher punishment (high heterogeneity)



poor are more diverse, on the other hand, this is clearly not the case: at the same income level but with the heterogeneity variable held constant at 16.7%, the probability of supporting harsher punishment is 58.2% among those who are not afraid of crime, but 76.4% for those who do feel afraid. These results closely parallel the implications of the theoretical argument developed above—the punitive preferences of the affluent are positively related to fear of crime when the poor are heterogeneous (and fear of crime has no effect on redistribution preferences), but not when the poor are more homogeneous (and fear of crime does have an effect on demand for redistribution). For the poor themselves, these relationships are quite different. While fear of crime makes little difference to their punitive preferences when the level of heterogeneity is high—in the 5th income percentile, the predicted probability is 65.3% for those who are not afraid of crime, and 71.2% for those who are afraid—this is less clearly the case under low heterogeneity. This is visible in Figure 3.18: among the poor, those who are afraid of crime have a higher probability of supporting harsher punishment when the level of heterogeneity remains low. The size of this effect is smaller than that for the affluent under high heterogeneity, but it is not completely insubstantial: again in the 5th percentile, the probability of being in favour of harsher punishment is 72.4% for those not afraid of crime, but 84.6% for those who do feel afraid.

To further illustrate these results, Figure 3.20 and Figure 3.21 plot the predicted probability of supporting harsher punishment for the rich and the poor separately, at different levels of population heterogeneity. As before, the rich are defined as those with an income $45,600 greater than the mean and the poor are those with an income $28,000 below the mean. In both figures, the patterns discussed above are notable. For the rich, the relationship between fear of crime and population heterogeneity is as expected: although fear of crime does not make a difference to punitive attitudes at low levels of heterogeneity, it becomes an increasingly important (and positive) determinant of such preferences as the level of heterogeneity grows. As with demand for redistribution, for those of the affluent who are not afraid of crime, the likelihood of supporting harsher punishment is not greatly affected by the level of heterogeneity—the predicted probability declines from 64.0% when heterogeneity is at the 5th percentile, to 60.6% at the mean and 57.1% at the 95th percentile. Among those who are afraid of crime, in contrast, this probability significantly increases with heterogeneity: from 65.2% at the 5th percentile and 72.2% at the mean, to 78.4% when heterogeneity is in

**Figure 3.20:** Predicted probability of supporting harsher punishment (high income)

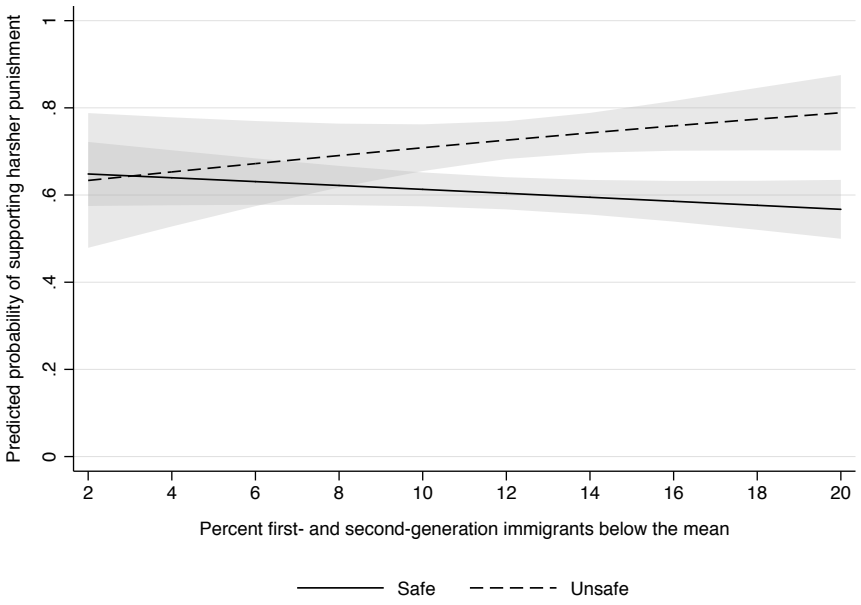
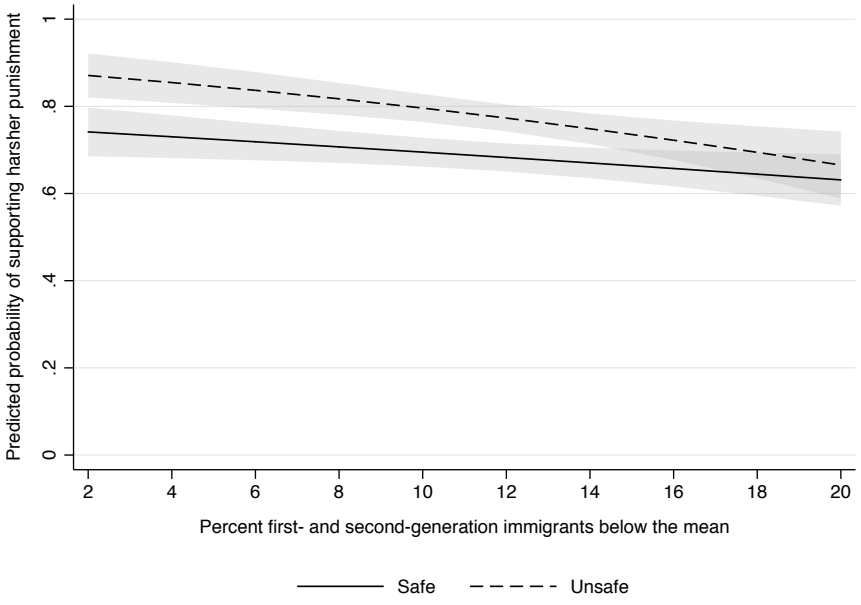



**Figure 3.21:** Predicted probability of supporting harsher punishment (low income)



the 95th percentile. For the poor, on the other hand, the relative size of the immigrant population has the opposite effect. At low levels of heterogeneity, those who are afraid of crime are significantly more likely to support harsher punishment, but this effect diminishes as the immigrant population below the mean grows.

Finally, to more systematically test whether these differences in predicted probabilities are significant, Figure 3.22 plots the average marginal effect of fear of crime on the likelihood of supporting harsher punishment at different levels of relative income, when the share of immigrants below the mean is low (10th percentile, 5.0%) and high (90th percentile, 16.7%). As before, these marginal effects can be interpreted as the change in the average predicted probability of agreeing with the statement that criminals should receive harsher punishment, associated with moving from not being afraid of crime to being afraid of crime.

The results in Figure 3.22 show that the main patterns discussed above stand up to this stricter statistical test. These results parallel those derived in the previous section: while fear of crime does have a small significant positive effect on the punitive preferences of the poor, it makes much more of a difference for those with higher incomes, but only at high levels of heterogeneity. This difference—between the effect of fear of crime on the preferences of the poor when the level of heterogeneity is low, and the effect of fear of crime on the preferences of the rich when heterogeneity is high—nonetheless remains significant, in line with my expectations. When the poor are homogeneous, the effect of fear of crime on punitive preferences declines with income, and is insignificant for incomes-levels greater than $2,200 above the mean. At the 5th percentile of the income distribution, the magnitude of this effect is approximately 12.1 percentage-points. When the poor are more diverse, on the other hand, fear of crime has a positive and statistically significant effect for everyone but those with the very lowest incomes, and the absolute size of this effect grows steadily (and significantly) as income increases. For the affluent, again, the size of this effect is all but small. At the mean of the income distribution, fear of crime increases the likelihood of supporting harsher punishment by 10.6 percentage-points. At the 90th percentile, the same effect is 15.2 percentage-points, at the 95th percentile it is 18.2 percentage-points, and at the 99th percentile it is 23.4 percentage-points.

Lastly, Figure 3.23 shows the average marginal effect of fear of crime on support for harsher punishment for the rich (using the same definition as before: 95th percentile) and the poor (5th percentile) at different levels of population

**Figure 3.22:** Marginal effect of fear of crime on punishment preferences (by income)

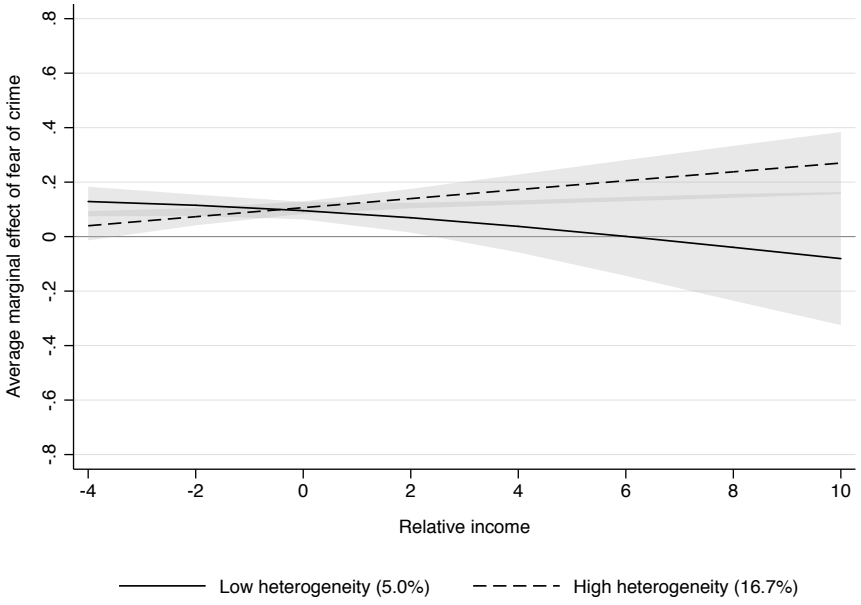
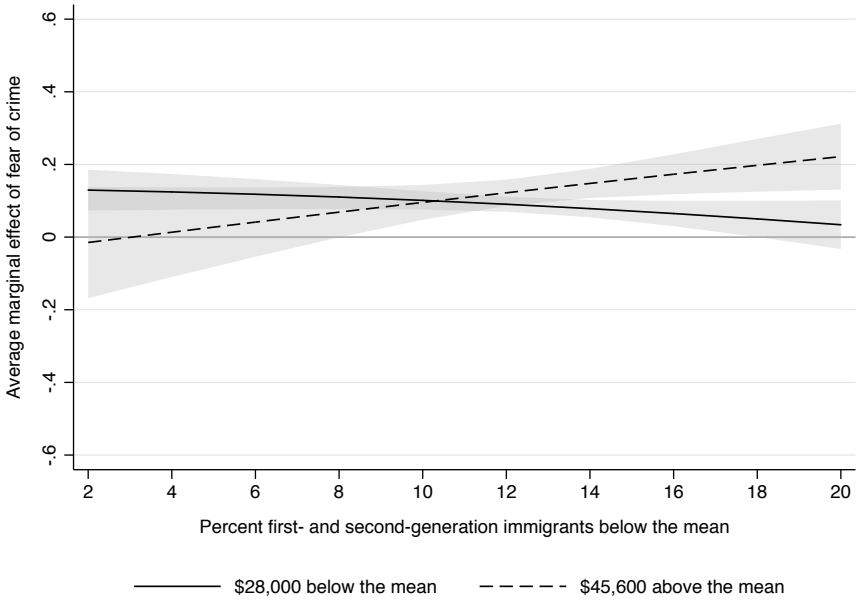



**Figure 3.23:** Marginal effect of fear of crime on punishment preferences (by heterogeneity)



heterogeneity. Among the poor, fear of crime has a significant effect on punishment preferences at all but the highest levels of heterogeneity, and the magnitude of this effect declines as heterogeneity increases: from 12.1 percentage-points at the 10th percentile (5.0% first- and second-generation immigrants below the mean), to 9.3 percentage-points at the mean (11.6%) and 6.0 percentage-points in the 90th percentile (16.7%). For the rich, on the other hand, fear of crime has a large and significant positive effect on support for harsher punishment at high levels of heterogeneity, but this effect diminishes as heterogeneity declines. It becomes insignificant when first- and second-generation immigrants make up less than 8.2% of below-average earners. As noted above, this effect is furthermore very large in magnitude, especially in the most heterogenous countries: from 11.7 percentage-points when the level of heterogeneity is at the mean, it increases to 18.2 percentage-points in the 90th percentile (as before: 16.7%), 21.3 percentage-points in the 95th, and 25.7 percentage-points in the 99th (23.1%).

When looked at together, the evidence derived in this and in the previous section thus clearly indicate that the political implications of fear of crime depend heavily on the level of heterogeneity among the poor. For those with low to average incomes, fear of crime has a substantively small but statistically significant positive effect on support for redistribution when the level of heterogeneity is sufficiently high, and a moderate positive effect on punitivity when the level of heterogeneity is low. I discuss possible explanations for these unexpected results below. For the affluent, on the other hand, these differences are much more dramatic. As the results above have shown, when the level of heterogeneity is low (again, using 5.0% immigrants below the mean as an illustration), fear of crime increases the probability that an individual in the 95th percentile supports redistribution by around 8.7 percentage-points, but has no effect on punitive attitudes. In countries where those below the mean are more diverse (16.7% immigrants), on the other hand, fear of crime has no negative effect on demand for redistribution, but increases the probability of supporting harsher punishment by more than 18.2 percentage-points. This difference is all but marginal, and casts significant doubt on the argument that the relationship between crime and redistribution is either straightforward or necessary.

## 3.5 Robustness tests

Before moving on, this last empirical section presents a series of robustness tests of the results derived over the previous pages. The first section below looks at redistribution preferences, and the second focuses on the demand for punishment. Although the findings from the main analysis provide strong empirical support for the hypotheses derived in Chapter 2, there are a number of alternative explanations and theories for these outcomes that need to be addressed before concluding. For completeness, I also present the results from models estimated with other measures of population heterogeneity, as well as using alternative statistical specifications. To simplify presentation, Table 3.6 (for redistribution preferences) and Table 3.7 (for punishment preferences) below do not present the complete regression results, but focus on the relevant average marginal effects of fear of crime: for the rich and for the poor, when the level of population heterogeneity is low and high. As before, the rich are defined as those earning $45,600 more than the mean, and the poor are defined as those earning $28,000 less than the mean. Low heterogeneity implies that 5.0% of those with incomes below the country-year mean have a non-native background, and high heterogeneity implies that this share is 16.7%. Unless otherwise noted, all results are based on the same basic random-intercept logit model used in the main analysis, and include the full battery of macro- and individual-level controls.

### 3.5.1 Redistribution preferences

The need to address the potential role of alternative explanations for the outcomes discussed above is perhaps the most obvious for the redistribution side of the analysis. On the one hand, several arguments emphasising the importance of other-regarding preferences and altruism have empirical implications that are reasonably similar to the theory tested here. On the other, the same holds true for models of redistribution preferences centred on occupational risks and skill-specificity. To that end, this section presents the results from a series of robustness tests, focusing first on the operationalisation of population heterogeneity, and second, on the possible confounding influence of considerations having to do with altruism, insurance and skill-specificity, and political ideology. Third, it shows that the main findings are robust to the inclusion of additional macro-level control variables as well as in alternative model specifications.

**Table 3.6:** Marginal effect of fear of crime on redistribution preferences

| | Rich | | Poor | |
|---|---|---|---|---|
| Heterogeneity: | Low | High | Low | High |
| Main analysis | 0.087*** | -0.012 | -0.007 | 0.024*** |
| | (0.000) | (0.377) | (0.413) | (0.000) |
| Alternative measures of heterogeneity | | | | |
| *Test 1.1:* Heterogeneity (ESS) | 0.082*** | -0.013 | -0.008 | 0.028*** |
| | (0.000) | (0.376) | (0.264) | (0.000) |
| *Test 1.2:* Heterogeneity (OECD) | 0.069*** | -0.003 | -0.012* | 0.041*** |
| | (0.000) | (0.874) | (0.079) | (0.000) |
| Additional individual-level controls | | | | |
| *Test 1.3:* Altruism | 0.084*** | -0.014 | -0.005 | 0.021*** |
| | (0.000) | (0.299) | (0.591) | (0.000) |
| *Test 1.4:* Skill specificity | 0.095*** | -0.023 | -0.010 | 0.027*** |
| | (0.000) | (0.113) | (0.246) | (0.000) |
| *Test 1.5:* Ideology | 0.081*** | 0.004 | -0.002 | 0.023*** |
| | (0.000) | (0.754) | (0.816) | (0.000) |
| Additional macro-level controls | | | | |
| *Test 1.6:* GDP per capita | 0.087*** | -0.012 | -0.007 | 0.024*** |
| | (0.000) | (0.379) | (0.412) | (0.000) |
| *Test 1.7:* Unemployment rate | 0.083*** | -0.014 | -0.008 | 0.023*** |
| | (0.000) | (0.295) | (0.303) | (0.000) |
| *Test 1.8:* Occupational unemployment rate | 0.083*** | 0.026 | 0.004 | 0.018** |
| | (0.000) | (0.139) | (0.648) | (0.050) |
| Alternative specifications | | | | |
| *Test 1.9:* Ordinal logit | 0.047*** | 0.020 | -0.027 | 0.029* |
| | (0.000) | (0.271) | (0.191) | (0.073) |
| *Test 1.10:* Only 'strongly agree' | 0.041*** | 0.019* | 0.010 | 0.038*** |
| | (0.001) | (0.069) | (0.237) | (0.000) |
| *Test 1.11:* Country fixed-effects | 0.085*** | -0.011 | -0.007 | 0.025*** |
| | (0.000) | (0.375) | (0.400) | (0.000) |

*Note:* 'Rich' defined as being $45,600 above the country-year mean, and 'poor' as being $28,000 below the country-year mean. Low heterogeneity is the 10th percentile of the relevant variable, and high heterogeneity is the 90th percentile. Numbers are average marginal effects of fear of crime, with p-values in parentheses.

**Population heterogeneity** The main analysis employs a survey-based variable to measure the level of population heterogeneity among those with relatively low incomes: this variable measures the percentage of first- and second-generation immigrants (with first-generation immigrants defined as those born abroad, and second-generation immigrants as those with two foreign-born parents) among those with incomes below the country-year mean. Since the theoretical argument developed in Chapter 2 posits that redistribution preferences will be sensitive to the identity of those most likely to engage in criminal activities—but not necessarily to the level of heterogeneity more generally—this operationalisation of heterogeneity comes with the important benefit of aligning closely with my theoretical concerns. On the other hand, a potential problem with this variable has to do with the fact that because the ESS sample size is relatively small for certain countries, the variable is sometimes sensitive to small changes in the underlying survey data.

To that end, Table 3.6 presents the results from two tests using different operationalisations of population heterogeneity. Test 1.1 substitutes the variable used in the main analysis with another survey-based variable, measuring the share of individuals with a non-native background in the population as a whole. This increases the size of both the numerator and the denominator, making the variable less sensitive to small changes in numbers. Test 1.2 moves beyond survey-measures, and uses OECD data on the share of foreign-born individuals in each country. As shown in Table 3.6, the substantive results remain largely unchanged in both specifications. In both Test 1.1 and Test 1.2, the positive effect of fear of crime on the redistribution preferences of the rich is highly statistically significant at low but not at high levels of heterogeneity. In both tests, the marginal effect of fear of crime on the preferences of the poor under high heterogeneity remains both positive and statistically significant, yet significantly smaller than the effect of fear of crime on the preferences of the affluent. The same holds true for the negative effect of fear of crime on the preferences of low-income voters in Test 1.2.

**Altruism** The possibility that other-regarding concerns matter for redistribution preferences has recently received increased attention in the comparative political economy literature. Given this, a potential criticism of my claim that the empirical patterns discussed above show that fear of crime drives support for redistribution from the affluent under low heterogeneity is that the observed outcomes are merely an expression of altruism. Arguably, this is a particularly rel-

evant concern given the possibility that other-regarding preferences are sensitive both to the level of inequality (see, for example, Dimick, Rueda, and Stegmueller, 2016), and on the level of population heterogeneity among the beneficiaries of redistribution (Rueda, 2018; Alesina and Glaeser, 2004; Luttmer, 2001). To demonstrate that the main results are not driven by such concerns, I estimate a model that controls for other-regarding preferences on the individual-level. As a proxy for altruism, I rely on an item that first asks respondents to consider the following description of a person: 'It is very important to her/him to help the people around her/him. She wants to take care for their well-being.' They are then asked to say how much this person is or is not like them, using a six-point scale ranging from 'Very much like me' to 'Not like me at all'. The results in Table 3.6 show that adding this variable to the model does not significantly affect the substantive results. Even when altruism is controlled for, the key patterns from the main analysis still hold up: the effect of fear of crime on support for redistribution from the rich remains positive and significant at the 99% confidence-level when heterogeneity is low, but insignificant when heterogeneity is high.

**Skill specificity** Moving on, an important literature in comparative political economy argues that because redistribution serves an insurance function by cushioning against potential income loss in the future, redistribution preferences are affected by exposure to labour market risks. More specifically, an influential argument in this vein emphasises that a key determinant of these risks is the specificity of a particular individual's productive skills (Iversen and Soskice, 2001; Estevez-Abe, Iversen, and Soskice, 2001). Summarised briefly, the basic intuition behind this argument is that because specific skills are not portable, unemployment is more costly to workers with specific skills than it is for those with general skills: whereas the latter are transferrable, specific skills carry little value to alternative employers. Hence, individuals with specific skills should be more supportive of redistribution (as insurance) compared to workers with general skills. To control for this influence on redistribution preferences, Test 1.4 in Table 3.6 includes the same skill specificity variable used by Iversen and Soskice (2001) and Cusack, Iversen, and Rehm (2006). Again, adding this control variable to the model does not alter the substantive results: as expected, skill specificity is associated with higher demand for redistribution, but this does not affect the importance of fear of crime, relative income, and population hetero-

geneity for redistribution preferences. In Test 1.4, the effect of fear of crime on support for redistribution from the rich remains significant only when the level of heterogeneity is low, and is marginally more pronounced than in the main analysis. As before, the effect of crime on the preferences of the poor under high heterogeneity remains significant but small in magnitude.

**Ideology** The main analysis does not include a control for left-right ideology, mainly because redistribution preferences—the dependent variable—is arguably an important part of an individual's ideological outlook. In other words, ideology is part of what it is that the analysis above attempts to explain, rather than an independent explanation for it. On the other hand, since some recent contributions in comparative political economy have argued that ideology has a separable effect on support for redistribution, Test 1.5 includes a variable that measures respondents' ideological self-placement on a ten-point left-right scale. Unsurprisingly, being more left-wing is associated with higher support for redistribution, and adding this variable to the model marginally weakens the main results. However, the central findings still stand: fear of crime has a positive and statistically significant effect on the redistribution preferences of the rich when the level of heterogeneity is low, but has no parallel implications under high heterogeneity.

**Additional macro-level controls** As mentioned above, I deliberately kept the regression models used in the main analysis parsimonious on the country-level, given concerns about degrees of freedom. As additional robustness checks, however, Test 1.6 and Test 1.7 add controls for national GDP per capita and the harmonised national unemployment rate. Both variables are taken from Eurostat. Following Rehm (2009), Test 1.8 proceeds to include a control for the occupational unemployment rate, calculated from the ESS for each survey year and measured at the ISCO-88 1-digit level. The results in Table 3.6 show that neither inclusion significantly affects the key results. For both the affluent and the poor, the results remain substantively the same as in the main analysis.

**Ordinal dependent variable** To demonstrate that the substantive results are not affected by the choice to use a binary dependent variable in the main analysis, Test 1.9 reports the results from an ordinal logic regression model estimated on the the untransformed, five-point dependent variable. In this case, the average

marginal effects can be interpreted as giving the predicted effect of fear of crime on the probability of 'strongly agreeing' with the statement that the government should reduce differences in income levels. Given this difference, the smaller magnitude of the substantive effects is not surprising (as shown in Table 3.1, around 27% of respondents 'strongly agree' with the statement, while around 43% 'agree'). Nonetheless, the key substantive results are the same as in the binary logit specification: for the affluent, fear of crime has a positive effect on demand for redistribution only when the level of population heterogeneity is low, but no parallel effect when the poor are more diverse. In this specification, moreover, fear of crime never has a significant effect on the preferences of the poor themselves.

**Only 'strongly agree' responses** In the main analysis, the dependent variable is coded as 1 if a respondent either 'agrees' or 'strongly agrees' with the statement that 'the government should reduce differences in income levels'. To check that the decision to collapse these two categories into one is substantively inconsequential, Test 1.10 presents the results from a binary logit model with a dependent variable measuring 'strongly agree' responses only. Although the magnitude of the estimated effect of fear of crime is significantly smaller than in the main analysis (as suggested above, this should not come as a surprise), the substantive results remain unchanged. The comparatively larger drop in magnitude of the effect of fear of crime for the rich relative to the poor, however, suggests that for the former, the patterns discussed in the main analysis are driven more by movements into the 'agree' rather than the 'strongly agree' category.

**Fixed-effects** Finally, Test 1.11 in Table 3.6 reports the results from a model estimated with both year and country fixed-effects. Although the model specifications used in the main analysis should be sufficient to deal with both within- and between-country unobserved confounders, adding country fixed-effects provides a useful and stringent test of the robustness of the results derived above. As discussed previously, all models in the main analysis include year fixed effects, which account for time-specific shocks affecting individuals in all countries at a given point in time. Test 1.11, in turn, adds unconditional country fixed-effects, which control for all unobserved time-invariant confounding variables on the country-level. As such, this model relies only on within-country variation in the macro-level variables, including heterogeneity and its interactions. As shown in

Table 3.6, the substantive results hold up even in this fairly stringent specification. The estimated effect of fear of crime on support for redistribution from the rich remains very similar to the results from the main model: the marginal effect is a positive and statistically significant when the level of population heterogeneity is low, but statistically insignificant when the poor are more diverse.

### 3.5.2 Punishment preferences

Turning to punitive preferences, the paragraphs below conduct a similar series of robustness tests, but focused on the effect of fear of crime, population heterogeneity, and income on support for harsher punishment, rather than redistribution. As before, I do not report full regression results, but focus on the relevant average marginal effects of fear of crime. These are reported in Table 3.7. Once again, the robustness checks show that the main results are robust both to the inclusion of additional control variables on the individual and country-level, other measures of heterogeneity, and in regression models with alternative specifications.

**Police rate** The results from the main analysis use the incarceration rate—that is, the number of incarcerated individuals per 100,000 population—to control for the existing level of punitivity. As I have mentioned before, including this control is important, not least because the dependent variable used in this part of the analysis measures responses to a survey-item that asks whether punishment should be made 'harsher', and is therefore explicitly comparative. Although the incarceration rate is arguably a better measure of existing 'harshness', Test 2.1 instead relies on a variable that records the number of police officers per capita. This substitution has no effect on the estimated effect of fear of crime, and the take-home findings still stand: for the rich, fear of crime has a large and statistically significant positive effect on support for harsher punishment when the level of heterogeneity is high, but no parallel effect when the level of heterogeneity is low. For the poor, the results also remain unchanged from the main analysis, with a positive (but significantly smaller) effect of fear of crime on punitivity independently of the level of heterogeneity.

**Population heterogeneity** Following the reasoning outlined in the previous section, Test 2.2 and Test 2.3 checks the robustness of the substantive results to alternative operationalisations of population heterogeneity. To that end, Test 2.2 re-

**Table 3.7:** Marginal effect of fear of crime on punishment preferences

| | Rich | | Poor | |
|---|---|---|---|---|
| Heterogeneity: | Low | High | Low | High |
| Main analysis | 0.028 | 0.182*** | 0.121*** | 0.060*** |
| | (0.618) | (0.000) | (0.000) | (0.003) |
| Alternative macro-level variables | | | | |
| *Test 2.1:* Police rate | -0.006 | 0.189*** | 0.129*** | 0.055*** |
| | (0.908) | (0.000) | (0.000) | (0.005) |
| *Test 2.2:* Heterogeneity (ESS) | 0.040 | 0.185*** | 0.127*** | 0.042** |
| | (0.317) | (0.000) | (0.000) | (0.048) |
| *Test 2.3:* Heterogeneity (OECD) | 0.032 | 0.197*** | 0.090*** | 0.080*** |
| | (0.392) | (0.000) | (0.000) | (0.000) |
| Additional individual-level controls | | | | |
| *Test 2.4:* Ideology | 0.006 | 0.168*** | 0.139*** | 0.060*** |
| | (0.918) | (0.000) | (0.000) | (0.002) |
| *Test 2.5:* Trust | 0.007 | 0.158*** | 0.119*** | 0.036* |
| | (0.893) | (0.000) | (0.000) | (0.073) |
| *Test 2.6:* Economic satisfaction | 0.011 | 0.179*** | 0.123*** | 0.050** |
| | (0.832) | (0.000) | (0.000) | (0.015) |
| *Test 2.7:* Unemployed | 0.010 | 0.184*** | 0.135*** | 0.052*** |
| | (0.849) | (0.000) | (0.000) | (0.009) |
| Additional macro-level controls | | | | |
| *Test 2.8:* GDP per capita | 0.010 | 0.184*** | 0.134*** | 0.052*** |
| | (0.849) | (0.000) | (0.000) | (0.009) |
| *Test 2.9:* Unemployment rate | -0.003 | 0.184*** | 0.134*** | 0.053*** |
| | (0.960) | (0.000) | (0.000) | (0.007) |
| Alternative specifications | | | | |
| *Test 2.10:* Ordinal logit | 0.007 | 0.151*** | 0.118*** | 0.055** |
| | (0.888) | (0.000) | (0.000) | (0.030) |
| *Test 2.11:* Only 'strongly agree' | 0.071 | 0.105*** | 0.099*** | 0.057*** |
| | (0.119) | (0.001) | (0.000) | (0.002) |

*Note:* 'Rich' defined as being $45,600 above the country-year mean, and 'poor' as being $28,000 below the country-year mean. Low heterogeneity is the 10th percentile of the relevant variable, and high heterogeneity is the 90th percentile. Numbers are average marginal effects of fear of crime, with p-values in parentheses.

places the variable used in the main analysis—which, again, measures the percentage of first- and second-generation immigrants below the mean—with an alternative survey-based variable that records the share of individuals with a non-native background in the population as a whole. Test 2.3, in turn, estimates the same model but using data on stocks of foreign-born individuals from the OECD (as a percentage of the population). Neither of these substitutions alter the central results: in both tests, fear of crime has a significant effect on support for punishment among the rich only when heterogeneity is high, but has no similar effect when the poor are less diverse. For the poor themselves, the results are also unchanged.

**Ideology** Test 2.4 includes a control for political ideology, using the same variable described in the previous section. As in the analysis of redistribution preferences, this variable is not part of the main analysis since it is plausible to suppose that an individual's punitive attitudes form part of his or her political ideology, especially for conservatives—this view is reflected in the common statement that 'a liberal is a conservative who has been mugged'. Nevertheless, since ideology has features fairly prominently in the comparative literature on punitive attitudes, it is included as an additional robustness test of the main results. As indicated in Table 3.7, controlling for political ideology does not alter the substantive findings from the main analysis: for the rich, fear of crime remains strongly associated to punitiveness when the poor are heterogeneous but not when they are less diverse. For the poor, fear of crime again has a positive effect on support for harsher punishment at both low and high levels of heterogeneity, and the magnitude of this effect is greater in the former case than in the latter.

**Trust** As discussed in Chapter 2, an important literature highlights expressive concerns and anxieties as key to understanding punitive attitudes, rather that instrumental factors more closely related to crime itself. An important variable in this literature is social trust, which is hypothesised to be negatively related to punitive preferences. To control for these explanations, Test 2.5 adds a measure of social trust to the model included in the main analysis. Trust is measured using a standard ESS item, which asks respondents 'generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people?', and measures responses on an 11-point scale with higher responses indicating greater trust. Including this variable in the model marginally weakens

the magnitude of the main results, but does not alter the substantive findings: for both the rich and the poor, the important results remain the same.

**Macroscopic economic evaluations** Following a similar reasoning, Test 2.6 includes a control for macroscopic economic evaluations. As with social trust, dissatisfaction with the state of the economy has been liked to support for punitive criminal justice policy in arguments emphasising the expressive rather than instrumental roots of punitiveness (Costelloe, Chiricos, and Gertz, 2009). To measure economic evaluations, I use a survey item asking 'on the whole how satisfied are you with the present state of the economy in [country]?', and again recored responses on the same 11-point scale ranging from 'extremely dissatisfied' to 'extremely satisfied'. As Table 3.7 shows, including this variable does not affect the main results. The effect of fear of crime on the punishment preferences of the rich remain significant under high but not low heterogeneity, while the effect on the preferences of the poor is always significant. As before, however, this effect is significantly larger in magnitude for the former than the latter.

**Unemployment** The last test of expressive explanations for punitiveness involves including a variable for unemployment. Arguments in this vein suggest that economic hardship on the individual-level should be positively correlated with punitive preferences Hogan, Chiricos, and Gertz (2005, for example). To control for this effect, Test 2.7 adds a simple indicator for unemployment (that is, a measure of whether the respondent is unemployed) to the model used in the main analysis. Doing so makes little difference, and the results of the model with unemployment are nearly identical to those of the model without it. As such, the substantive findings still stand: for the rich, fear of crime has a positive effect on punitive preferences when the level of population heterogeneity is high but not when it is low, while for the poor the effect of fear of crime is significant (and again, smaller than for the rich) independently of the level of heterogeneity.

**Additional macro-level controls** Following the logic outlined in the previous section, Test 2.8 adds a controls for national GDP per capita. This does not change the results from the main analysis. The same conclusion holds true for Test 2.9, which includes a measure of the harmonised unemployment rate.

**Ordinal logit** Again following the reasoning outlined in the previous section, Test 2.10 reports the result from an ordinal logit regression, using the untransformed five-point dependent variable. As before, I report the estimated marginal effect on fear of crime on the probability of being in the 'strongly agree' category. The results in Table 3.7 again show that the choice to use a binary dependent variable in the main analysis is substantively inconsequential: for both the rich and the poor, the effect of fear of crime is the same as before.

**Only 'Strongly agree' responses** In the main analysis, the dependent variable indicates whether a respondent 'agrees' or 'strongly agrees' that 'people who break the law should be given much harsher sentences than they are these days'. To test whether the main results are robust to a stricter definition of punitiveness, Test 2.11 reports the results from a model using a dependent variable representing 'strongly agree' responses only, with all other answers coded as negative outcomes. Not surprisingly given the frequencies reported in Table 3.2, the magnitude of the estimated effects are smaller than when the 'strongly agree' and 'agree' responses are collapsed, but remain substantively similar. For the rich, fear of crime has a significant positive effect on punitiveness when the poor are heterogeneous, but not when they are homogenous. For the less affluent, on the other hand, fear of crime has a significant (but smaller) positive effect at both low and high levels of heterogeneity, and this effect is larger in magnitude in the former case than in the latter.

## 3.6 Summary

Taken as a whole, the results derived over the previous pages provide a number of interesting insights. For the affluent, first, the results are very much in line with my key expectations: the political consequences of fear of crime depend, above all, on the level of population heterogeneity among the poor. In homogeneous contexts, first, fear of crime has a statistically significant and substantively large effect on demand for redistribution, but no effect on support for harsher punishment. When the poor are more heterogeneous, by contrast, the opposite is true: fear of crime increases support for punitive criminal justice measures, but makes no difference to demand for redistribution. These results are congruous with both Hypothesis 1 and Hypothesis 2. The background expectations stated as Premise 1—that there exists a robust and substantively large positive

relationship between the local level of income inequality and fear of crime—and Premise 2—that support for redistribution significantly declines with relative income—are also clearly supported by these observational results.

For the less affluent, on the other hand, the results are more mixed. On the redistribution side of the analysis, my expectation—Premise 3—that the effect of fear of crime on preferences should increase with income is quite clearly borne out. Although the results show that fear of crime does have a significant positive effect on support for redistribution among the poor provided the level of heterogeneity is sufficiently high, the magnitude of this effect remains small, both in absolute or substantive terms and relative to the parallel effect observed for the affluent under conditions of low heterogeneity. The same cannot necessarily be said for the punishment side of the analysis, however. As shown perhaps most clearly in Figure 3.20 and Figure 3.21, fear of crime has a statistically significant and substantively non-negligible positive effect on support for harsher punishment among the poor, at all but the very highest observed levels of heterogeneity. Although this effect remains significantly smaller in magnitude than the corresponding, positive effect observed among the more affluent in high heterogeneity settings, it is both unexpected and somewhat puzzling.

Although I have no certain explanation for this outcome, one possibility lies in the observation that the poor are disproportionally affected by actual criminal victimisation, as discussed in Chapter 2. It is possible, for example, that victimisation by an in-group member—unlike more abstract feelings of fear of crime—triggers a preference for punishment, in line with the growing experimental literature on group-based punishment behaviour and second-order cooperation (for example, see Shinada, Yamagishi, and Ohmura 2004). Summarised briefly, a central tenet of arguments in this vein is that because benefits of cooperation tend to be shared within rather than across groups, individuals have a stronger motive to punish in-group rather than out-group members, even when punishment is costly to the punisher. If this is the case, in turn, the effect of heterogeneity on the relationship between fear of crime and support for punishment among the poor becomes more explicable. As I return to in Chapter 6, pursuing this possibility further is a natural continuation of the work developed in this dissertation.

# Chapter 4

# Crime, Redistribution and Punishment in the United States

At least since Alexis de Tocqueville's travels across the country, the notion of American exceptionalism has been widespread—taking divergent forms ranging from Reagan's 'shining city upon a hill' to Stalin's remarks about 'the heresy of American exceptionalism' after learning, in 1929, of the declaration by the American Communist Party that it was exempt from the laws of historical materialism, and hence no longer interested in revolution (McCoy, 2012; Morgan, 1999). Whatever the specifics of this idea, a very minimal definition takes it to imply that the US is different: that the history, culture or values of the US combine to make it truly unique, that it has a special role or mission in world affairs, and, perhaps—depending on who you ask—that it is therefore in some sense superior to other countries and societies. Although this is not the right place to evaluate these different claims (for interesting but contrasting assessments, however, see Lipset, 1996, and Baldwin, 2009), it is certainly true that when it comes to the issues that lie at the heart of this dissertation—inequality, redistribution, crime and punishment, heterogeneity and identity—the US appears not only to be different but also rather extreme, especially when compared to Western Europe.

A few numbers help to illustrate this point. Take inequality: an American worker in the 90th percentile earns roughly 2.3 times more, after taxes and

transfers, than someone with median income, and nearly 6.5 times more than someone in the 10th percentile (OECD, 2019a). By contrast, in an average European country—take France—the corresponding 90/50 ratio is 1.9 and the 90/10 ratio is just 3.5. In Denmark, the least unequal West European country on this metric, the 90/50 ratio is 1.6 and the 90/10 ratio is only 2.9. As is well known, the main reason for these differences is not that market inequality is necessarily so much higher in the US than it is in Europe—at least not across most of the income distribution—but rather that its tax-and-transfer system is significantly less redistributive (Piketty and Saez, 2007; Alvaredo et al., 2013; OECD, 2015). Top incomes are an exception: while the top percentile of earners in the US account for 20.2% of national income, before taxes, the corresponding share is 13.9% in the UK, 10.8% in France, and well below 10% in all of Scandinavia (WID, 2019).

Inequality aside, the US is also more diverse. Although the fact that population heterogeneity manifests itself differently on either side of the Atlantic—race being more salient in the US, while immigration is more relevant in Europe—complicates a straightforward comparison of numbers, the ethnic fractionalisation index developed by Fearon (2003) can be used to make this point: the US scores higher on this measure than all West European countries but Belgium and Spain, both of which have large but geographically concentrated minority groups, and Switzerland, which is arguably a special case. Despite the recent wave of in-migration, moreover, Alesina and Glaeser's (2004, pp. 136) comment that 'by almost any measure, most European countries, especially those that are small, are remarkably homogenous' still largely holds true. In all of Greece, Finland, Portugal and Italy, for example, more than 90% of the population is native-born (OECD, 2019b). In the US, on the other hand, 72% of the population considers itself white, 13% identify as black, and 6% as Asian. Cross-cutting race, 18% of Americans classify themselves as hispanic (ACS, 2018). These national figures furthermore hide significantly higher levels of heterogeneity within certain states (and as shown below, cities): for example, 31% of the population in Georgia is black, while 39% of people living in California identify as hispanic. The relatively recent history of institutionalised racism, segregation, and the civil rights movement—unparalleled in Western Europe—also contribute to the unique salience of race in the US, both in politics and in society at large.

When it comes to issues relating to crime and punishment, finally, the US comes even more into its own. Many of these numbers will be familiar, but they

remain quite remarkable. The US houses less than 5% of the world population, but accounts for nearly 20% of its inmates. Combining numbers from state and federal prisons as well as local jails, the current US incarceration rate is approximately 655 per 100,000 population, including children. In Western Europe, by contrast, the corresponding incarceration rate ranges from a high of 140 in England and Wales to a low of 37 in Iceland, with a median of around 81 (Walmsley, 2018). While the reasons behind this discrepancy are complex—as I show below, heterogeneity is a significant part of the explanation—it is important to emphasise that it cannot be fully accounted for by differences in the crime rate. The US does indeed have a higher incidence of homicide than any country in Western Europe—4.88 per 100,000 population, compared to the European average of 0.92 (UNDOC, 2019)—yet the rate of non-violent crime is relatively similar on either side of the Atlantic, and sometimes higher in Europe. Burglaries, for example, are less than half as common in the US as they are in Denmark or the Netherlands, and the incidence of rape in Sweden is twice that reported in the US. What makes the US different, on the other hand, is the punitiveness of its criminal justice system: both in terms of its reliance on incarceration for many non-violent (especially drug-related) offences, and, perhaps even more so, the prevalence of comparatively long prison sentences (for a discussion, see Tonry, 2000).

Disaggregated by race, moreover, these incarceration statistics become, if anything, even more striking. As mentioned above, although blacks constitute approximately 13% of the US population, they make up nearly 40% of its inmates. By contrast, although more than two-thirds of Americans are white, they represent only 39% of the prison population (Sakala, 2014). Less abstractly, these figures imply that for significant subsets of Americans, and unlike what is the case in any other advanced industrial democracy, imprisonment has become a standard life event. Pettit and Western (2004), for example, estimate that by their early 30s, recent cohort black men are about twice as likely to have served time in prison than to have completed a bachelor's degree, and male black high school drop-outs are more likely than not to have a prison record. For whites, on the other hand, these probabilities are on average six to eight times lower.

Taken together, these points suggest that the US is an especially critical case for the argument of this dissertation, for several reasons. On the one hand, the exceptionalism of US is intrinsically interesting: both the absence of significant redistribution and the relative punitiveness of its criminal justice system are

important political phenomena that deserve explanation. The theory developed in Chapter 2 implies that these outcomes are related—and causally connected both to inequality and to population heterogeneity. Analysing whether the empirical implications of my argument are borne out in the US is therefore quite natural. Second, the US is also a valuable addition to the empirical part of this dissertation because of the large amount of sub-national variation it offers on the explanatory variables of interest. As I discuss below, while some parts of the US are indeed as distinct from Western Europe as the discussion above would suggest, this is not true everywhere. While this may be obvious in terms of heterogeneity, it is also—and more interestingly—the case when it comes to support for redistribution and punitive attitudes. Thus, analysing the US alone enables me to leverage this variation while avoiding some of the well-known challenges inherent to cross-national comparisons using observational data. Finally, including the US allows me to engage directly with two important previous literatures: on the one hand, scholarship looking at the relationship between racial considerations (in terms of both racial composition and racial attitudes) and punitive preferences, and on the other, the more recent political economy literature on between-state variation in the relationship between income, demand for redistribution and partisanship. The argument and evidence presented in this dissertation contributes to both, and thus offers a new perspective on some familiar empirical puzzles.

Somewhat surprisingly, despite the vast scholarly efforts devoted to understanding both demand for redistribution (mainly by political scientists, and some economists) and punitive preferences (first and foremost by criminologists and sociologists) in the US, little previous empirical work has bridged the divide between these two literatures. This chapter attempts to do so by analysing a large individual-level dataset derived from the General Social Survey (GSS), covering the time period from 1993 to 2010, matched with a wide range of contextual data. It addresses the same set of expectations as Chapter 3—fundamentally, first, that the local level of inequality has a positive effect on fear of crime, and second, that importance of fear of crime to preferences over redistribution and criminal justice policies increases with income, and that these effects are highly contingent on the level of heterogeneity among the least well-off. More complex empirical issues—especially related to causality—are dealt with in the following chapter, which aims to test the micro-level mechanism linking the identity of potential criminals to preferences for alternative means of dealing with crime using experimental data.

The main body of this chapter begins in Section 4.1, by briefly revisiting the theoretical argument developed in Chapter 2 together with its main empirical implications. As before, a key expectation that follows from the model is that inequality promotes fear of crime, first, and the effect of fear of crime on preferences is contingent on the identity of those most likely to engage in crime, second. In practice, this means that it depends on the level of heterogeneity among those with relatively low incomes. Where the affluent share a salient identity trait with the poor, I expect fear of crime to increase support for redistribution, but have no effect on law enforcement preferences. Where the identities of the affluent and the poor diverge, by contrast, I expect the opposite outcome to follow. For clarity, this theoretical story and its implications are then contrasted with previous arguments regarding income, redistribution preferences and partisanship in the US.

The remaining parts of the chapter develops the analysis. Section 4.2 describes the underlying data, and Section 4.3 presents some initial descriptive patterns that emerge from it. With the necessary housekeeping out of the way, Section 4.4 turns to the main results. Paralleling the conclusions from Europe, I find that the local level of inequality has a highly robust, positive effect on fear of crime. More importantly, I also show that identity has important consequences for how fear of crime affects the preferences of white Americans. As the local area becomes more diverse, fear of crime becomes an increasingly important determinant of support for increased spending on law enforcement, but matters less and less for redistribution preferences. That is, in contexts where the level of racial heterogeneity is high, fear of crime has a reasonably large and statistically significant positive effect on demand for law enforcement, but does not affect support for redistribution. At low levels of heterogeneity, on the other hand, these relationships are reversed: when the poor are more white and less black, fear of crime has a large positive effect on redistribution preferences, but no effect on support for increased spending on law enforcement. As such, the analysis developed in this chapter lends strong empirical support for the implications of the argument developed in Chapter 2, and corroborate the European findings from Chapter 3.

## 4.1 Theoretical background

As I have said, the aim of this chapter is to test the claim that the racial composition of the area in which an individual lives matters for how fear of crime trans-

lates into political preferences, together with the background assumption that fear of crime is related to the level of inequality. It is useful to briefly revisit the logic of this argument. The model in Chapters 2 takes as its starting point the claim that tax-and-transfer considerations form an important determinant of redistribution preferences, and hence that support for redistribution declines as an individual's income increases. It also acknowledges, however, that this is not the only causal mechanism through which inequality can affect demand for redistribution: inequality also matters because of the micro-level implications of its negative externalities, especially fear of crime. The second stage of the argument, in turn, considers how concern about crime affects preferences, and has two parts. The first suggests that because tax-and-transfer concerns are immediate while externality considerations are more long-term, the former dominates the preferences of the poor while the latter becomes more important for the affluent. Thus, whereas I expect support for redistribution from the poor to be both ubiquitous and relatively stable, I expect the preferences of the affluent to show greater variation.

Given this, the second—and more important—stage of the argument asks: when should we expect the affluent to demand redistribution as a solution to crime, rather than more policing or harsher punishments for criminals? A central part of the answer to this question has to do with the level of heterogeneity amongst the poor. Borrowing insights from social psychology, I argue that heterogeneity makes the rich less likely to perceive crime and inequality as casually related, and correspondingly less likely to demand redistribution in response to fear of crime. Instead, they will turn to different forms of protection: policing, law enforcement, harsher punishments and, if possible, private security. Thus, the main thrust of the argument is that although fear of crime, and by extension inequality, can indeed generate further support for redistribution from the rich, this effect will vary from context to context with the level of heterogeneity amongst the poor. The same goes for the relationship between fear of crime and punitiveness: while I expect a positive effect of fear of crime on punitivity, the model implies that this effect is contingent on the identity of the poor. As such, the argument helps to explain two well-known empirical puzzles: geographic variation in the relationship between income and demand for redistribution, and the widely noted negative relationship between population heterogeneity and redistribution preferences.

In light of the prominence of these issues in recent comparative political economy research, it is useful to contrast this theoretical model—in which the

affluent demand redistribution because of their concern about crime, and heterogeneity matters because it weakens this effect—to alternative explanations for the same outcomes. Much has been both said and written, of course, about the potentially evolving relationship between income, redistribution preferences and voting in the US. Simplifying somewhat, it is possible to divide these arguments into two main groups: one focusing on the 'distracted poor in poor states', and the other emphasising the role of the 'cosmopolitan rich in rich states'.

The former literature, first, suggests that in poorer and predominantly rural states, second-dimension issues—mainly concerning non-economic questions relating to culture, morality, religion and, increasingly immigration, or simply 'values' for short—have come to dominate the policy preferences and voting behaviour of low-income individuals. Although not new (for a useful overview of its historical roots, see Stonecash, 2000), this idea is perhaps most clearly articulated by Frank (2004), and the argument that 'cultural wedge issues like guns and abortion' have allowed the Republicans to attract an increasing share of the white working-class vote, especially in the poorer parts of the South, the Midwest and the Northwest. The flip side of this logic, in turn, emphasises the 'yuppification' of the Democrats in an attempt to appeal to higher-income urban professionals and minority groups in costal states. To borrow a parallel from Gelman (2008), the picture that emerges from this perspective, therefore, is one of the US electorate as increasingly divided between 'Walmart America', where working-class voters drive pick-up trucks, like NASCAR, watch Fox News and vote Republican, and 'Whole Foods America', where better-paid and better-educated middle-class voters drink organic craft coffee, drive a hybrid Toyota Prius, listen to NPR and vote Democrat. Outside of academia, not least, his has been and remains a popular narrative: as Mark Penn and Andrew Stein write in the *New York Times*, for example, 'central to the Democrats' diminishment has been their loss of support among working-class voters, who feel abandoned by the party's shift away from moderate positions on trade and immigration, from backing police and tough anti-crime measures, from trying to restore manufacturing jobs' (2017). While arguments in this vein differ in the details, they share the common implication that the key to understanding redistribution in the US to do with the lack of demand for redistribution among poor voters in predominantly rural states.

Despite its popularity, however, there are good reasons to remain somewhat sceptical of this perspective. On a conceptual level, it is important to avoid the

invalid inference that because the rich, coastal states (Connecticut and California, for example) tend to support the Democrats while the poor, inland states tend to vote Republican (think Arkansas and Kansas), rich voters tend to be pro-redistribution and support the Democrats, while poor voters tend to be against redistribution and support the Republicans. The latter does not follow from the former: it is perfectly possible for Republican voters to be richer, on average, than Democratic voters, yet for richer states to be more Democratic, on average, than poorer states. What is more, the salience of 'second-dimension' issues itself says little about the relationship between income and redistribution preferences—it is possible to be socially conservative but economically liberal, in the American sense, and vice versa. Empirically, finally, and in line with this reasoning, the findings reported below cast significant doubt on the claim that low-income individuals in poorer states have lost interest in redistribution. On the contrary, and paralleling the results of Gelman (2008), Bartels (2006), McCarty, Poole, and Rosenthal (2006) and Rueda and Stegmueller (2019), I find that across the US, support for redistribution among the poor is both widespread and fairly ubiquitous, contrary to the idea that a significant share of low-income voters have economic preferences that go against their material self-interest. As such, the results from the analysis in this chapter suggest that the answer to Thomas Frank's question—What's the Matter with Kansas?—has little to do with the redistribution preferences of the poor. Rather, and as I discuss below, most of the geographic variation in demand for redistribution in the US has to do with the preferences of the rich: while the preferences of the poor remain relatively constant across contexts, the preferences of the rich show much greater variation. In this sense, therefore, there is not a world of difference between the US and Europe.

With that said, the analysis presented below is of course not the first to note that there is a significant amount of geographical heterogeneity in the redistribution preferences of rich Americans. In particular, the contributions by Gelman (2008) and McCarty, Poole, and Rosenthal (2006) have emphasised the link between state wealth and Democratic voting and partisanship among high-income voters. Summarised briefly, these arguments suggest that while the effect of income on support for the Republicans is unambiguously positive—meaning that as income increases, the probability of voting for the Democrats declines—in rich and poor states alike, rich voters in rich states are more likely to vote Democrat than rich voters in poor states because of the relationship between state wealth

and cosmopolitanism, or post-material values. As Gelman (2010, pp. 140) writes, 'it is richer Americans in richer parts of the country, more than the poor and the rural, who are voting based on "God, guns, and gays"…at the higher income brackets, we see the red-state, blue-state deadlock'. Others have made similar observations: Ansolabehere, Rodden, and Snyder (2006, pp. 110), for example, find that while 'moral issues have slightly smaller impact on the vote choices of rural residents than urban and suburban residents', it is also true that 'low-income Americans are significantly less inclined to vote on moral values than are high-income groups'. In important respects, this is the inverse of the 'distracted poor' argument discussed in the previous paragraph: while Frank (2004) and much of the popular narrative about the income-vote relationship in the US suggests that second-dimension issues are disproportionally important for the poor (leading the poor to vote for the right, against their material self-interest), this line of argument suggests that such concerns are more important for the more affluent (leading them to vote for the Democrats, again, against their self-interest). This finding, in turn, shifts attention away from demand- to supply-side factors.

While the basic empirical implications of this argument are similar to those that follow from the theoretical model developed this dissertation—especially in emphasising geographical variation on in the redistribution preferences of the rich—it is important to note that they are not identical. First, rather than state wealth, I emphasise the importance of the local level of inequality and its connection to fear of crime. Moreover, although some states are rich and unequal (Connecticut), or poor and relatively equal (Idaho), this correlation is far from perfect: New Hampshire, for example, is both rich and equal, while Alabama and Tennessee are both poor and unequal. The same holds true for metropolitan regions, as the main macro-level unit used in the analysis below: some MSAs are rich and unequal (San Francisco-Oakland, for example) and some are poor and equal (Kansas City), yet others are rich and equal (Seattle-Tacoma) while others are both poor and unequal (New Orleans). In this sense, my argument and results are compatible with those of Rueda and Stegmueller (2019).

Second—and in contrast to the arguments discussed above—the theoretical logic proposed in this dissertation implies that there is a direct relationship between racial heterogeneity and support for redistribution from the affluent, especially at the local level. That is, while I expect affluent individuals living in unequal areas to have higher redistribution preferences than those who live in

places that are more equal, my argument implies that this effect is highly contingent on the racial composition of the poor. This means that rather than a simple 'rich state-poor state' or 'unequal state-equal state' divide, I expect the preferences of the rich to vary across two dimensions: local inequality and local racial composition. Finally, and perhaps more crucially, my explanation for these outcomes differs starkly from previous theories. Where Gelman (2008) and others focus on post-materialism and cosmopolitan, liberal values, I emphasise the relationship between inequality and fear of crime, first, and the effect of racial composition on the relation between fear of crime and preferences, second.

## 4.2 Data

The analysis developed below relies primarily on individual-level data from the General Social Survey (GSS), covering the period from 1993 to 2010—that is, when geographic information at the county level is available. Similar datasets have been used extensively in previous studies, both on the determinants of redistribution preferences (Alesina and Giuliano, 2011; Alesina and La Ferrara, 2005; Rueda and Stegmueller, 2019, and many others) and on the antecedents of punitive attitudes (for example, Enns, 2014; Cundiff and Alwin, 2015; Ramirez, 2013; Barkan and Cohn, 2005). Following the same logic as in the analysis of ESS data in Chapter 3, I limit the sample used for the main analysis to non-hispanic white respondents who are not retired or in full-time education. Since a measure of hispanic status is only available in the surveys from 2000 and later, this means that the primary models reported below rely on data from the 2000 to 2010 period only. As a robustness test in Section 4.5, however, I show that the results of the main analysis remain substantively unchanged when hispanic respondents are included in the sample. Missing data is accounted for by listwise deletion.

Compared to other large-scale surveys fielded in the US, the GSS comes with a number of distinct advantages. It is large, covers a relatively long time-period with consistent items, and employs a highly robust and well-documented sampling methodology. Given the purpose of this chapter, moreover, the GSS also has a number of more specific merits: it includes all of the core variables needed to test my theoretical argument—most importantly, a consistent measure of income, and variables measuring fear of crime, support for redistribution and views about law enforcement—as well as detailed geographical information allowing respon-

dents to be matched with local-level contextual data. To the best of my knowledge, no other large comparative survey, whether cross-sectional or panel, ticks all the same boxes. The American National Election Study (ANES), for example, does not ask about fear of crime. The same holds true for the surveys fielded by the International Social Survey Program (ISSP), which include neither a consistent measure of income, nor items tapping into fear of crime and punitive preferences.

That being said, the analysis developed below nonetheless comes with a number of methodological challenges. In particular, an important issue has to do with residential segregation, especially along racial lines: the tendency for non-hispanic whites and blacks to live in different places. As a large literature has documented, while nationwide residential segregation has declined quite sharply since the early 1980s, it remains an important part of contemporary American political geography (Cutler, Glaeser, and Vigdor, 1999; Fischer et al., 2004; Boustan, 2011). Moreover, most of the noted aggregate-level decline is attributable to decreasing segregation between neighbourhoods within cities, rather than decreasing segregation between higher-level units such as cities or states: although members of different racial groups are increasingly likely to live in the same neighbourhood or area within a given city or metropolitan area, they have not become significantly more likely to live in the same city to begin with (for a useful discussion if these issues, see Fischer et al., 2004). From a theoretical perspective, residential segregation does not necessarily pose a problem: my argument simply implies that the identity of those most likely to engage in crime against a particular victim matters for the latter's views about the appropriate solutions to crime, independently of segregation. Taking one step back, residential segregation is likely to matter for who those likely perpetrators of crime are, but it does not—at least not in and of itself—modify this basic theoretical reasoning.

Empirically, however, residential segregation poses more of a challenge, for two main reasons. One has to do with residential sorting: as in any purely cross-sectional study, the analysis developed below suffers from the limitation that residence in a particular state or city, with a given level of heterogeneity, might not be independent from pre-existing attitudes towards redistribution and law enforcement. Although there is no perfect solution to this problem, as a robustness test in Section 4.5.1, I replicate the main analysis using a limited sample including only those respondents who at the time of the survey resided in the same state where they also lived at age 16. This reduces the sample size dramatically, yet the

main effects—including that of heterogeneity—remain unchanged. Apart from sorting, persistent racial residential segregation also implies that aggregate-level measures of heterogeneity—at the state level, for example—are likely to be misleading. The analysis below takes two steps to address this issue. First, to account for segregation between cities and larger areas within states, I use Metropolitan Statistical Areas (MSAs), defined by the Census Bureau as one or several counties with a combined population of 50,000 or more, as the main macro-unit of analysis. This allows me to measure heterogeneity at a theoretically relevant, comparatively local level. Although using even smaller geographical units—such as counties, neighbourhoods, or census tracts—may help to resolve the segregation issue further, doing so also comes with a number of drawbacks, including but not limited to restrictive sample sizes in the GSS. MSAs also come with the benefit of capturing regions that are interconnected—an urban core with outlying suburban areas, for example. To the extent that individual perceptions regarding crime depend not only on contextual factors in the local area where they live, but also on the characteristics of the areas in which they work, travel through, and generally spend time in, MSAs are thus a highly reasonable choice of macro-unit. To control for segregation within cities, finally, I also control for MSA-level residential segregation in all the main models discussed below (see Section 4.2.4 for details).

### 4.2.1 Dependent variables

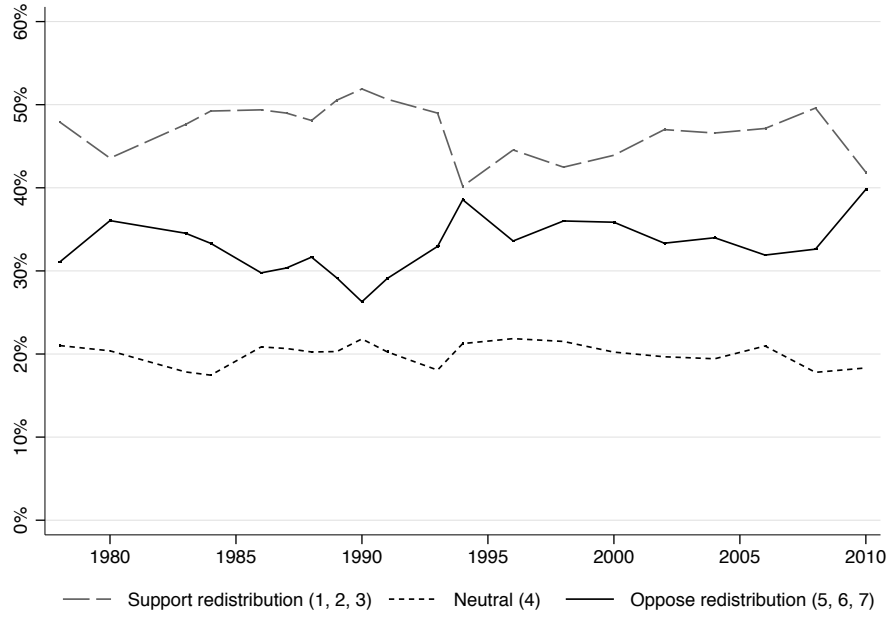
**Redistribution preferences** To measure support for redistribution, I use a standard item included all waves of the GSS since 1978 (with the exception of the surveys conducted in 1982 and 1985). This item asks respondents whether they think that the federal government should reduce income differences between the 'rich' and the 'poor'. It presents respondents with the following vignette:

> 'Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor. Here is a card with a scale from 1 to 7. Think of a score of 1 as meaning that the government ought to reduce the income differences between rich and poor, and a score of 7 meaning that the government should not concern itself with reducing income

differences. What score between 1 and 7 comes closest to the way you feel?'

The same survey item has been employed by a large number of previous authors analysing redistribution preferences using GSS data (Rueda and Stegmueller, 2019; Alesina and Angeletos, 2005; Alesina and Giuliano, 2011, and many others), and is furthermore reasonably similar to corresponding items included in other large opinion surveys (including the ESS analysed in Chapter 3). To keep the interpretation of the results as simple as possible, I use responses to this question to construct a binary variable coded as 1 if a respondent answered 1, 2 or 3, and as 0 otherwise. The resulting variable thus indicates whether a respondent supports rather than is neutral about or opposes redistribution. While there are several advantages to this procedure, using a binary dependent variable importantly allows me to calculate simple predicted probabilities—that is, the predicted

**Figure 4.1:** Support for redistribution in the US, 1978-2010




*Note:* GSS data, 1978-2010. The figure shows the (truncated) distribution of responses to the redistribution item, based on the untransformed survey variable: lower values indicate greater support for redistribution, and higher values indicate less support for redistribution. Percentages sum to 100%, and are calculated with only complete responses in the denominator.

probability of expressing some degree of support for redistribution—and therefore to avoid some of the complexities that often arise with the interpretation of ordinal regression results. As a robustness test, however, I replicate the main analysis using the untransformed ordinal variable, and show that this does not substantively alter the results (for details, see Section 4.5 below).

For reference, Figure 4.1 plots the percentage of respondents who support (with an answer of three or lower on the seven-point scale, dashed line), are neural about (an answer of four, solid line), and oppose redistribution (an answer of four or higher, short-dashed line) in each survey year since 1978. For the sample as a whole, the overall mean response is 4.28, with a standard deviation of 1.96. These simple statistics suggests that while support for redistribution is indeed lower in the US than it is in Western Europe—as shown in Table 3.1 in Chapter 3, more than 70% of ESS respondents either 'agree' or 'strongly agree' with the statement that the government should reduce differences in income levels, compared to less than 15% who disagree—it is nevertheless more widespread than some might assume: while around one-third of GSS respondents actively oppose redistribution, more than 45% express some degree of agreement with the claim that the federal government should work to reduce income inequality. What is perhaps also surprising is the apparent stability of these opinions over time. Apart from a sharp turn towards increasing opposition to redistribution in the early 1990s (followed by a gradual increase in support over the following decade) and a similar shift in the years leading up to 2010, the distribution of opinion in the late 2000s is remarkably similar to that at the start of the 1980s. As I show below, however, these aggregate level patterns hide a significant amount of lower-level variation, both between income groups and between states, regions and cities.

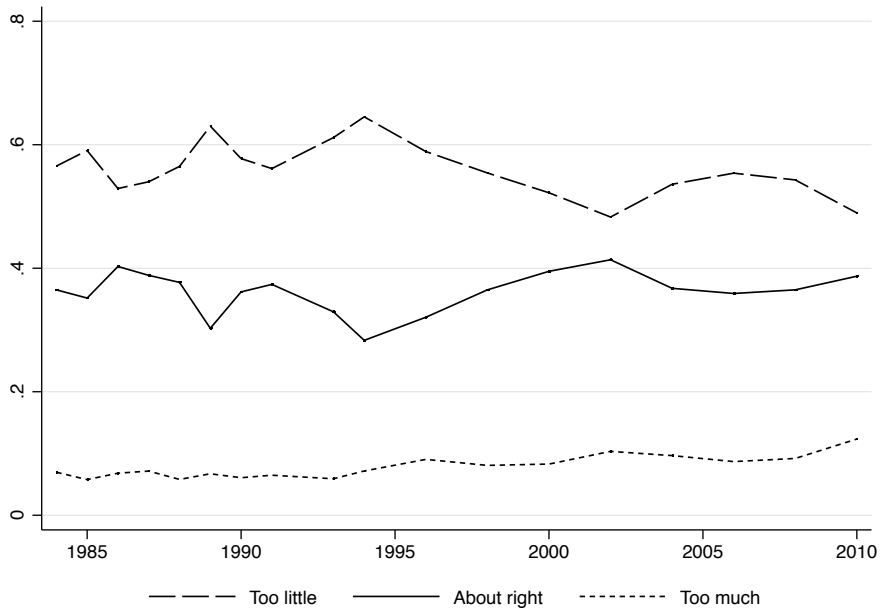
**Support for law enforcement spending** Since 1988, the GSS also includes a useful item asking respondents about their views of government spending on law enforcement. This item presents respondents with the following statement:

> 'We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount. Are we spending too much, too little, or about the right amount on law enforcement?'

Answers are recorded on a three-point scale from 'too little' to 'about right' and 'too much'. As with the redistribution question discussed above, I use responses from this item to construct a binary variable with 'too little' answers coded as positive outcomes, and 'about right' and 'too much' answers coded as zero.

For illustration, Figure 4.2 below plots the distribution of answers to this question—representing the view that spending on law enforcement should increase (dashed line), that it is about right (solid line), or that it should decrease (short-dashed line)—for the full 1978 to 2010 GSS dataset. These trends will not come as a surprise to those familiar with the literature on punitiveness and law enforcement preferences in the US, but are nonetheless quite notable: on average, around 55% of respondents think that spending on law enforcement is too low, compared to 35% who believe that spending is good as it is, and less than 9% who think that current spending on law enforcement is too high. The time trend is also interesting. Support for increased spending increased moderately

**Figure 4.2:** Support for law enforcement spending in the US




*Note:* GSS data, 1984-2010. The figure shows the distribution of responses to the law enforcement item described in the main text. Percentages sum to 100%, and are calculated with only complete responses in the denominator.

from the early 1980s to the mid-1990s, after which it decreased steadily until around 2003. The remainder of the 2000s show a slight increase followed by a similarly sized decrease in support for increased spending, together with signs of growing polarisation in these opinions (for further discussion of these trends, see Applegate et al., 2000; Ramirez, 2013). Part of this is likely explained by trends in actual law enforcement spending, which increased dramatically through most of the 1980s and 1990s—from around $84 billion (in real dollars) at the start of the 1980s to $189 billion at the end of the 1990s, or equally, increased from $363 to $708 per capita—but has remained relatively constant since (Kyckelhahn, 2011; see also BJS, 2019). In the analysis below, these trends are controlled for either explicitly or with time fixed-effects. As before, moreover, these aggregate figures hide significant amounts of more fine-grained differences, as shown below.

### 4.2.2 Independent variables

Following the theoretical logic in Chapter 2 and paralleling the analysis of European data in Chapter 3, the analysis below has four main, substantively interesting independent variables: fear of crime, relative income, and on the MSA-level, the level of population heterogeneity and inequality. Descriptive statistics for these and all other variables used in the analysis are available in Appendix B.

**Fear of crime** To measure fear of crime, I once again use a survey item that asks 'Is there any area right around here—that is, within a mile—where you would be afraid to walk alone at night?'. Answers are recorded on a simple, two-point yes/no scale. In the analysis below, I code this variable such that a positive outcome indicates an affirmative response and a zero outcome represents a negative response. Thus, the variable used in the main regressions can be interpreted as a straightforward indicator of whether the respondent is afraid of crime.

As discussed in Chapter 3, while variations of this item have been used extensively in previous work on fear of crime, it has also been criticised from several angles. Given my theoretical interests, the perhaps most serious issue relates to the fact that the question can be argued to tap into fear of violent crime rather than fear of property crime. Although this is not necessarily problematic in its own right, it does pose a challenge to the extent that inequality matters for property crime, but does not have an effect on violent crime. In

practice, however, there are several reasons for dismissing this argument. As I have said previously, first, several prominent economic models of criminal behaviour predicts a positive relationship between inequality and violent crime, either directly or as a consequence of crime motivated by economic incentives (see, for example, Bourguignon, 2001; Ehrlich, 1973; Kelly, 2000; Soares, 2004). Similarly but from a more sociological perspective, deprivation theory links inequality to frustration and anger amongst the poor, which in turn predicts involvement in violent crime (Hagan and Peterson, 1995; Merton, 1938). In line with these literatures, moreover, a large body of empirical evidence—both cross-national and US-focused—supports the expectation that changes in inequality affects not only property crime but also violent crime (see Fajnzylber, Lederman, and Loayza, 2002a,b; Blau and Blau, 1982; Kelly, 2000).

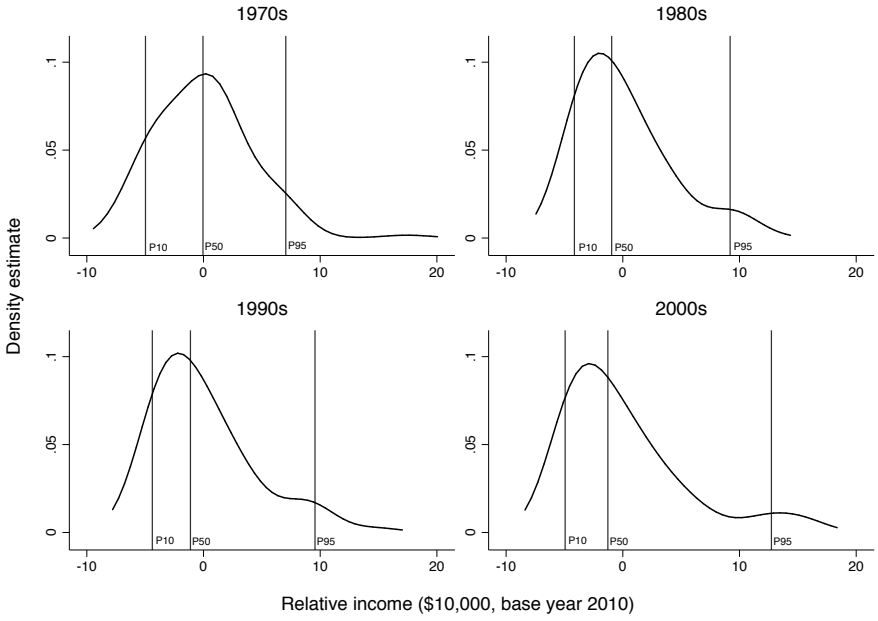
**Relative income** The GSS records respondents' total gross household income in a number of income bands, which differ both in number and in nominal value between survey years. To develop a variable that accurately captures real relative income—defined as the inflation adjusted difference between a respondents' household income and mean income—I follow the same method as for the ESS and discussed in Chapter 3. To summarise: I first convert each income band to its midpoint, and impute a 'midpoint' for the (open-ended) top category using the formula suggested by Hout (2004). Each income variable is then centred on the year-mean, meaning that zero always represents the mean. Finally, I convert all income figures to units of constant $10,000 with base year 2010, using the CPI-U-RS index published by the Bureau of Labor Statistics (BLS, 2019).

Figure 4.3 plots the distribution of the resulting relative income variable for each decade covered by the GSS data. While this dataset is admittedly ill-suited for studying inequality—not least because it almost certainly underestimates the income share of those on the extreme right-hand tail of the distribution—these estimates clearly reflect the increase in inequality noted elsewhere (Piketty, 2014; Atkinson, Piketty, and Saez, 2011). Since the 1970s, the real income distance between those at the bottom and those in the centre of the income distribution has remained relatively stable (see Autor, Katz, and Kearney, 2006). What is more remarkable, however, is the rapid income growth experienced by those with the highest incomes, as is visible in the widening income gap between the median and the 95th percentile. In the 1970s, this difference averaged around $64,000

(again, in 2010 dollars). In the 1980s it grew to $92,300, and in the 1990s it grew even more to reach $115,500. In the decade starting in 2000, finally, it averaged more than $156,000. These estimates largely parallel those derived using the more widely used Current Population Survey (CPS, 2018) data collected by the Census Bureau, which estimates a difference of $79,900 in the 1970s, $94,500 in the 1980s, $115,100 in the 1990s, and $133,200 in the 2000s.

**Heterogeneity** To measure population heterogeneity, I use census data to calculate the percentage of black residents in each MSA. For non-census years, I linearly interpolate this variable. As discussed in Chapter 3, while the theoretically relevant consideration has to do heterogeneity among the poor, this variable measures heterogeneity without accounting for its relationship to income. Despite this downside, however, there are good reasons for preferring the census measure over a variable constructed from survey responses to the GSS, not least since the

**Figure 4.3:** Distribution of relative income by decade




*Note:* GSS data, 1972-2010. The figures show the distribution of relative income in the pooled GSS sample for each decade. On the $x$-axis, the reference lines represent the 10th (P10), 50th (P50) and 95th (P95) percentiles. Gaussian kernel estimates, bandwidth = 2.

sample size in some MSAs is fairly small (both in absolute terms, and relative to the country-level samples used to derive this variable in Chapter 3). As previous studies have shown, moreover, an overwhelming share of black individuals are concentrated in the lower half of the income distribution, implying that this is not necessarily an issue. Hence, I stick with the census measure for the main analysis, but include a corresponding survey based variable measuring heterogeneity in the lower half of the income distribution as a robustness test in Section 4.5.

**Inequality** Following the same logic, the sample size in most MSAs is too small to derive robust inequality estimates directly from the GSS data. This, in turn, poses a dilemma. There are two alternative sources for MSA-level inequality data. Since census micro-data is not available for geographic delineations with fewer than 100,000 inhabitants, one option is data from the Current Population Survey (CPS). Again, however, confidentiality reasons mean that geographic information in the CPS is only available for a subset of the MSAs included in the main analysis. For these reasons, I instead employ MSA-level gini data from the American Communities Survey (ACS), as tabulated by the Census Bureau. This dataset comes with the benefit of covering all the MSAs of interest, but on the downside, is only available from 2006 and onwards (and then on an annual basis). For each MSA, I therefore take the average of these measurements to estimate the level of local inequality for the period from 1993 to 2010. Although the amount of cross-sectional variation in inequality is significantly larger than the corresponding variation within MSAs over time, this approach of course remains far from ideal. Once again, however, data availability limits what I can do.

### 4.2.3 Individual-level control variables

In addition to these substantive variables, the analysis below also controls for a number of other factors that have been shown to matter for redistribution preferences and/or support for punitive criminal justice policy. Similar batteries of control variables have been used by a large number of previous authors, including Rehm (2009), Iversen and Soskice (2001), Rueda and Stegmueller (2016), Alesina and Giuliano (2011), Unnever and Cullen (2010), and many others.

**Age** The regression models estimated below include a control for the respondent's age, measured in years, and its square. As mentioned previously, past

research suggests that support for redistribution increases with age, net of other factors (Rueda and Stegmueller, 2016; Iversen and Soskice, 2001). In contrast, research on the relationship between age and punitiveness has produced less consistent results: while some studies report a positive relationship, others find that age and punitiveness are unrelated, and yet others suggest that the relationship is contingent on other factors, such as gender (see Rossi and Berk, 1997; Hough et al., 2013; Mayhew and van Dijk, 1997, amongst many others).

**Gender** In line with most previous studies on redistribution as well as punitive preferences, I also include a control for the respondent's gender, coded as 1 if he or she self-identifies as female. Previous research suggests that women tend to be more supportive of redistribution than men (cf. references in previous paragraph), possibly due to household effects or gender differentials in employment security. As with age, evidence about the relationship between gender and punitiveness are again more mixed, although the majority of empirical studies find that women ten to be less supportive of punitive policies than men, all else equal (Applegate, Cullen, and Fisher, 2002; Gault and Sabini, 2000).

**Education** The analysis below also controls for educational attainment, measured in years of full-time schooling. Theoretically, the relationship between education and support for education is somewhat ambiguous. On the one hand, and net of current income, education can be expected to negatively affect support for redistribution to the extent that it matters for beliefs or prospects of upward social mobility (that is, if eduction is a proxy for expected future earnings). On the other hand, the well-documented association between education and liberal beliefs points in the opposite direction. Most empirical studies, however, find a negative relationship between educational attainment—especially the difference between having and not having a college degree—thus suggesting that the former mechanism dominates the latter (Alesina and Giuliano, 2011; Rueda and Stegmueller, 2016). Similarly, a large body of evidence points to a strong negative effect between education and support for punitive criminal justice policy (Dowler, 2003; Payne et al., 2004; Bowers, 1998; Rossi and Berk, 1997).

**Religiosity** Likewise, I also control for religiosity, using a standard item from the GSS. This question asks respondents how religiously 'fundamentalist' they

are, and records answers on a three-point scale from 'liberal', to 'moderate' and 'fundamentalist'. In most survey years, around 40% of respondents self-identify as moderates, while the remaining 60% are close to equally divided between the fundamentalist and the liberal categories. As mentioned in Chapter 3, previous research suggests that individuals that hold strong religious beliefs tend to be both less supportive of redistribution and more supportive of punitive criminal justice policy (Scheve and Stasavage, 2006; Stegmueller et al., 2012).

**Marital status** Finally, I also include a dummy variable indicating whether the respondent is legally married. Although evidence about the relationship between marital status and demand for redistribution remains somewhat mixed, there is a relatively robust correlation between being married and being more punitive towards criminals (see Dowler, 2003, and Payne et al., 2004, amongst many others).

### 4.2.4 Macro-level control variables

As in Chapter 3, I deliberately keep my main models comparatively parsimonious on the macro-level, controlling only for residential segregation, the existing level of redistribution and current law enforcement employment. Additional MSA-level controls, however, are added as robustness tests in Section 4.5.

**Residential segregation** As mentioned above, all models estimated below control for MSA-level residential segregation, using the White-Black Dissimilarity Index (DI, also referred to as the index of dissimilarity). Measured at the census tract level, the DI can be interpreted as the percentage of either racial group that would have to relocate to a different census tract in order for all such tracts to have the same racial composition as the MSA as whole. The range of possible values thus theoretically ranges from 0 to 1 (or 0% to 100%), with higher scores indicating greater segregation. For census years, DI data is available from the American Communities Project at Brown University (ACP, 2019). For non-census years, I use linear interpolation to estimate MSA-level scores.

**Federal transfers** Like most analyses of redistribution preferences, I also attempt to control for existing levels of redistribution. Although national trends in welfare spending are accounted for by the year fixed-effects included in all models discussed below, the same does not hold true for between-state (and

MSA) differences. It is possible, for example, that agreeing with the statement that the federal government should do more to reduce income differences means one thing in Colorado, which receives a comparatively small share of federal per-capita transfers, but another thing in Florida, where the same share is much larger. Following Dimick, Rueda, and Stegmueller (2016), I therefore include a variable that measures the average nominal amount of direct federal transfers received by citizens in each state in each year, based on data from the Bureau of Economic Analysis (BEA, 2018). This measure includes transfers received through a variety of programmes: Social Security, Medicaid, Supplemental Social Income, the Supplemental Nutrition Assistance Programme, as well as veterans' benefits and some forms of Federal unemployment compensation (see Appendix B for a full discussion). While employing an MSA-level control of existing redistribution would have been desirable, BEA data is unfortunately only available on the state level.

**Law enforcement employment** Following a similar logic, on the other side of the analysis I include a control for the number of law enforcement officers per capita, based on data from the annual Law Enforcement Employees report provided as part of the FBI's Uniform Crime Reporting Program (Department of Justice, 2018). This variable includes both civilian and non-civilian employees, covers local, state and federal agencies, and is measured at the MSA-level. Although spending data might have been preferable—given, in particular, that my dependent variable asks about law enforcement spending—no consistent measure of law enforcement expenditures is available below the state level.

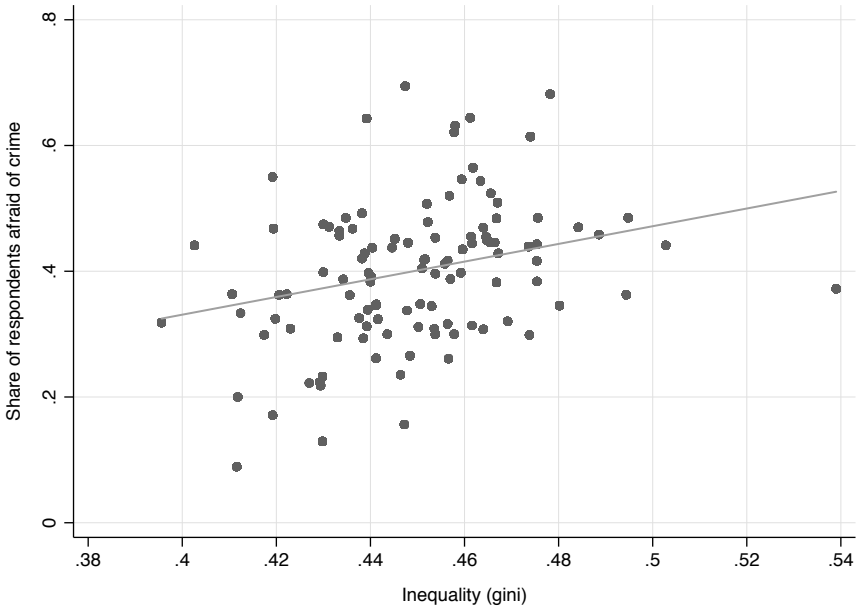
## 4.3 Descriptive patterns

Before turning to the main empirical analysis below, it is useful to consider some simple but interesting descriptive patterns that emerge from the data. To that end, Figure 4.4 begins by plotting the relationship between MSA-level inequality and the share of respondents who report being afraid of crime, pooled across all survey years. Although there are some distinct outliers—most notably the Bridgeport-Stamford-Norwalk MSA in Connecticut, with very high inequality but an average prevalence of fear of crime—there appears to be a clear, positive correlation between local inequality and fear of crime ($r = 0.33$, $p < 0.01$), par-

alleling the pattern observed in Europe and discussed in Chapter 3. Once again, I return to analyse this relationship more systematically in Section 4.4.1 below.

Moving on to redistribution preferences, Figure 4.5 disaggregated support for redistribution by income: it plots the share of respondents that support redistribution among the rich (defined as those having an income $23,550 or more above the national mean, representing the 75th percentile and above) and among the poor (with an income less than $37,080 below the mean, 25th percentile and below), together with the mean for the sample as a whole. Not surprisingly, support for redistribution is higher among the poor than among the more affluent: on average, around 55% poor respondents agree that the government should reduce income differences, compared to just above 35% of the rich. The difference between the two income groups remains relatively constant over time, apart from brief periods of increased polarisation in the early 1990s, when support for redistribution among declined sharply among the affluent but remained constant among the poor, and the late 2000s, when the opposite trend is notable. Finally, the time trends are very similar those noted earlier in this chapter: following a steady increase in support for redistribution throughout the 1980s,

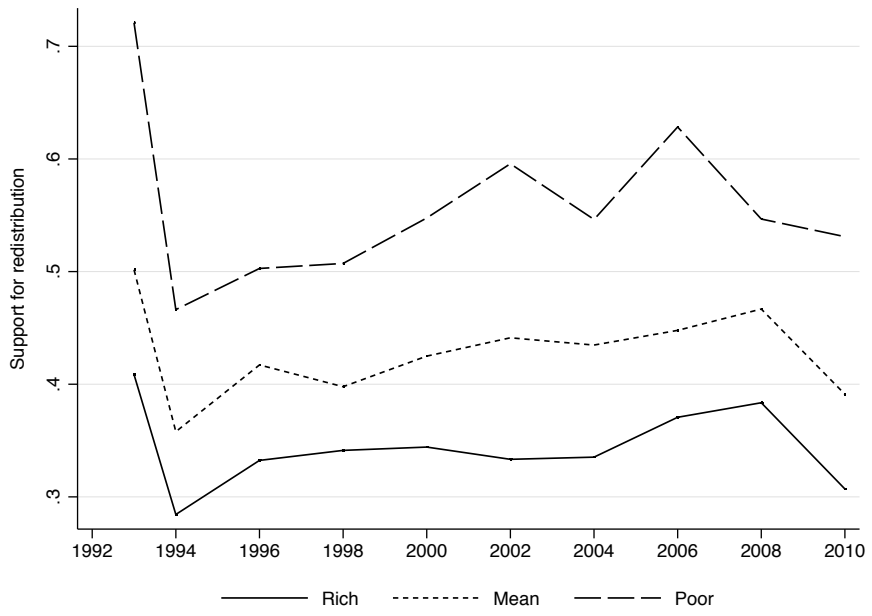
**Figure 4.4:** Inequality and fear of crime



support declined dramatically in the early 1990s, and then increased incrementally until the around 2008, for rich and poor respondents alike.

While the data presented in Figure 4.5 is illustrative, it nevertheless obscures a significant amount of lower level variation that is central to my theoretical argument, not least in the relationship between relative income and demand for redistribution. To touch on this issue, Figure 4.6 plots the distribution of the difference (in percentage-points) between the share of the poor (again, 25th percentile or below) that demand redistribution and the corresponding share of the rich (75th percentile or above) that demand redistribution, for each MSA for which I have data. Positive values (on the $x$-axis) thus imply that the average level of support for redistribution is higher among the poor than among the rich, while negative values imply the opposite. Although these numbers should be interpreted with some degree of caution given the comparatively small size of some MSAs, they point to an impressive amount of geographical variation: in some metropolitan areas, aggregate level support for redistribution appears to be almost unaffected by relative income, whereas in other areas the preferences of the rich and the poor are much further apart. In the Nashville-Davidson-Murfreesboro MSA, for ex-

**Figure 4.5:** Support for redistribution in the US by income



ample, the difference in support for redistribution between the rich and the poor is only around 7 percentage-points, whereas the same difference is 14 percentage-points in the New York City-Newark-Jersey City MSA, and more than 31 percentage-points in the Washington-Arlington-Alexandria MSA. The more systematic analysis developed later in this chapter helps to explain these patterns.

Moving on, as with the relationship between income and demand for redistribution, the argument developed in Chapter 2 implies that the effect of fear of crime on redistribution preferences depends on the level of population heterogeneity among the poor. While I explore this relationship at length below, an implication of this logic is that the relationship between fear of crime and support for redistribution should exhibit a significant amount of geographical variation: that is, if the argument is correct, it follows that whereas individuals who are afraid of crime should be more supportive of redistribution than those who are not afraid of crime in some places, this should not be the case elsewhere.

To show that this is indeed the case, Figure 4.7 plots the distribution of the percentage point difference between the share of respondents that support redistribution among those who are afraid of crime, and the share of respondents

**Figure 4.6:** Poor-rich difference in support for redistribution

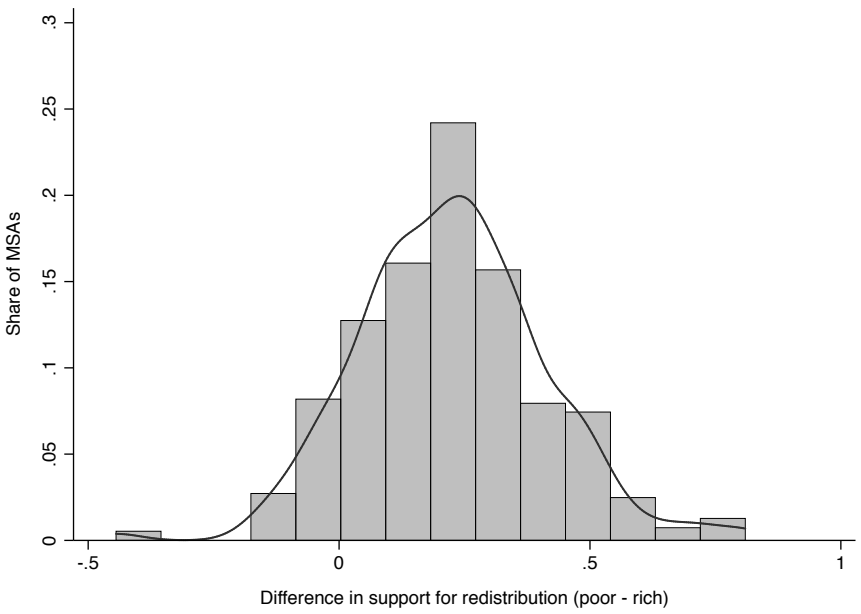


**Figure 4.7:** Fear of crime and support for redistribution

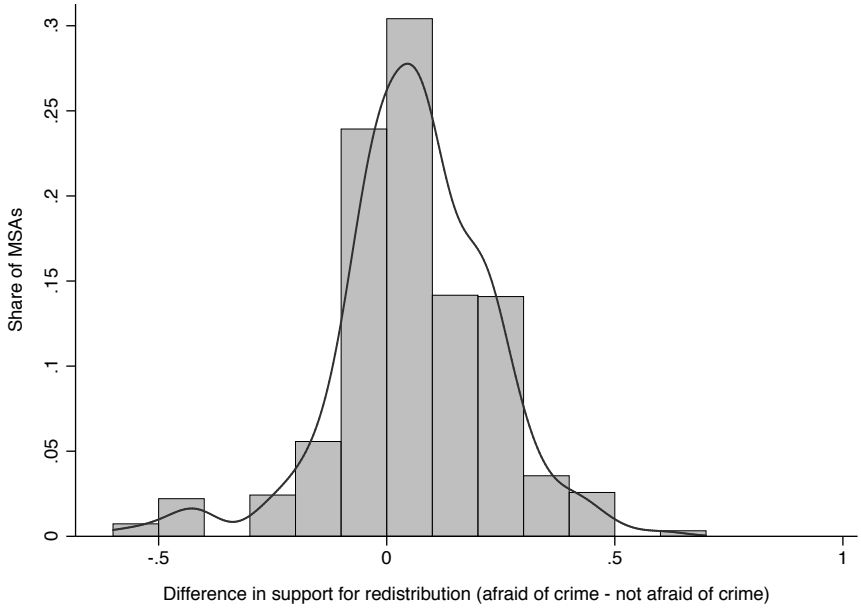
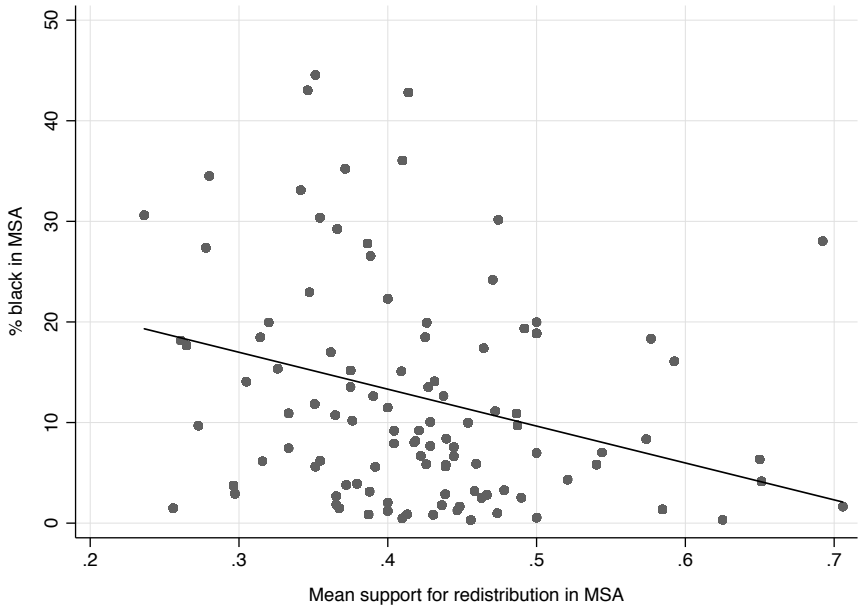



**Figure 4.8:** Support for redistribution and black population



that support redistribution among those who are not afraid of crime, again in each MSA. Interpretation thus mirrors that of Figure 4.6: higher values indicate that support for redistribution is more widespread among those who are afraid of crime, while negative values imply the opposite. Two points are particularly noteworthy. First, individuals who are afraid of crime tend to be more supportive of redistribution than those who feel safe. The mean difference is around 5.1 percentage-points. The second and perhaps more important point to note, however, is once again the significant amount of variation that this difference exhibits across metropolitan regions. For the sample as a whole, the difference ranges from around 45 percentage-points in Burlington, VT, to less than $-25$ percentage-points in Tuscaloosa, AL, and Muncie, IN. Overall, the difference is negative in about one-third of MSAs. This is not necessarily problematic, however, since there are a multitude of reasons for why fear of crime and opposition to redistribution may be correlated, but not causally related. These numbers do little to explain this variation, of course, but they show relationship between fear of crime and demand for redistribution is far from homogenous.

Finally, another implication of my argument is that support for redistribution should vary negatively with the level of heterogeneity among the poor. While I explore this issue in some detail below—and, importantly, show both that part of this effect is accounted for by fear of crime, and that it has more to do with the redistribution preferences of the rich than those of the poor—Figure 4.8 provides a snapshot of the macro-level correlation between mean support for redistribution among white respondents and the percentage of black individuals in the population, averaged across all survey years. The negative correlation between the black population share and demand for redistribution among whites is not surprising, and has been commented on before, but is nonetheless notable. The most diverse MSAs uniformly have relatively low average levels of support for redistribution: in the MSAs where more than one-quarter of the population is black, only two—Durham-Chapel Hill, NC, and Charleston, NC—have an above average share of respondents that favour redistribution, compared to 13 MSAs where fewer than average respondents favour redistribution. In line with expectations, at low levels of heterogeneity the variance in redistribution preferences is greater. Again, the analysis below helps to explain these patterns.

Turning to the other side of my argument, Figure 4.9 plots support for increased law enforcement spending, disaggregated by income group. As before,

I define the poor as those with an income less than $37,080 below the mean (that is, those in the 25th percentile or below) and the rich as those earning $23,550 or more above the mean (75th percentile and above). As noted above, support for increased spending on law enforcement is reasonably widespread in this sample, but shows a declining trend throughout the 1990s. More interestingly, however, Figure 4.9 also shows that for most of this period, support for law enforcement varies by income: the affluent are more supportive of spending on law enforcement than what is the case among the poor. There is also evidence of decreasing polarisation, with the difference in preferences between the rich and the poor peaking in the late 1990s, but declining to almost zero since.

Moving on, as with demand for redistribution, the theoretical argument developed in Chapter 2 implies that although individuals who are afraid of crime should be more punitive in some regions, this relationship will vary with heterogeneity between different contexts. If the argument is accurate, therefore, there should be geographical variation in the extent to which those who are afraid of crime are more punitive than those who feel safe. To that end, Figure 4.10 plots the distribution of the difference (again in percentage-points) in the proportion of respondents that support more law enforcement spending between those who feel unsafe and those who feel safe. Figure 4.10 can thus be interpreted in a similar way to Figures 4.6 and 4.7 above: more positive values indicate that support for increased law enforcement spending is more widespread among those who are afraid of crime relative to what is the case among those who are not afraid. As with the relationship between fear of crime and demand for redistribution, Figure 4.10 shows that the relationship between fear and law enforcement preferences is all but stable. On average, the share of individuals in favour of increased spending on law enforcement is around 7 points greater for those who are afraid of crime. This figure, however, masks a great deal of geographical variation: across the sample of MSAs, the difference ranges from more than 75 percentage-points (in Poughkeepsie-Newburgh-Middletown, NJ) to -50 percentage-points (in Pine Bluff, AR), with the inter-quartile range falling between -1.5 and 17 points.

Lastly, Figure 4.11 plots the percentage of respondents in each MSA that support increased law enforcement spending against the percentage of black residents. As with redistribution preferences, my argument implies that—all else equal—support for spending on criminal justice policies will be lower in areas where the minority population is reasonably small, but higher where the

**Figure 4.9:** Support for law enforcement spending in the US by income

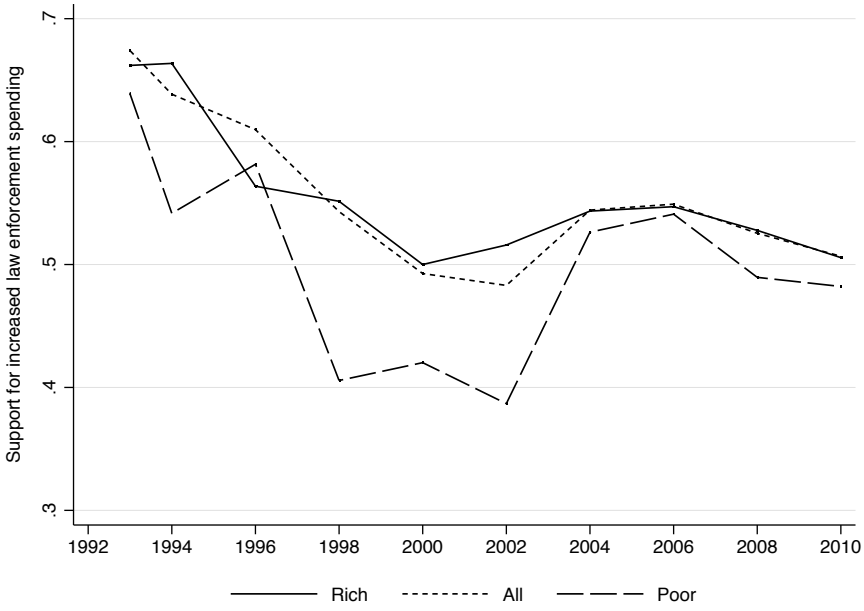



**Figure 4.10:** Support for law enforcement spending and fear of crime

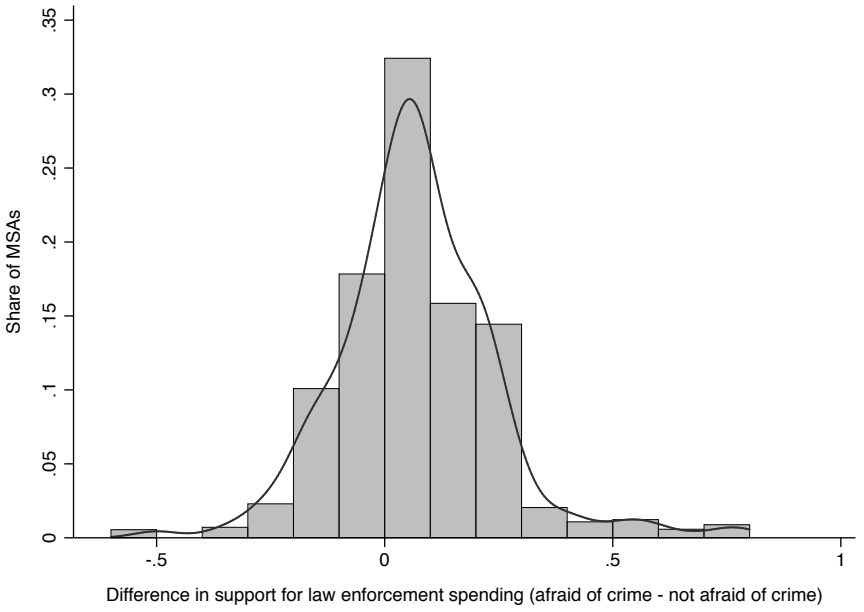
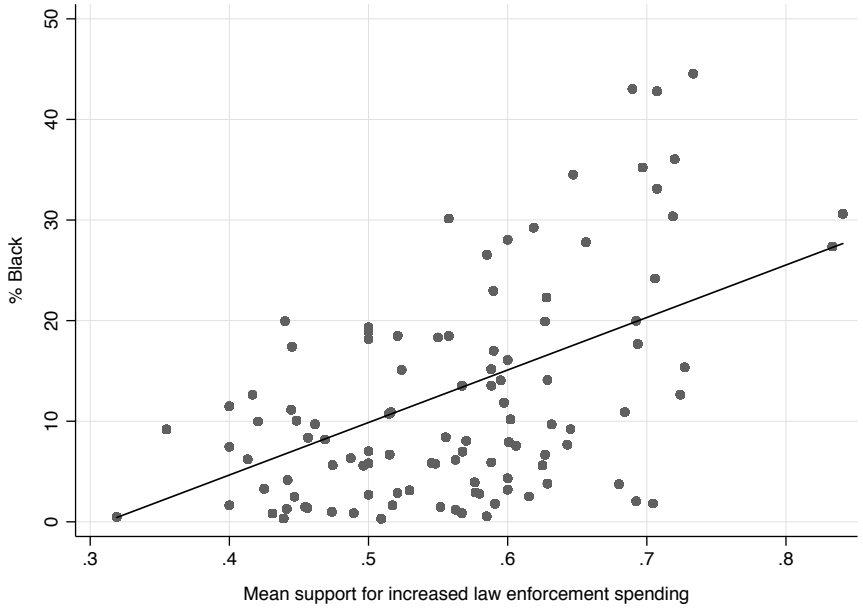


**Figure 4.11:** Support for law enforcement spending and black population




population is more diverse. Although purely descriptive, Figure 4.11 makes it clear that this expectation is plausible: the size of the black population in a locality is strongly correlated with aggregate level support for spending more on law enforcement ($r = 0.42, p < 0.01$). The least diverse MSA, Eau Claire, WI, is also the MSA with the smallest share of respondents in favour of increasing spending on law enforcement. Similarly, of the MSAs with more than 30% black inhabitants, all but one fall within the top decile of MSA in terms of the proportion of individuals favouring more law enforcement.

## 4.4 Results

The pages below proceed to analyse these patterns more systematically. To that end, the next section considers the relationship between MSA-level inequality and fear of crime. The subsequent parts of this chapter look at preferences: first for redistribution, and second for increased government spending on law enforcement. Taken as a whole, the empirical results lends considerable support for both sides of my argument. As in Europe, I find that local inequality is a significant

predictor of fear of crime, for the rich and the poor alike. More importantly, my key expectations regarding the role of heterogeneity are both clearly borne out: in racially homogeneous MSAs, fear of crime increases demand for redistribution but has no effect on support for increased law enforcement expenditures, whereas the opposite holds true in MSAs that are more diverse. More unexpectedly—but also in line with previous arguments regarding income and redistribution preferences in the US—I also find that the hypothesised joint effect of relative income, population heterogeneity and fear of crime on redistribution preferences is significantly larger in magnitude outside of the traditional South. As a consequence, the expectation that the effect of fear of crime on preferences increases with income—that is, Premise 3—is only borne out once the analysis is limited to the set of non-southern states. I discuss possible explanations for this result below.

### 4.4.1 Inequality and fear of crime

As precursor to the main analysis, Table 4.1 below begins by reporting the results from a series of regressions estimating the individual- and MSA-level determinants of fear of crime. My main interest lies in the effect of inequality. Again, although the relationship between inequality and fear of crime is not the main concern of this dissertation, demonstrating this connection empirically adds both relevance and substantive importance to my argument about fear of crime and preferences. Unlike what is the case in Europe, moreover, empirical studies of the relationship between regional inequality and fear of crime in the US, perhaps surprisingly, remain relatively sparse. Analysing whether this background expectation—that is, Premise 1: the conjecture that the local level of inequality has a positive effect on fear of crime—holds true is thus a useful first step.

To that end, Model 1 in Table 4.1 is a baseline specification, with the level of inequality as the only independent variable. Model 2 adds a number of individual-level controls that, as I have said above, have been found to matter for fear of crime in the previous literature: age and its square, gender, education, marital status, unemployment, household size, relative income, and a summary indicator of whether the respondent is black. Model 3 in turn adds MSA-level controls for real GDP per capita (using estimates from the Bureau of Economic Analysis), the annualised unemployment rate (calculated from county-level data from the Bureau of Labour Statistics), and the black population share. Model 4, finally, is iden-

**Table 4.1:** Determinants of fear of crime

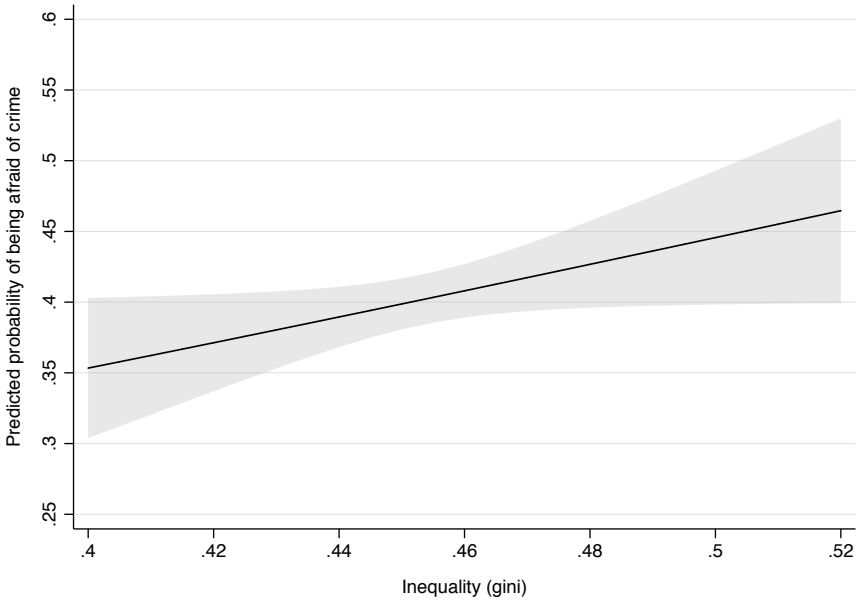
| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| Inequality | 7.167*** | 7.495*** | 4.350** | 4.350** |
| | (1.985) | (2.062) | (2.183) | (2.184) |
| Age | – | -0.019** | -0.017** | -0.017** |
| | | (0.008) | (0.008) | (0.008) |
| $Age^2$ | – | 0.000*** | 0.000*** | 0.000*** |
| | | (0.000) | (0.000) | (0.000) |
| Female | – | 1.165*** | 1.189*** | 1.189*** |
| | | (0.045) | (0.046) | (0.046) |
| Education | – | -0.001 | -0.003 | -0.003 |
| | | (0.008) | (0.008) | (0.008) |
| Married | – | -0.108** | -0.096* | -0.095* |
| | | (0.053) | (0.054) | (0.054) |
| Unemployed | – | -0.085 | -0.065 | -0.065 |
| | | (0.119) | (0.122) | (0.122) |
| Household size | – | -0.015 | -0.019 | -0.019 |
| | | (0.018) | (0.019) | (0.019) |
| Relative income | – | -0.050*** | -0.047*** | -0.071 |
| | | (0.005) | (0.006) | (0.094) |
| Black | – | 0.062 | 0.057 | 0.057 |
| | | (0.065) | (0.067) | (0.067) |
| % Black | – | – | 0.010** | 0.010** |
| | | | (0.004) | (0.004) |
| MSA GDP per capita | – | – | 0.000 | 0.000 |
| | | | (0.000) | (0.000) |
| MSA unemployment rate | – | – | 4.837** | 4.833** |
| | | | (2.194) | (2.194) |
| Inequality × Relative income | – | – | – | 0.051 |
| | | | | (0.204) |
| Constant | -3.411*** | -3.843*** | -3.053*** | -3.056*** |
| | (0.902) | (0.959) | (0.971) | (0.972) |
| BIC | 15,652 | 12,813 | 12,157 | 12,166 |
| Log-likelihood | -7,770 | -6,310 | -5,968 | -5,968 |
| Observations | 11,695 | 10,304 | 9,758 | 9,758 |
| MSAs | 111 | 111 | 107 | 107 |

Numbers are coefficients. Robust standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

tical to Model 3 but adds an interaction between inequality and relative income. All models are mixed-effects logit regressions with MSA-level random-effects and year fixed-effects, and are estimated with cluster robust standard errors.

Taken as a whole, these these results again bring few surprises. Paralleling the analysis of European data in Chapter 3, first, a number of the control variables are estimated to have significant effects on fear of crime. Both age and being a woman are associated with increased fear of crime, in line with the predictions from the vulnerability literature. These effects are furthermore quite large, especially that of gender: all else equal, the results from Model 3 imply that women are more than 25 percentage-points more likely than men to be afraid of crime. Likewise, the effect of relative income is again both statistically significant and negative, suggesting that fear of crime is more prevalent among the poor than it is among the more affluent. In terms of magnitude, the effect of income is

**Figure 4.12:** Predicted probability of being afraid of crime




*Note:* Average predicted probability of being afraid of crime at different levels of local inequality, with 95% confidence intervals. Calculations based on the results from Model 3 in Table 4.1. For reference, the discrete probability difference between the 10th (gini = 0.430) and 90th percentiles (gini = 0.494) of inequality is significant at the 95% confidence level ($\chi^2 = 3.94, p = 0.047$).

neither negligible nor huge in substantive terms: on average, the probability of being afraid of crime falls from 45.4% to 32.0% as relative income increases from the 10th to the 90th percentile, or equally, each additional $10,000 of household income is associated by a 1.1 percentage point decline in the likelihood of fearing crime. In contrast to the results derived using the ESS, finally, the interaction between relative income and inequality falls far short of statistical significance. On the macro-level, the most notable result concerns the moderately large, positive effect of MSA-level unemployment: net of the effect of inequality, moving from the 10th percentile (with an unemployment rate of 3.3%, roughly the level observed in the Atlanta-Sandy Springs-Marietta MSA in 1998) to the 90th percentile (7.9%, as seen in Pittsburgh in 2010) increases the average predicted probability of fear of crime by around six percentage-points.

More importantly, however, the effect of inequality is both positive—as expected—and statistically significant across all four models reported in Table 4.1. The size of the estimated effect of inequality is significantly reduced once the full set of macro-level controls are introduced in Model 3, yet it remains significant at the 95% confidence-level in all specifications. To provide a more intuitive illustration of this effect, Figure 4.12 plots the average predicted probability of being afraid of crime, with continuous variation in MSA-level inequality on the $x$-axis. The predicted probabilities confirm that while inequality appears to have a smaller effect on fear of crime than what was the case in the European data analysed in Chapter 3, the positive relationship remains quite notable. In the most equal MSAs—take Anchorage as an example, with a gini of 0.411—the average predicted probability of fear of crime is around 36.3%. In MSAs with an average level of inequality, such as the Nashville-Murfreesboro-Franklin MSA in Tennessee (with a gini of 0.459), by contrast, the same probability is just above 40.7%, and in the most unequal MSAs—take New York-Northern New Jersey-Long Island metropolitan area, with a gini of 0.503—it is 44.8%. Once again, therefore, the argument that inequality and fear of crime are positively related receives significant support from the analysis developed in this dissertation.

### 4.4.2 Demand for redistribution

Turning to the relationship between fear of crime and redistribution preferences, Table 4.2 presents the first set of regression results. Model 1 is a baseline mixed-

effects logistic regression model with MSA-level random intercepts, including only fear of crime, relative income and the percentage of black residents. Model 2 adds the full set of two- and three-way interaction terms between these variables. Model 3, finally, includes the full set of individual and macro-level controls (age, gender, marital status, education, religion, and state-level federal transfers). To save space, I only report the results for the substantive variables of interest and their interactions here. The full set of estimates, together with descriptive statistics, are included in Appendix B. All models are estimated with cluster corrected standard errors on the MSA-level, and include year fixed-effects.

A number of interesting results emerge from these estimates. For a start, a number of the control variables have significant effects on demand for redistribution. Being a woman is positively associated with redistribution preferences: all else equal, women are approximately four percentage-points more likely than men to agree with the statement that the federal government should reduce income differences. Religiosity, on the other hand, has a significant and comparatively large negative effect: moving from self-identifying as 'liberal' to 'fundamental' decreases the probability of supporting redistribution by about ten percentage-points. Like the findings from Chapter 3, these results are consistent with those derived in the previous comparative political economy literature.

Although these effects are interesting in their own right, they are however not the primary focus on this chapter. More important, as a start, is the effect of relative income. As discussed in Chapter 2, I argue that this variable—measuring the difference between a respondent's income and the national mean—captures the influence of material self-interest on demand for redistribution: to the extent that redistribution works around the mean, as in the RMR-model, it will provide a net benefit for those with below-mean earnings but impose a net cost on individuals earning more than the mean. Hence, if income-maximisation matters for the demand for redistribution, we should observe a negative relationship between relative income and preferences. It is this reasoning that informs Premise 2, as the expectation that support for redistribution will decline as relative income increases.

The results reported in Table 4.2 lend considerable support for this baseline expectation: relative income and demand for redistribution are inversely related. Across all three models, the estimated effect of relative income on revealed redistribution preferences is both negative—implying that as relative income increases, the likelihood of supporting redistribution declines—and statistically significant

**Table 4.2:** Determinants of redistribution preferences (including the South)

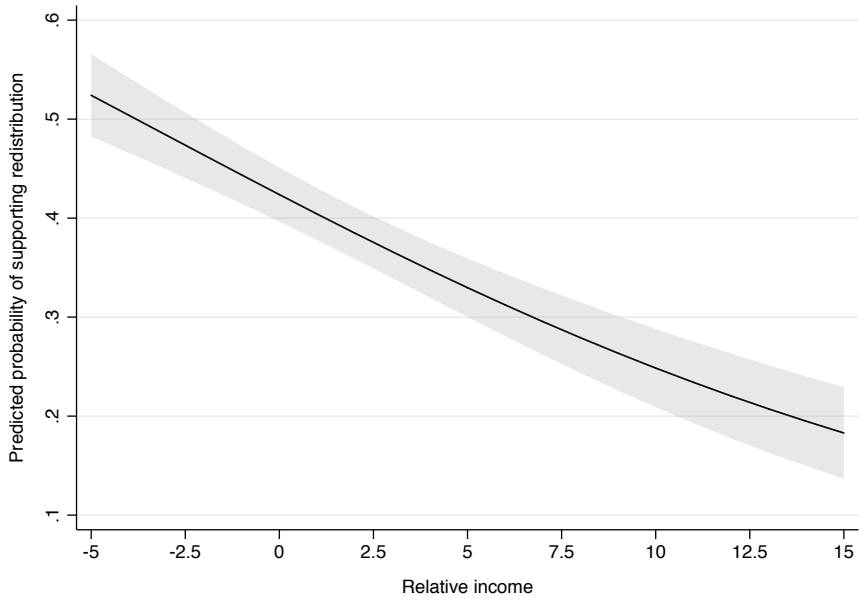
| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| Relative income | -0.074*** | -0.088*** | -0.102*** |
| | (0.011) | (0.023) | (0.025) |
| Fear of crime | 0.381*** | 0.552*** | 0.478*** |
| | (0.096) | (0.156) | (0.165) |
| % Black | -0.013** | -0.009 | -0.012* |
| | (0.006) | (0.006) | (0.007) |
| Fear of crime × Relative income | – | 0.032 | 0.046 |
| | | (0.036) | (0.037) |
| Relative income × % Black | – | 0.001 | 0.002 |
| | | (0.002) | (0.002) |
| Fear of crime × % Black | – | -0.013 | -0.011 |
| | | (0.012) | (0.012) |
| Fear of crime × Relative income × % Black | – | -0.003 | -0.004* |
| | | (0.002) | (0.002) |
| Constant | -0.303** | -0.361*** | -0.571 |
| | (0.127) | (0.124) | (0.706) |
| Controls | No | No | Yes |
| Year fixed-effects | Yes | Yes | Yes |
| BIC | 2,291 | 2,318 | 2,247 |
| Log-likelihood | −1,108 | −1,107 | −1,042 |
| Observations | 1,702 | 1,702 | 1,628 |
| MSAs | 111 | 111 | 111 |

Numbers are coefficients. Robust standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Full set of estimates included in Table B.1 in Appendix B.

at the 99% confidence-level. To provide a more intuitive illustration of this effect, Figure 4.13 plots the average predicted probability of supporting redistribution at different income levels, using the results from Model 3. As above, average predicted probabilities are calculated by holding all covariates in the model at their observed values and then averaging those predictions, unlike 'traditional' or probabilities derived holding the other variables in the model at some constant value.

The results in Figure 4.13 make it very clear that relative income matters: support for redistribution is the highest among those with the lowest incomes, and then steadily declines as relative income increases. This effect is furthermore quite large in magnitude. At the 10th percentile of the income distribution, corresponding to a yearly income of $51,900 less than the mean, the probability of supporting redistribution is just below 53%. For an individual in the 25th percentile ($38,560 below the mean) the same probability is 50%, and at the mean (where relative income is zero by definition) it is just about 43%. Moving

**Figure 4.13:** Predicted probability of supporting redistribution




*Note:* Average predicted probability of supporting redistribution with 95% confidence intervals. Calculations based on the results from Model 3 in Table 4.2. Relative income (on the $x$-axis) is in units of constant $10,000 with base year 2010. In all years, 0 represents the national mean.

up the income distribution, support for redistribution then declines rapidly: at the 75th percentile ($23,700 above the mean), the predicted probability is 38%, in the 90th percentile ($89,780 above the mean) it is 26%, and in the 95th percentile ($119,690 above the mean) it is 22%. Thus, the argument that tax-and-transfer concerns form an important driver of individual redistribution preferences—in the US as in Europe, and as implied by the standard RMR-model and Premise 2—receives a great deal of support from the analysis in this chapter.

It is also clear, however, that such concerns are not all matter for redistribution preferences. Two observations, in particular, help to make this point. Although it is undeniably the case that demand for redistribution declines with income, it is also true—like in Europe—that support for redistribution remains decisively non-negligible even among income-groups that do not have a plausible tax-and-transfer motive for demanding such policies. As shown in Figure 4.13, for example, even in the top decline of the income distribution, between one-fifth and one-quarter of Americans agree the government should reduce income differences. Second, and relatedly, the relationship between relative income and support for redistribution also exhibits a significant amount of geographical variation. On a descriptive level, Figure 4.6 above has already hinted at this, and shown that there is considerable between-MSA variation in the extent to which the redistribution preferences of the affluent are similar to those of the poor. If redistribution preferences are only about material self-interest, these observations are indeed difficult to account for.

In Chapter 2, I have argued that a possible explanation for these patterns—support for redistribution from the affluent, and cross-sectional variation in the extent to which this is the case—has to do with the relationship between inequality and fear of crime, first, and that between fear of crime and demand for redistribution, second. The second part is the focus of the analysis developed here: the argument that although fear of crime can have a positive effect on support for redistribution, this effect will be contingent on the social identity of the poor (Hypothesis 1), and increase in magnitude with relative income (Premise 3). Applying this logic to the US, it thus follows that I expect to find a positive effect of fear of crime on support for redistribution among affluent whites in homogenous contexts, but no parallel effect in MSAs that are more racially diverse.

To assess this argument, Model 2 and Model 3 in Table 4.2 include a three-way interaction term between relative income, fear of crime, and the size of

the black population in the MSA, together with the three constituent two-way interactions between these variables and their main effects. These terms are challenging to interpret directly. To provide a more intuitive illustration of the results, therefore, Figure 4.14 and Figure 4.15 plot the average predicted probability of supporting redistribution, conditional on fear of crime, relative income and population heterogeneity, together with 95% confidence intervals for the estimates. The probabilities in Figure 4.14 are calculated holding the percentage of black residents in the MSA constant at 1.6% (representing the 10th percentile), while those in Figure 4.15 hold the same variable constant at 27.4% (90th percentile). For reference, the former roughly corresponds to the value observed for the Portland-Vancouver-Hillsboro MSA in 2010, while the latter is represented by the Atlanta-Sandy Springs-Marietta MSA in 1998. Neither of these values are close to the extremes in the sample. Both sets of probabilities are calculated using the results from Model 3, which includes MSA-level random effects and the full set of individual- and macro-level control variables.

Figure 4.14 and Figure 4.15 reveal several interesting findings. Most importantly given my argument, they show that fear of crime matters for demand for redistribution, and that this effect depends both on the level of population heterogeneity and on income. For the affluent, the results are very much in line with expectations. As shown in Figure 4.14, when the level of heterogeneity is low, the probability of supporting redistribution is higher among those who are afraid of crime than among those who are not afraid, and the difference between the these groups increases with relative income. Crime matters little to the preferences of the poor, but makes much more of a difference to those of the affluent. For the latter, this effect is reasonably large: at the 90th income percentile ($89,780 above the mean), for example, fear of crime increases the probability of supporting redistribution from 23.1% to 41.1%. At the 10th percentile ($51,900 below the mean), on the other hand, the same difference is less than six percentage-points. At high levels of heterogeneity, by contrast, a very different picture emerges. For the affluent, the estimated probability of supporting redistribution is lower for those who are afraid of crime than it is for those who feel safe. More surprisingly, fear of crime has the opposite effect on the preferences of the poor. While more pronounced in magnitude, it is notable that these patterns are remarkably similar to those derived using European data in Chapter 3 (see especially Figure 3.12 and Figure 3.13 on page 108).

**Figure 4.14:** Predicted probability of supporting redistribution, 1.6% black

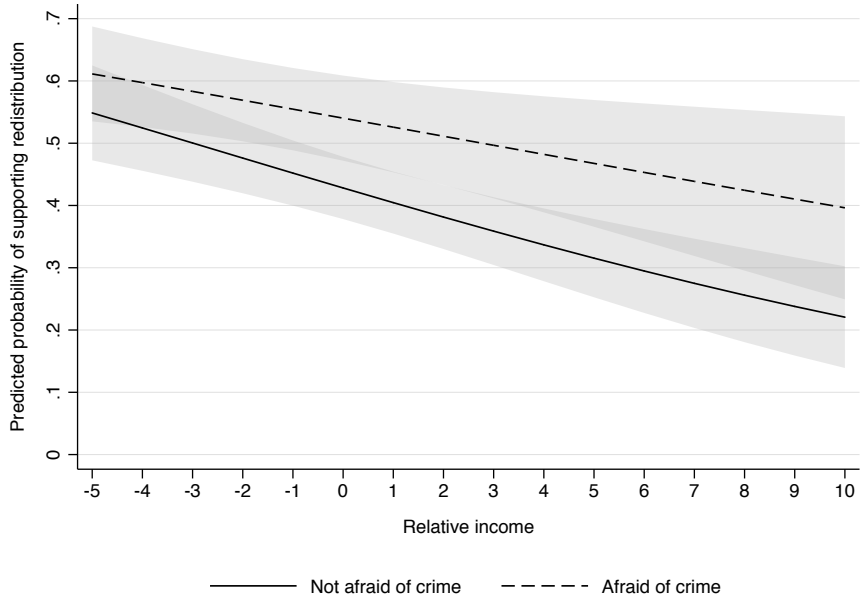
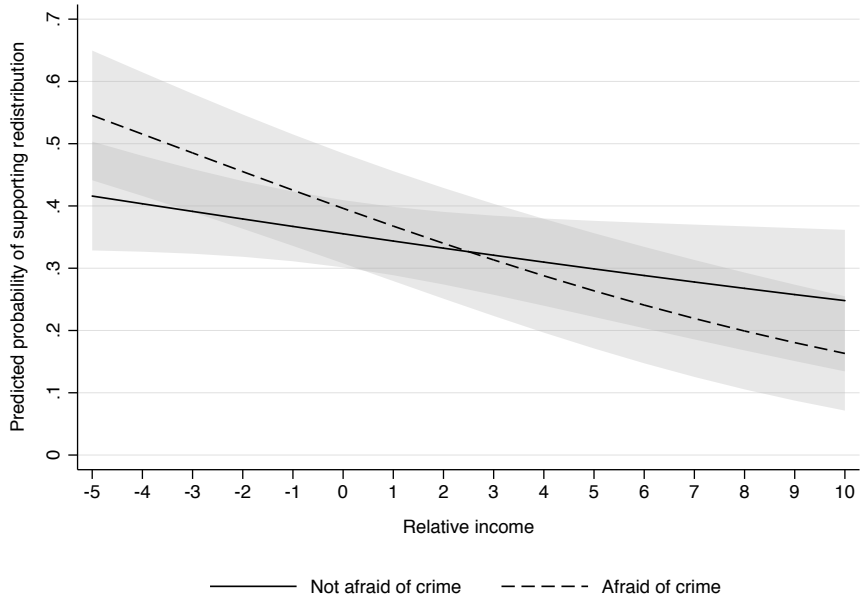



**Figure 4.15:** Predicted probability of supporting redistribution, 27.4% black



To assess the statistical significance of these differences, I proceed to estimate corresponding sets of average marginal effects. Recall that although non-overlapping confidence intervals of predicted probabilities does imply significance, the opposite is not true: overlap only means that the least likely value of one estimate overlaps with the least likely value of the other. Given that the likelihood that both estimates are at the extreme end of their respective confidence intervals is lower than the likelihood that only one of them is, it follows that confidence intervals can overlap even though the difference between the estimated probabilities is itself significant. Hence, the correct way to assess whether the effect of fear of crime is indeed statistically significant is through marginal effects. Similarly to the average predicted probabilities discussed above, moreover, average marginal effects are derived by first calculating marginal effects while holding all covariates in the model at their observed values, and then averaging those prediction to produce a single average marginal effect. Since fear of crime is dichotomous, the resulting estimates carry a straightforward interpretation as the predicted change in the probability of supporting redistribution associated with a change from not being afraid of crime to being afraid of crime. Positive marginal effects thus implies that fear of crime has a positive effect on preferences, while negative marginal effects imply the opposite.

With that said, Figure 4.16 and Figure 4.17 below plot the average marginal effect of fear of crime on support for redistribution at different levels of relative income, together with 95% confidence intervals for the predictions. The marginal effects in Figure 4.16 are calculated with the share of black residents in the MSA held constant at 1.6% (10th percentile, as for the predicted probabilities in Figure 4.14), while those in Figure 4.17 hold the same variable constant at 27.4% (90th percentile, as in Figure 4.15). With regards to my expectations, these results paint a mixed picture. On the one hand, the effect of fear of crime on demand for redistribution clearly depends on the level of population heterogeneity. In MSAs with a comparatively low share of black residents, fear of crime is associated with a statistically significant increase in support for redistribution for all but those with the very lowest incomes. In MSAs that are more diverse, by contrast, the outcome is the opposite: fear of crime has a positive effect on preferences only for a small subset of individuals at the bottom of the income distribution, but does not otherwise affect redistribution preferences. These results thus corroborate my expectation that the relationship between fear of crime

**Figure 4.16:** Marginal effect of fear of crime on redistribution preferences, 1.6% black

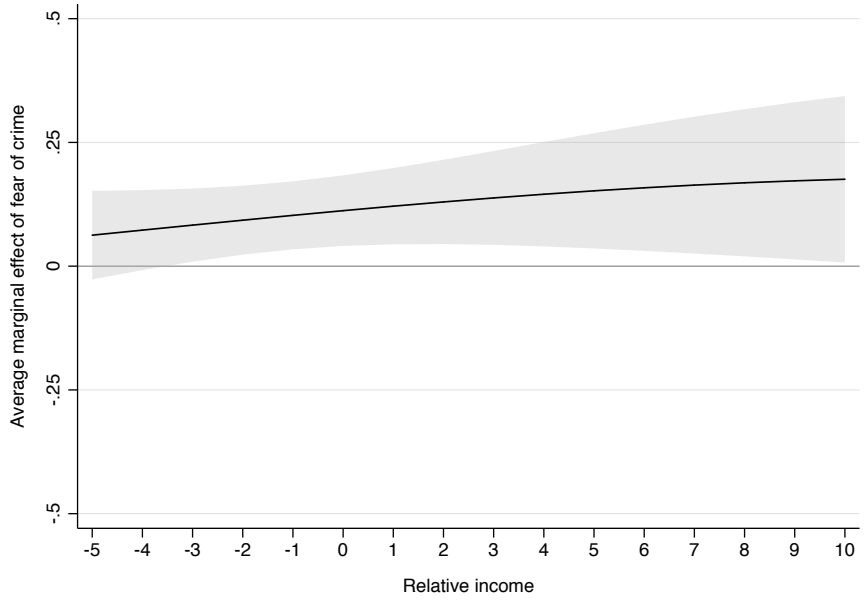
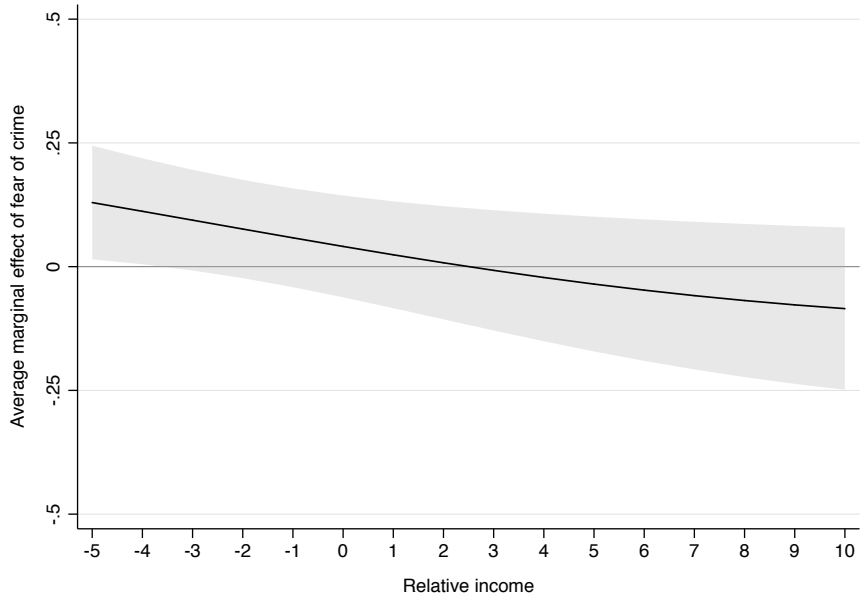



**Figure 4.17:** Marginal effect of fear of crime on redistribution preferences, 27.4% black



and demand for redistribution depends on heterogeneity. On the other hand, the marginal effects shown in Figure 4.16 also reveal that although the point estimate for the effect of fear of crime on preferences increases in magnitude with relative income, this increase is itself not statistically significant. On the whole, therefore, although these results confirm Hypothesis 1—as the expectation that fear of crime has a positive effect on preferences, conditional on the level of population heterogeneity—they fail to do the same for Premise 3, suggesting that this effect should increase in magnitude with relative income.

It is possible, however, that this null result is at least in part driven by Southern exceptionalism. The political distinctiveness of the South is well documented, both in the general literature on political behaviour and in political economy research on the relationship between income and partisanship (for an excellent discussion of these issues, see Hersh and Nall, 2016). A common conclusion in the latter literature is that the income-partisanship relationship—in practice, the tendency for the poor to vote Democrat and for the rich to vote Republican—is stronger in the South than in the non-South, although the explanation for this observation varies, as discussed above. Gelman (2010), for example, emphasises state income as the key mediating variable, while Hersh and Nall (2016) point to the importance of the combination of South's history of racial inequality, late democratisation and local levels racial heterogeneity. In related work, Rueda and Stegmueller (2019) also find that although income matters to redistribution preferences both in and outside of the Southern states, the preferences of the affluent exhibit less variation conditional on inequality than is the case elsewhere. Thus, whatever the reason for these observations, they suggest that it may be useful to examine the patterns discussed above while excluding the South.

To that end, Table 4.3 replicates the analysis presented in Table 4.2, but excludes respondents from the South. Following the Census Bureau classification, I define the South as Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia. This reduces the sample size at both the individual- and the MSA-level by about one-third. As before, all regressions reported in Table 4.2 are mixed effects logit models with MSA-level random intercepts, together with year fixed-effects. Model 1 includes only fear of crime, relative income and the percentage of black residents in the MSA as independent variables, and Model 2 adds the battery of two- and three-way in-

**Table 4.3:** Determinants of redistribution preferences (excluding the South)

| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| Relative income | -0.065*** | -0.131*** | -0.154*** |
| | (0.011) | (0.028) | (0.030) |
| Fear of crime | 0.512*** | 0.629*** | 0.600*** |
| | (0.113) | (0.187) | (0.191) |
| % Black | -0.001 | -0.003 | -0.013 |
| | (0.011) | (0.014) | (0.023) |
| Fear of crime × Relative income | – | 0.097** | 0.137*** |
| | | (0.044) | (0.043) |
| Relative income × % Black | – | 0.006*** | 0.008*** |
| | | (0.002) | (0.002) |
| Fear of crime × % Black | – | -0.007 | -0.006 |
| | | (0.018) | (0.017) |
| Fear of crime × Relative income × % Black | – | -0.009*** | -0.013*** |
| | | (0.003) | (0.003) |
| Constant | -0.404*** | -0.421*** | -0.435 |
| | (0.138) | (0.152) | (1.059) |
| Controls | No | No | Yes |
| Year fixed-effects | Yes | Yes | Yes |
| BIC | 1,610 | 1,628 | 1,578 |
| Log-likelihood | −770 | −765 | −712 |
| Observations | 1,171 | 1,171 | 1,113 |
| MSAs | 71 | 71 | 71 |

Numbers are coefficients. Robust standard errors in parentheses.
Two-tailed significance tests: *** $p$ <0.01, ** $p$ <0.05, * $p$ <0.1
Full set of estimates included in Table B.2 in Appendix B.

teraction terms. Model 3, finally, includes both the interactions and the controls.

Once again, several of the control variables have significant effects on support for redistribution: being female has a positive effect, while the effect of religiosity is negative. More importantly, however, the effect of relative income remains both negative and highly statistically significant, confirming that the finding discussed above with regards to the influence of material self-interest on redistribution preferences is not limited to the Southern states. The size of this effect is marginally smaller than when the South is included in the sample, but nonetheless remains comparatively large: moving from the 10th to the 90th percentile decreases the likelihood supporting redistribution by just above 26 percentage-points. Crucially, finally, the three-way interaction term between fear of crime, relative income and the black population share likewise remains negative, but is now statistically significant at the 99%-level in both Model 2 (without controls) and Model 3 (with controls). While this result is again difficult to interpret based on the estimated in Table 4.3 alone, it is notable that the magnitude of this effect is considerably larger than before.

To provide a more intuitive illustration of this interaction effect, I again turn to average predicted probabilities: Figure 4.18 and Figure 4.19 below show the average predicted probability of supporting redistribution conditional on fear of crime at different levels of relative income, together with 95% confidence intervals for the prediction. The probabilities in Figure 4.18 are calculated with the percentage of black residents in the MSA held constant at 1.2% (corresponding to the 10th percentile in the distribution of MSAs outside of the South), while those in Figure 4.19 hold the same variable constant at 18.7% (90th percentile).

Two important points should be noted about these estimates. The first is that although the direction of all the main effects remain unchanged, they are much larger in magnitude when the analysis is limited to the non-Southern states. This is particularly evident with respect to the effect of fear of crime on the redistribution preferences of the affluent in low heterogeneity contexts, as shown in Figure 4.18 (compare Figure 4.14). Outside of the South, the preferences of the rich exhibit much greater variation conditional on fear of crime. This result is consistent with the literature on Southern exceptionalism discussed above (see especially Hersh and Nall, 2016, and Rueda and Stegmueller, 2018).

Secondly, these results again align closely with my theoretical expectations: fear of crime increases demand for redistribution among high-income voters

**Figure 4.18:** Predicted probability of supporting redistribution, 1.2% black

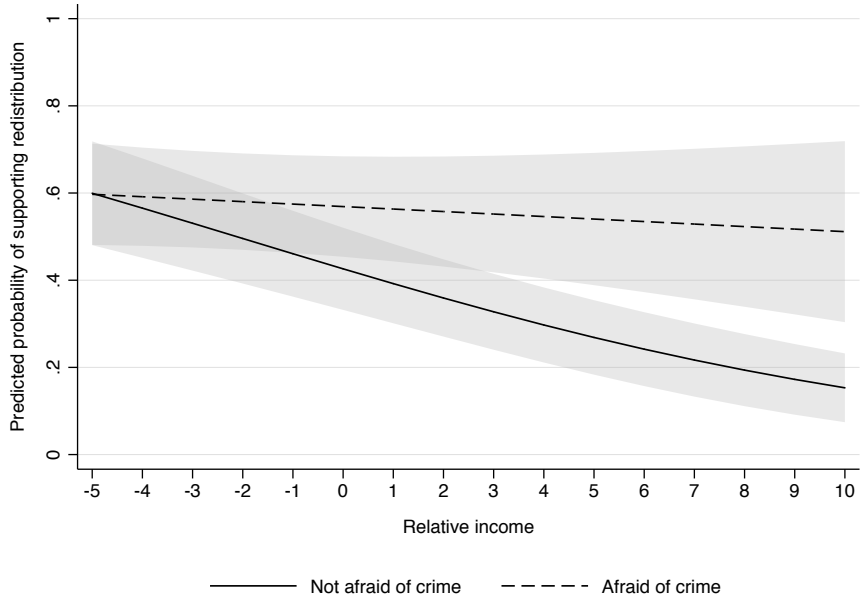
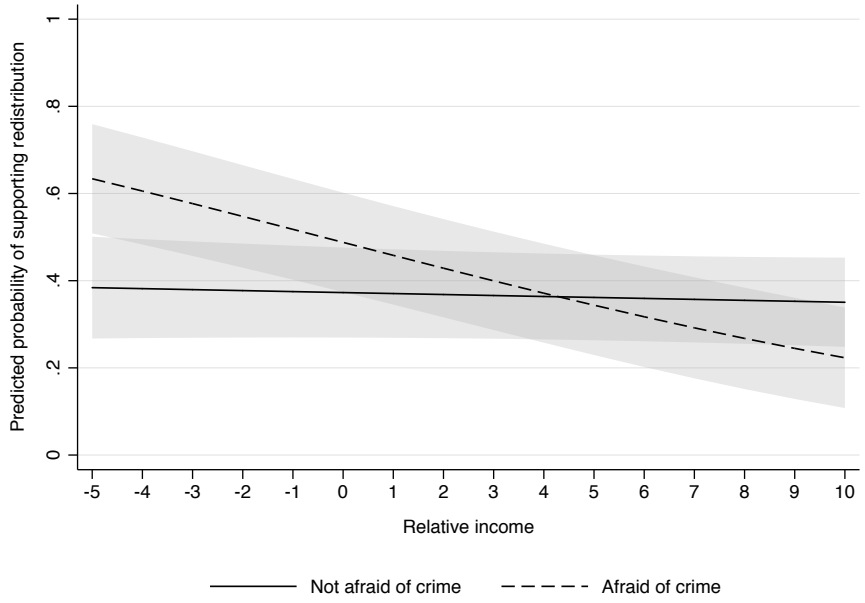



**Figure 4.19:** Predicted probability of supporting redistribution, 18.6% black



provided the level of heterogeneity is sufficiently low, but has no similar effect in places that are more diverse. In low-heterogeneity MSAs, affluent individuals who are afraid of crime are considerably more likely to support redistribution compared to those with similar incomes who feel safe. As a consequence, fear of crime has a distinctly moderating effect on the relationship between income and redistribution preferences: the negative effect of income on support for redistribution is much smaller—and, in fact, statistically insignificant—for those who are afraid of crime. The magnitude of this effect is thus large: at the 90th percentile of the income distribution ($89,780 above the mean), the predicted probability of supporting redistribution for an individual who feels safe is 17.3%, compared to 51.7% for someone who is afraid of crime—a difference of nearly 35 percentage-points. At the 10th percentile (again, $51,900 below the mean), by comparison, the same difference is less than one percentage-point. In high-heterogeneity MSAs, on the other hand, the pattern is again very different. Among the affluent, the probability of supporting redistribution is lower, rather than higher, for those who are afraid of crime than for those who are not. As before, moreover, crime also appears to have a positive effect on the redistribution preferences of low-income individuals in contexts of high heterogeneity.

To assess the significance of these differences, I again proceed to estimate corresponding sets of average marginal effects. These are shown in Figure 4.20 and Figure 4.21. Following the same logic as before, the marginal effects in Figure 4.20 are calculated with the percentage of black residents in the MSA set at 1.2% (10th percentile outside of the South), while those in Figure 4.21 hold the same variable constant at 18.6% (90th percentile). These results largely substantiate the points discussed above. As in the analysis of the full sample, fear of crime has a significant positive effect on support for redistribution among the affluent when the level of population heterogeneity remains low, but no similar effect on preferences in metropolitan regions that are more diverse. In the former case, the magnitude of the positive effect of fear of crime increases—now significantly—with income, and is statistically significant at the 95% confidence-level for individuals with incomes $22,000 below the mean (corresponding to the 42nd income percentile) or more, despite the now considerably smaller sample size. At high levels of heterogeneity, on the other hand, fear of crime either has no, or at the highest income levels (89th percentile and above), a negative effect on the preferences of the affluent. Moreover, the positive effect of fear of crime on support for

**Figure 4.20:** Marginal effect of fear of crime on redistribution preferences, 1.2% black

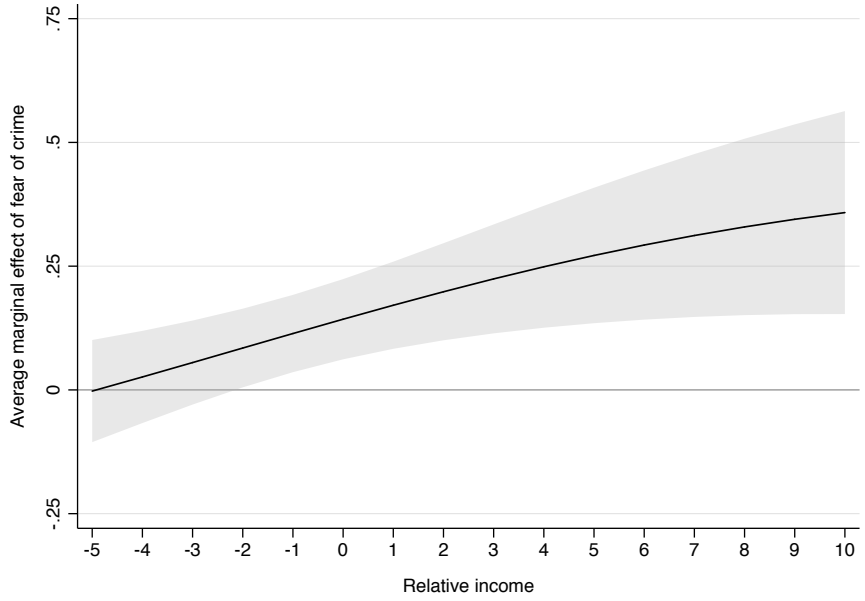
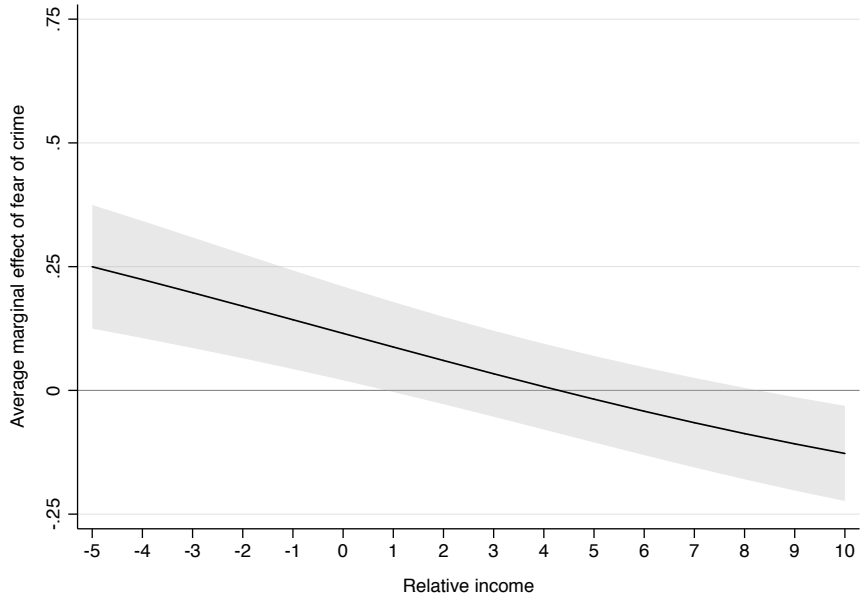



**Figure 4.21:** Marginal effect of fear of crime on redistribution preferences, 18.6% black



redistribution among the poor in high heterogeneity contexts—as noted in the analysis of the full sample above—also persists. While this latter finding is unexpected, the results for the affluent are clearly in line with this side of my argument: fear of crime increases support for redistribution among high-income individuals in homogenous contexts, but has no similar effect in places that are more diverse.

### 4.4.3 Demand for law enforcement spending

Taken together, the results derived above thus provide considerable support for the theoretical argument developed in Chapter 2. Both with and without the South, heterogeneity significantly affects the relationship between fear of crime and the redistribution preferences, in line with the implications of my argument. When the South is excluded from the sample, the magnitude of this effect also significantly increases with income. It is also comparatively large: outside of the South and holding the share of black residents in the MSA constant at 1.2% (as before, 10th percentile), fear of crime increases the probability that an individual in the 90th percentile of the income distribution support redistribution by no less than 34.4 percentage-points. As the level of population heterogeneity increases, this effect first diminishes in size and drops out of statistical significance, and then becomes negative and significant at sufficiently high income levels. In all specifications, finally, relative income has a significant negative effect on redistribution preferences, while fear of crime has a positive effect on support for redistribution among the poor when the level of heterogeneity is high. As such, neither Premise 2 (about relative income) nor Hypothesis 1 (about population heterogeneity) can be rejected based on these results. The same does not hold true for Premise 3 (suggesting that the importance of fear of crime to preferences increases with income), however, since fear of crime does have a significant positive effect on the preferences of low-income individuals in low heterogeneity settings.

With this result in hand, the remainder of this section considers the other side of my theoretical argument: the relationship between fear of crime, racial heterogeneity, and support for increased spending on law enforcement. As discussed in some detail in Chapter 2, I argue that population heterogeneity increases the prevalence of dispositional explanations of criminal behaviour, and hence lessens the extent to which affluent individuals view crime as causally related to economic inequality. Consequently, increasing heterogeneity should weaken the

**Table 4.4:** Determinants of law enforcement preferences

| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| Fear of crime | 0.219** | 0.205 | 0.247 |
| | (0.108) | (0.169) | (0.184) |
| % Black | 0.016*** | 0.018*** | 0.023*** |
| | (0.005) | (0.006) | (0.007) |
| Fear of crime × Relative income | – | 0.009 | 0.003 |
| | | (0.033) | (0.035) |
| Relative income × % Black | – | -0.003*** | -0.003*** |
| | | (0.001) | (0.001) |
| Fear of crime × % Black | – | -0.001 | -0.003 |
| | | (0.010) | (0.010) |
| Fear of crime × Relative income × % Black | – | 0.003 | 0.003 |
| | | (0.002) | (0.002) |
| Constant | -0.300** | -0.292* | -0.689 |
| | (0.149) | (0.159) | (0.684) |
| Controls | No | No | Yes |
| Year fixed-effects | Yes | Yes | Yes |
| BIC | 2,286 | 2,305 | 2,231 |
| Log-likelihood | −1,106 | −1,101 | −1,035 |
| Observations | 1,621 | 1,621 | 1,552 |
| MSAs | 110 | 110 | 110 |

Numbers are coefficients. Robust standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Full set of estimates included in Table B.3 in Appendix B.

link between fear of crime and demand for redistribution, but strengthen the relationship between fear of crime and punitive attitudes. If this argument is correct, therefore, fear of crime should be positively associated with support for increased law enforcement spending in high-heterogeneity MSAs, but have no similar effect on preferences in places that are more racially diverse.

To test these expectations, I again estimate a series of mixed-effects logit regressions with MSA-level random intercepts and year fixed-effects. These are reported in Table 4.4. As discussed in Section 4.2.1, the dependent variable in these models measures whether a respondent thinks the government spends 'too little' on law enforcement, rather than 'too much' or the right amount. Model 1 is again a baseline specification, including only the substantive variables (fear of crime, relative income and the percentage of black residents in the MSA) but no interactions or controls. Model 2 in turn adds the three two-way and one three-way interaction terms between these variables. Model 3, finally, includes both the interactions and the full battery of individual- and macro-level controls.

Once again, several interesting points emerge from these results. Although the joint effect of fear of crime, relative income and population heterogeneity is difficult to decipher from Table 4.4 directly, several of the control variables are estimated to have significant effects on law enforcement preferences (see Table B.3 in Appendix B for the full set of coefficient estimates). All else equal, the effect of religiosity is positive and comparatively large in magnitude: moving from self-identifying as religiously 'liberal' to 'fundamental' increases the probability of supporting increased law enforcement spending more than 7.8 percentage-points. The same holds true for age. Education, on the other hand, has a relatively large and highly significant negative effect: at the margin, each additional year of full-time schooling decreases the likelihood of thinking that law enforcement spending should increase by approximately 1.3 percentage-points, meaning that college graduates are about five percentage-points less likely to do so than high-school graduates. With the exception of the insignificance of gender, these results are consistent with those in the previous literature.

Turning to the relationship between fear of crime, population heterogeneity and relative income, Figure 4.22 and Figure 4.23 plot the average predicted probability of supporting increased law enforcement expenditures, conditional on fear of crime for the range of observed values of relative income (on the $x$-axis). As in the analysis of redistribution preferences, the probabilities shown

**Figure 4.22:** Predicted probability of supporting more law enforcement spending, 1.6% black

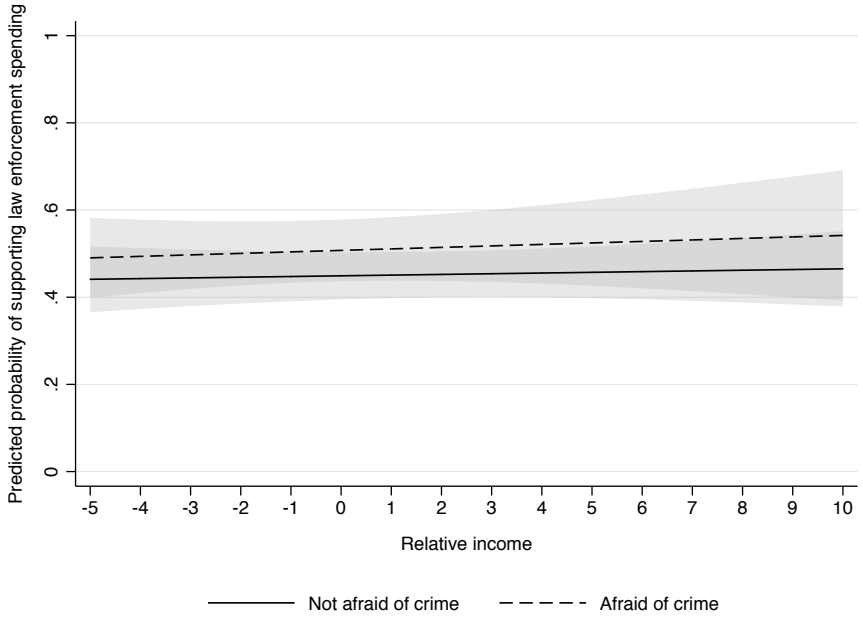
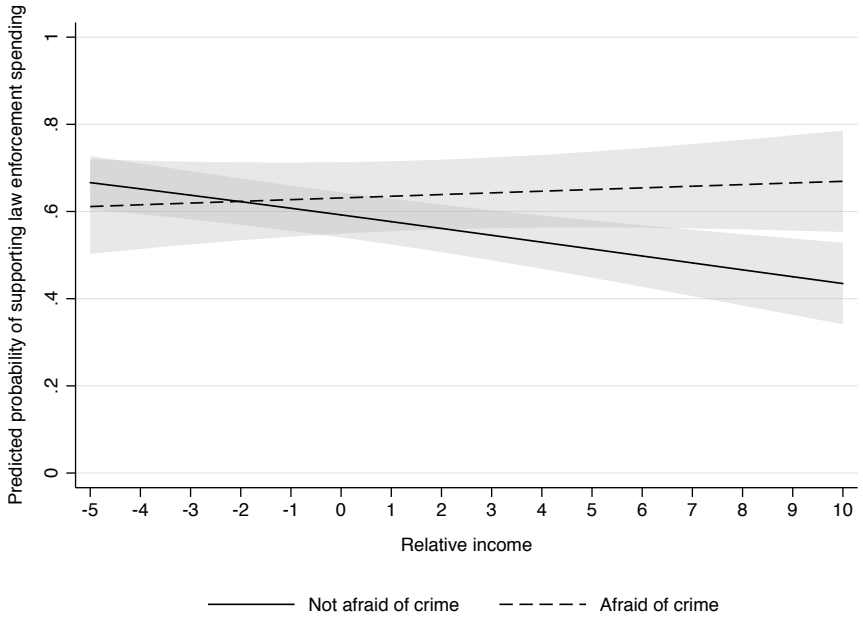



**Figure 4.23:** Predicted probability of supporting more law enforcement spending, 27.4% black



in Figure 4.22 are calculated with the share of black residents in the MSA held constant at 1.6% (approximately the 10th percentile), while those presented in Figure 4.23 hold the same variable constant at 27.4% (90th percentile).

These results are highly consistent with my theoretical expectations: among the affluent, fear of crime increases demand for increased law enforcement spending when the level of population heterogeneity is high, but makes much less of a difference to preferences in settings that are less diverse. At the 90th percentile of the income distribution (corresponding to an annual income of $89,780 more than the mean) and holding the percentage of black residents constant at 1.6% (as in Figure 4.22), for example, the probability of supporting increased law enforcement spending is about 46% for an individual who feels safe, and 54% for someone who is afraid of crime. In the high-heterogeneity setting modelled in Figure 4.23, by contrast, the corresponding probabilities are 45% and 67%—a difference more than twice as large. For the poor, moreover, fear of crime never makes a large difference to demand for law enforcement, irrespective of heterogeneity. These results are in line both expectations: the relationship between fear of crime and punitive attitudes depends on the level of population heterogeneity, and that the importance of fear of crime to preferences increases with income.

Moving on, Figure 4.24 and Figure 4.25 below plot the corresponding sets of average marginal effects of fear of crime, together with 95% confidence intervals for the estimates. As before, the marginal effects shown in Figure 4.24 are calculated with the heterogeneity held variable constant at 1.6%, while those in Figure 4.25 hold the same variable constant at 27.4%. Since the dependent variable is again binary, these estimates can be straightforwardly interpreted as the predicted change in the likelihood of supporting more law enforcement spending associated with moving from 'feeling safe' to being afraid of crime.

Taken as a whole, these results confirm the significance (and, in contexts of low heterogeneity, insignificance) of the patterns discussed above. When the level of local population heterogeneity remains low, as shown in Figure 4.24, the effect of fear of crime on law enforcement preferences is not significantly different from zero at any level of relative income. At high levels of heterogeneity, on the other hand, the effect of fear of crime on preferences both increases in magnitude with relative income, and is statistically significant for individuals earning more than approximately $24,000 above mean income. The implications of these results are clear: fear of crime never makes a difference to the

**Figure 4.24:** Marginal effect of fear of crime on law enforcement preferences, 1.6% black

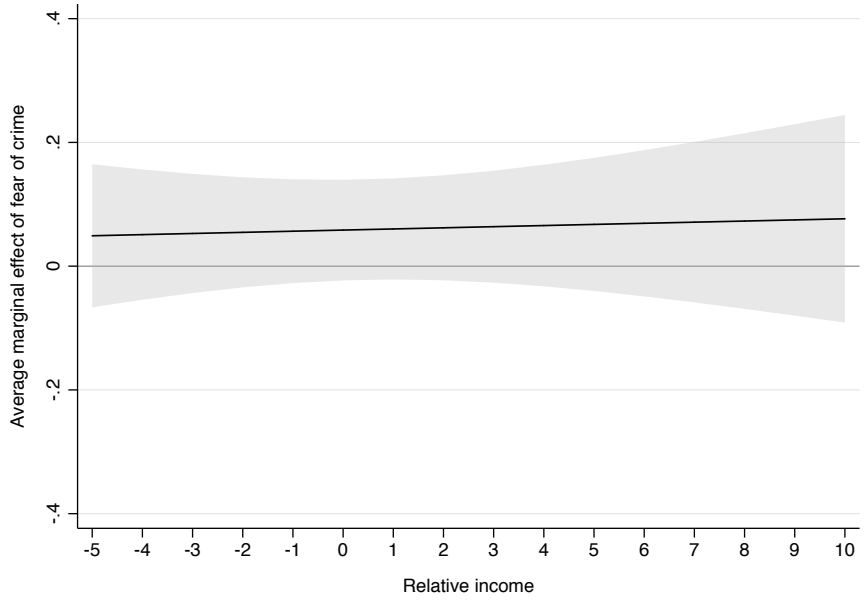
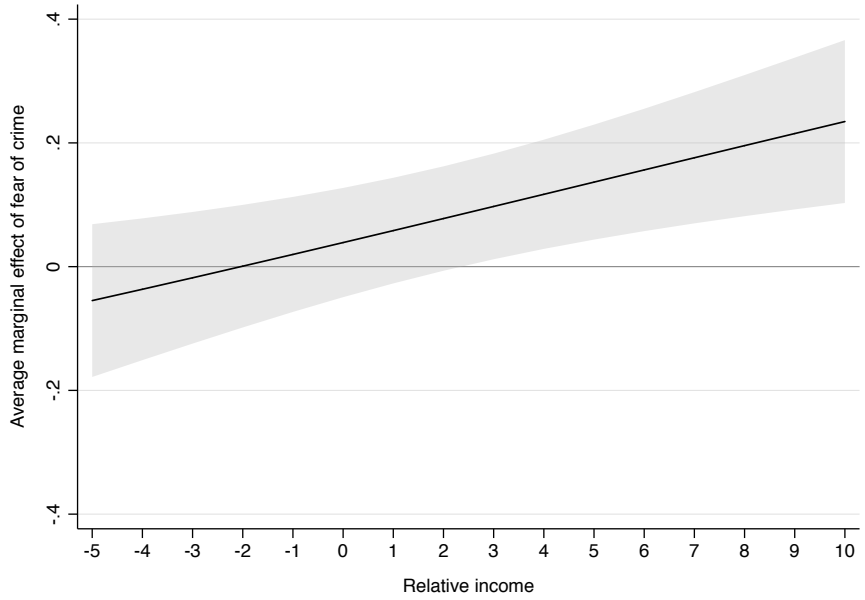



**Figure 4.25:** Marginal effect of fear of crime on law enforcement preferences, 27.4% black



law enforcement preferences of low-income individuals, and has a positive effect on the preferences of the affluent only in settings that are sufficiently diverse, and not in places that are more homogenous—the opposite result from that derived in the preceding analysis of redistribution preferences.

## 4.5 Robustness tests

In this last empirical section, I present a series of robustness tests of the results derived over the previous pages. These tests aim to check the sensitivity of my results to alternative theoretical explanations and statistical modelling choices. Section 4.5.1 first focuses on the analysis of redistribution preferences, while Section 4.5.2 centres on demand for law enforcement. To simplify presentation of these results, I do not present full regression tables but instead focus on selected but theoretically relevant marginal effects. To that end, Table 4.5 (for redistribution preferences) and Table 4.6 (for law enforcement preferences) present the estimated average marginal effect of fear of crime on preferences for an individual in the 90th percentile of the income distribution, at low (10th percentile of the relevant variable) and high (90th percentile) levels of population heterogeneity. Recall that if my argument is correct, fear of crime should have a positive effect on demand for redistribution only when the level of heterogeneity remains low, while the opposite should hold true for the relationship between fear of crime and support for increased law enforcement spending. Given the results derived above, for all tests, I report two results: one based on the entire sample of respondents, and one where individuals residing in South are excluded. Unless otherwise noted, all reported marginal effects are estimated using the result from the same random-intercept logit model used in the main analysis, including the full batteries of individual- and macro-level controls.

### 4.5.1 Redistribution preferences

The robustness tests for the redistribution side of my argument are shown in Table 4.5 below. I begin by including a set of additional individual-level control variables that were left out of the main analysis to keep the models parsimonious: political ideology, religious denomination, social class, work status, and an indicator of whether the respondent lives in the urban core of the MSA. Sec-

**Table 4.5:** Marginal effect of fear of crime on redistribution preferences

| | Including the South | | Excluding the South | |
|---|---|---|---|---|
| Heterogeneity: | Low | High | Low | High |
| Main analysis | 0.172** | −0.077 | 0.344*** | −0.108** |
| | (0.033) | (0.344) | (0.000) | (0.025) |
| Additional individual-level controls | | | | |
| *Test 1.1:* Ideology | 0.148* | −0.089 | 0.299*** | −0.107** |
| | (0.055) | (0.286) | (0.001) | (0.044) |
| *Test 1.2:* Denomination | 0.174** | −0.078 | 0.346*** | −0.107** |
| | (0.032) | (0.338) | (0.000) | (0.028) |
| *Test 1.3:* Class | 0.180** | −0.080 | 0.352*** | −0.108** |
| | (0.028) | (0.337) | (0.000) | (0.030) |
| *Test 1.4:* Work status | 0.174** | −0.079 | 0.345*** | −0.111** |
| | (0.032) | (0.331) | (0.001) | (0.016) |
| *Test 1.5:* Central county | 0.171** | −0.074 | 0.344*** | −0.108** |
| | (0.034) | (0.359) | (0.000) | (0.026) |
| Insurance concerns | | | | |
| *Test 1.6:* Occupational unemployment | 0.173** | −0.077 | 0.346*** | −0.105** |
| | (0.032) | (0.343) | (0.000) | (0.027) |
| *Test 1.7:* Skill specificity | 0.118 | −0.086 | 0.281*** | −0.120** |
| | (0.151) | (0.302) | (0.006) | (0.013) |
| Additional macro-level controls | | | | |
| *Test 1.8:* State income | 0.171** | −0.085 | 0.337*** | −0.106** |
| | (0.029) | (0.298) | (0.000) | (0.027) |
| *Test 1.9:* MSA income | 0.165** | −0.067 | 0.339*** | −0.104** |
| | (0.043) | (0.409) | (0.001) | (0.035) |
| *Test 1.10:* MSA GDP per capita | 0.176** | −0.074 | 0.369*** | −0.095** |
| | (0.038) | (0.365) | (0.000) | (0.044) |
| *Test 1.11:* MSA unemployment rate | 0.168** | −0.078 | 0.342*** | −0.106** |
| | (0.042) | (0.363) | (0.001) | (0.029) |
| Alternative specifications | | | | |
| *Test 1.12:* 'Same state' only | 0.130 | −0.113 | 0.333*** | −0.197*** |
| | (0.170) | (0.297) | (0.003) | (0.002) |
| *Test 1.13:* Heterogeneity (GSS) | 0.148** | −0.090 | 0.258*** | −0.173* |
| | (0.040) | (0.289) | (0.002) | (0.054) |
| *Test 1.14:* With hispanics | 0.101 | −0.102** | 0.180** | −0.078* |
| | (0.164) | (0.035) | (0.049) | (0.087) |
| *Test 1.15:* Ordinal DV | 0.031** | −0.003 | 0.055*** | −0.002 |
| | (0.042) | (0.760) | (0.008) | (0.872) |

*Note:* Numbers are average marginal effects of fear of crime, with corresponding p-values in parentheses (*** $p$ <0.01, ** $p$ <0.05, * $p$ <0.1), estimated at the 90th percentile of the distribution of relative income. Low heterogeneity is defined as the 10th percentile, and high heterogeneity as the 90th percentile. All models have the same specification as Model 3 in Table 4.2 and Table 4.3.

ond, I control for insurance-based explanations of the outcomes discussed above by including measures of occupational unemployment and skill specificity. In the third set of tests, I directly address arguments–as discussed in Section 4.1 above—emphasising the relationship between state wealth and support for redistribution among the rich by controlling for state- and MSA-level income, respectively. Finally, I show that the results remain intact when employing a set of alternative statistical specifications and variables.

**Additional individual-level controls** In Test 1.1, I add a control for left-right ideology, using a standard item that asks respondents to place themselves on a seven-point scale ranging from 'extremely liberal' to 'extremely conservative'. As mentioned in Chapter 3, this variable is not included in the main analysis because I argue that redistribution preferences form an important element of an individual's political ideology, meaning that it is part of the outcome to be explained rather than an independent explanation for it. Nonetheless, the results in Table 4.5 show that the main findings hold even when individual ideology is controlled for, at least outside of the South. With the latter included, the main effect of fear of crime in low heterogeneity settings marginally drops out of statistical significance. With the South excluded, on the other hand, the same effect somewhat declines in magnitude but remains highly significant.

Next, Test 1.2 and 1.3 adds controls for religious denomination (in addition to the measure of religiosity included in the main analysis) and social class, measured using an item asking respondents to classify themselves as 'lower class', 'working class', 'middle class' or 'upper class'. Neither variable affects the main results. In Test 1.4, I include set of indicators for respondents' current work status (working full-time, working part-time, temporarily not working, unemployed, retired, keeping house, or other). Both with and without respondents from the South, the key results remain unchanged: fear of crime has a positive effect on redistribution preferences when the level of population heterogeneity is low, but has either no effect (with the South included) or a negative effect (outside of the South) in places that are more racially heterogenous.

As mentioned in the main text, an additional source of concern has to do with residential sorting within MSAs. It is not only possible but even likely, for example, that individuals with similar demographic profiles and political preferences tend to live in the same areas within any given metropolitan region. Although

the cross-sectional nature of the GSS makes it challenging to completely control for such sorting effects, Test 1.5 adds an indicator of whether a respondent lives in a 'central county', defined by the Census Bureau as the county or counties that constitute the urban core of a metropolitan area. The results in Table 4.5 show that this addition does not significantly affect the main results.

**Insurance concerns** As discussed throughout the preceding chapters, an influential literature in comparative political economy has emphasised the insurance function of redistribution: to the extent that individuals are forward looking, it is possible that redistribution preferences (today) are affected by the risk and associated cost of unemployment (in the future). In related work, moreover, I have argued that the importance of such insurance motives for redistribution preferences increases with income (Kahn and Rueda, 2019, see also Thewissen and Rueda, 2019), paralleling the argument regarding fear of crime developed here. Theoretically, these arguments are compatible. However, to show that the results derived in the main part of this chapter are not driven by such insurance motives, I proceed to include two additional controls. Following Rehm (2009), Test 1.6 adds a measure of occupational unemployment risk, measured at the ISCO-88 1-digit level and calculated from the GSS for each survey year. To capture the relative cost of unemployment, Test 1.7 in turn adds the measure of skill specificity used by Cusack, Iversen, and Rehm (2006) and Iversen and Soskice (2001). While unemployment risk exposure does not change the key findings, the inclusion of skill specificity renders fear of crime insignificant in the model estimated on the entire sample. When the set of Southern states is excluded, on the other hand, the effect of fear of crime remains significant, albeit smaller in magnitude than in the model without skill specificity.

**Additional macro-level controls** As discussed in Section 4.1, a similarly important set of arguments has pointed to the importance of state wealth for the relationship between individual income and support for redistribution and partisanship (Gelman, 2010; McCarty, Poole, and Rosenthal, 2006). Given my concern with geographical variation in the redistribution preferences of the affluent, the empirical implications of these arguments is reasonably similar to those analysed here. To control for such explanations, therefore, Test 1.8 includes a variable measuring the median household income in each state in each

year, based on data from the Current Population Survey (CPS). Following the same logic, Test 1.9 in turn includes a variable measuring median income on the MSA-level, estimated directly from the GSS. Additionally, Test 1.10 and Test 1.11 introduce controls for MSA-level GDP per capita and the MSA-level unemployment rate, respectively. As mentioned in the main text, data for the former variable comes from the Bureau of Economic Analysis, while the latter is calculated from county-level unemployment data supplied by the Bureau of Labor Statistics. The results in Table 4.5 show that neither addition affects the main results significantly: the effect of fear of crime on redistribution preferences remains positive when the level of population heterogeneity is low, and either insignificant or negative when the level of heterogeneity is high.

**Alternative specifications** Finally, I test whether the results derived above are robust to a set of alternative statistical specifications. In Test 1.12, I first limit the sample to respondents who at the time of the survey resided in the same state as they did aged 16 (while restricting the sample further to respondents living in the same city would admittedly have been desirable, my already small sample sizes limit what I can do). As mentioned in the main text, this represents an attempt at addressing concerns about residential sorting. In this specification, no effects are significant when respondents from the South are included, but the main effects remain intact outside of the South. Given the significant reduction in sample size and the smaller magnitude of the effects with the South included, however, these changes are not necessarily surprising.

In Test 1.13, in turn, I substitute the census based measure of heterogeneity with a variable constructed directly from survey responses to the GSS, measuring the share of black residents in the MSA with an income below the year-mean. As discussed above, although this variable comes with the advantage of aligning more closely with my theoretical concerns, it also suffers from the major drawback that is it highly imprecise in smaller MSAs where the sample size of the GSS is correspondingly small. Nonetheless, and with this caveat in mind, employing this variable rather than the census measure does not significantly alter the main findings.

In Test 1.14, I proceed to include hispanic whites in the estimation sample. As such, this test is more theory driven. I argue that in the US context, hispanic status constitutes a salient identity divide, just like race. Limiting the sample of respondents in the main analysis is thus justified, since my argument concerns

how fear of crime matters differently to members of the majority group—in the US, non-hispanic whites—depending on the identity of those with comparatively low incomes. Adding hispanics thus introduces a degree of multidimensionality to the analysis, making the results less intelligible and arguably less directly applicable to my theoretical logic. The results in Table 4.5 largely confirm this expectation. When the South is included in the sample, the positive effect of fear of crime under low heterogeneity drops out of significance, while the negative effect found in high heterogeneity settings becomes significantly different from zero. In the reduced sample that excludes the Southern states, on the other hand, the key results from the main analysis still stand.

Finally, to show that the findings are not driven by my choice to use a binary dependent variable above, Test 1.15 reports the results from an ordinal logit model estimated using the original, seven-point dependent variable (transformed such that higher values indicate greater agreement with the statement that the government should reduce income differences). In this case, the reported average marginal effects can be interpreted as the effect of fear of crime on having a score of six—the second highest category—on this variable. Given this, the magnitude of the estimated effects is naturally smaller. The key takeaway results from the main analysis remain substantively unchanged, however.

### 4.5.2 Law enforcement preferences

Turing to the other side of my argument, Table 4.6 presents a similar series of tests, focusing on the relationship between fear of crime, population heterogeneity and support for increased spending on law enforcement. As before, I begin by adding a range of additional variables: ideology, religious denomination, social class, work status, and indicators of residence in a central county or a large city. I likewise add controls for MSA-level GDP per capita and the unemployment rate. In a second series of tests, I control for expressive models of punitive attitudes by including measures of social trust, economic satisfaction and unemployment. Finally, I also report the results from models estimated with the heterogeneity variable from the GSS, the full sample of both hispanic and non-hispanic white respondents, and an ordinal dependent variable.

**Table 4.6:** Marginal effect of fear of crime on law enforcement preferences

| | Including the South | | Excluding the South | |
|---|---|---|---|---|
| Heterogeneity: | Low | High | Low | High |
| Main analysis | 0.075 | 0.215*** | 0.089 | 0.152** |
| | (0.343) | (0.001) | (0.424) | (0.023) |
| Additional controls | | | | |
| *Test 2.1:* Ideology | 0.072 | 0.217*** | 0.096 | 0.144** |
| | (0.349) | (0.000) | (0.371) | (0.025) |
| *Test 2.2:* Denomination | 0.075 | 0.216*** | 0.084 | 0.160** |
| | (0.344) | (0.001) | (0.448) | (0.017) |
| *Test 2.3:* Class | 0.085 | 0.207*** | 0.101 | 0.154** |
| | (0.284) | (0.001) | (0.359) | (0.026) |
| *Test 2.4:* Work status | 0.053 | 0.213*** | 0.040 | 0.164*** |
| | (0.492) | (0.001) | (0.694) | (0.006) |
| *Test 2.5:* Central county | 0.075 | 0.214*** | 0.088 | 0.152** |
| | (0.339) | (0.001) | (0.431) | (0.024) |
| *Test 2.6:* Large city | 0.075 | 0.215*** | 0.090 | 0.153** |
| | (0.340) | (0.001) | (0.417) | (0.023) |
| *Test 2.7:* MSA GDP per capita | 0.076 | 0.206*** | 0.088 | 0.134** |
| | (0.381) | (0.001) | (0.471) | (0.047) |
| *Test 2.8:* MSA unemployment rate | 0.082 | 0.201*** | 0.089 | 0.152** |
| | (0.297) | (0.002) | (0.423) | (0.023) |
| Expressive models of punitive attitudes | | | | |
| *Test 2.9:* Trust | 0.035 | 0.350*** | -0.037 | 0.339*** |
| | (0.790) | (0.001) | (0.804) | (0.000) |
| *Test 2.10:* Economic satisfaction | 0.059 | 0.214*** | 0.041 | 0.168** |
| | (0.445) | (0.001) | (0.693) | (0.005) |
| *Test 2.11:* Unemployment | 0.075 | 0.214*** | 0.091 | 0.152** |
| | (0.339) | (0.001) | (0.419) | (0.024) |
| Alternative specifications | | | | |
| *Test 2.12:* 'Same state' only | 0.068 | 0.245*** | 0.151 | 0.123 |
| | (0.528) | (0.002) | (0.240) | (0.353) |
| *Test 2.13:* Heterogeneity (GSS) | 0.079 | 0.231*** | 0.028 | 0.296*** |
| | (0.358) | (0.009) | (0.790) | (0.000) |
| *Test 2.14:* With hispanics | 0.069 | 0.098** | 0.084 | 0.053 |
| | (0.261) | (0.022) | (0.315) | (0.180) |
| *Test 2.15:* Ordinal DV | 0.090 | 0.167*** | 0.093 | 0.153** |
| | (0.199) | (0.008) | (0.345) | (0.019) |

*Note:* Numbers are average marginal effects of fear of crime, with corresponding p-values in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$), estimated at the 90th percentile of the distribution of relative income. Low heterogeneity is defined as the 10th percentile, and high heterogeneity as the 90th percentile. All models have the same specification as Model 3 in Table 4.4.

**The South** As a special kind of robustness test, Table 4.6 reports the results of a model identical to that used in the main analysis, but estimated on non-Southern respondents only. Although—unlike for the redistribution side—there is no prima facie reason for expecting the importance of fear of crime, population heterogeneity and income on law enforcement preferences to be different in the South, this test allows me to check whether the results reported above remain intact even in this reduced sample of respondents. The results shown below clearly confirm that this is indeed the case. Excluding the South reduces the magnitude of the estimated effect of fear of crime, but the main findings persist: fear of crime has a significant positive effect on support for increased law enforcement spending in high-heterogeneity MSAs, but no effect on preferences in contexts that are less racially diverse.

**Additional controls** In Test 2.1, I again add a control for political ideology, using the same variable described in the previous section (seven point scale from 'extremely liberal' to 'extremely conservative'). As in the analysis of redistribution preferences, while there are good reasons for expecting ideology and punitiveness to be related, this variable is not included in the main analysis because I argue that criminal justice attitudes are a part—rather than a consequence—of an individual's ideological outlook. The results in Table 4.6 show that adding this variable to the model does not substantially affect the main results. Unsurprisingly, being more conservative is associated with increased demand for law enforcement (all else equal, moving from 'extremely liberal' to 'extremely conservative' increases the likelihood of thinking that spending should increase by about 20 percentage-points), but controlling for this relationship does not alter the joint effect of fear of crime and population heterogeneity on preferences.

Test 2.2 and 2.3 in turn adds controls for religious denomination and self-identified social class. Neither inclusion has a noticeable effect on the substantive results. The same holds true in Test 2.4, which introduces a control for respondents' current work status (indicators for employed full-time, employed part-time, temporarily not working, unemployed, retired, keeping house, or other). As in the main analysis, fear of crime remains a significant determinant of law enforcement preferences in high heterogeneity contexts only. To address concerns about residential sorting, Test 2.5 again adds the indicator of whether a respondent lives in the central county of an MSA. Following a similar logic,

Test 2.6 likewise adds a variable indicating residence in a large city, defined as one with a population of 250,000 or more. Finally—and on the macro-level—Test 2.7 introduces a control for MSA-level GDP per capita, and Test 2.8 adds the measure of MSA-level unemployment. As can be seen in Table 4.6, the results from the main analysis remain largely unaffected by the inclusion of these variables: fear of crime has on effect on law enforcement preferences in places where the level of population heterogeneity is low, but has a large positive effect on such preferences where the level of heterogeneity is higher.

**Expressive models of punitive attitudes** As discussed in Chapter 2, important literatures in both criminology and sociology view punitive attitudes and law enforcement preferences as fundamentally expressive phenomena, rather than as rational responses to crime itself. To check that my results are not driven my such considerations, Test 2.9 begins by adding a measure of social trust. This variable is constructed from a survey item asking 'generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?', with responses recorded on a three point scale ranging from 'cannot trust most people' through 'it depends' to 'can trust most people'. Including this variable in the model dramatically magnifies the results from the main analysis, confirming that social trust is a relevant confounder, especially in the South: with this variable included in the model, the difference between the full- and non-South sample results disappear, and the estimated marginal effect of fear of crime in high heterogeneity settings increases to about 34 percentage-points.

Following a similar logic, Test 2.10 adds a variable measuring individual-level economic satisfaction, based on a standard GSS item asking 'would you say that you are pretty well satisfied with your present financial situation, more or less satisfied, or not satisfied at all?'. Like social trust, economic insecurity has been widely theorised to influence attitudes to punishment and law enforcement: while the many arguments in this vein differ in their details, they all share the common contention that criminal justice preferences are an expression of anxieties rooted in the broad, structural changes that have transformed most advanced industrial economies over the last few decades. As Garland (2001, pp. 148) writes for example, 'shifts in the economic and social position of large sections of the middle and working classes' has led to 'more aggressive controls for an "underclass" that was perceived to be disorderly, drug-prone and dangerous' (see also Costelloe,

Chiricos, and Gertz, 2009. Theoretically, this argument is compatible with that proposed in this dissertation, provided expressive and instrumental influences on law enforcement preferences are orthogonal. As expected, moreover, I find that economic dissatisfaction has a significant positive effect on support for increased law enforcement spending. This does not, however, affect the findings from the main analysis. As a last check that my results are not driven by expressive concerns, Test 2.11 adds an indicator for current unemployment to the model used in the main analysis. Doing so make little difference: both with and without respondents from the South, all the main results stand nearly unchanged.

**Alternative specifications** Finally, I again check that the main results are not sensitive to alternative statistical specifications. As in the robustness checks for the redistribution side of my argument, Test 2.12 restricts the sample to respondents who have not moved to a different state, thus reducing the sample size by about half. Mirroring the previous results, this renders all effects insignificant outside of the South (where, even in the main analysis, they were smaller in magnitude as well as less efficient), while those estimated with respondents from all states remain comparatively unchanged. Test 2.13 in turn substitutes the Census based measure of heterogeneity for a variable estimated from the GSS, giving the share of black respondents in each MSA with incomes below the national year-mean. The results in Table 4.6 show that this does not affect the main findings. Test 2.14 proceeds to include hispanic individuals in the sample. As before, this considerably weakens the results: the effect of fear of crime in high heterogeneity contexts remain significant (albeit substantially diminished in magnitude) when the South is included in the sample, but drops out of significance when respondents from the South are excluded. Given that there are theoretical reasons for limiting the analysis to non-hispanics, however, this is not necessarily a concern. Lastly, Test 2.15 reports the results from an ordinal logit model estimated with the original, three-level dependent variable. Once again, the key results are unaffected.

## 4.6 Summary

The empirical analysis developed in this chapter has shown that in the US—as in Europe—the relationship between fear of crime and political preferences depend

heavily on the level of population heterogeneity. In metropolitan areas with a comparatively small black population share, I find that being afraid of crime is associated with a statistically significant and substantively large positive effect on support for redistribution, but has no parallel effect on support for increased spending on law enforcement. In MSAs that are more diverse, on the other hand—at least for the affluent—the situation is the opposite: fear of crime has no effect on redistribution preferences, but significantly increases support for law enforcement. These results are congruent with the expectations stated in Hypothesis 1 and 2. The trade-off between redistribution and criminal justice measures as alternative, competing solutions to crime as a social problem is thus very real. The empirical results derived above also lend strong support for Premise 2: relative income has a strong negative effect on support for redistribution in the US. The same holds true for Premise 1, and the expectation that the local level of inequality has a significant positive effect on fear of crime.

With regards to the expectation—Premise 3—that the importance of fear of crime to preferences, whether for redistribution or law enforcement, will increase in income, the results paint a perhaps more mixed picture. On the law enforcement side, first, this expectation is clearly borne out: as shown above, fear of crime never makes a difference to the preferences of the poor, but has a large positive effect on support for law enforcement spending among the rich provided the level of heterogeneity is sufficiently high. For the other side of my argument—about redistribution preferences—this is less evidently the case, however. Although I do find evidence in support of the expected income effect in low heterogeneity settings outside of the South, fear of crime also has an unanticipated yet significant positive effect on the redistribution preferences of low- and middle-income individuals when the level of heterogeneity is high (see the results in Figure 4.21). On one level, this result is quite counterintuitive: for below average-income, white Americans, fear of crime does not have a positive effect on support for redistribution in MSAs that are racially homogenous, but does have such an effect in places that are more diverse. What might explain this finding? One possibility again has to do with residential sorting: if progressive views about crime are not independent from the likelihood of deciding to live in a high-heterogeneity MSA—most of which are concentrated in the South, Northeast, and the traditional 'Rust Belt'—for example, this outcome would immediately be less surprising. Such an explanation could also account

for why the same pattern is not evident in the European results derived in Chapter 3, since sorting between countries is less of an issue than domestic sorting between cities. Given the cross-sectional nature of the GSS, however, there is unfortunately little I can do to directly test this alternative explanation.

# Chapter 5

# Identity and Crime Preferences: Experimental Evidence

To complement the observational part of the empirical analysis, I develop and implement an experimental design aimed at examining the micro-level causal mechanism at the core of my theoretical model: the argument that the social identity of potential criminals affect the preferences that potential victims have over alternative means of deterring crime. As I have discussed at some length in Chapter 2, I expect divergent group identities between victims and perpetrators of crime to be associated with a decline in support for redistribution relative to punishment, compared to the case where both share the same social identity. Although the results derived thus far lend strong support for the main, real-world implications of this argument—above all, that the level of population heterogeneity among the poor significantly conditions the relationship between fear of crime and preferences—a number of reasons make it challenging to assess this causal mechanism using observational data alone. Aside from the more general concerns about causal identification in non-experimental analyses (for an excellent discussion of these issues, see especially Morton and Williams, 2010), additional complications arise from the risk of residential sorting—as mentioned in the previous chapter, the possibility that individuals with particular preferences towards redistribution and law enforcement tend to sort into geographic units in

a way that is not independent from its level of population heterogeneity. Using observational data alone, separating these two effects—that of residential sorting, on the one hand, from any independent effect of heterogeneity, on the other—is fundamentally difficult. Using an experimental setup with random assignment, by contrast, allows me to estimate the causal effect of social identity directly.

For clarity, it is important to emphasise that the experiment is not intended as a standalone test of my entire theoretical argument. Rather, and much more narrowly, it is designed to addresses its key, micro-level causal mechanism: the claim that the social identity of potential criminals matter for the choices potential victims of crime make regarding alternative means of making crime less attractive. As such, it does not assess my claim inequality causes fear of crime, nor the argument that the importance of fear of crime to preferences, for redistribution and for law enforcement, depends on relative income. The main reason for this is again empirical: in relative terms, it is indeed this part of my argument that poses the greatest set of challenges when relying on the type of cross-sectional, observational data used in Chapter 3 and Chapter 4. Aside from the aforementioned problems relating to residential sorting, distinguishing contingent group explanations—that is, effects that follow from particular views or beliefs that natives in Europe or whites in the US may have about immigrants and blacks, respectively—from more general and universal effects that stem from group membership in isolation is fundamentally difficult, if at all possible, when relying on solely on observational data. In allowing for the creation of arbitrary groups and identities devoid of any pre-exiting biases, on the other hand, the experiment allows me to directly overcome this empirical challenge.

Before proceeding, however, it may be useful to briefly revisit this part of my theoretical logic. I have argued that the importance of fear of crime to preferences over alternative means of reducing crime, whether through redistribution or punishment, hinges on the relationship between social identity and causal attribution. As discussed in some detail in Chapter 2, there are good reasons for expecting shared group membership between potential perpetrators and potential victims of crime to facilitate situational explanations of criminal behaviour, including material concerns affected by inequality, but for divergent identities between perpetrators and victims to promote dispositional explanations of crime that are fundamentally unrelated to economics. Given that the argument from fear of crime to support for redistribution presupposes a causal inference from

inequality to crime, in turn, it follows that the degree of heterogeneity—in the simplest sense, shared or divergent social identities—between victims and criminals comes with important consequences for the relationship between fear of crime and support for redistribution, on the one hand, and punishment, on the other. If this argument is correct, therefore, potential victims of crime given a choice between redistribution and punishment as alternative means of producing deterrence should become more likely to select the former over the latter when faced with a potential perpetrator that is a member of their own identity group, and conversely, more likely to chose punishment when faced with an out-group opponent. It is this fundamental empirical expectation that the experiment implemented in this chapter is designed to address.

With these points in mind, the sections below first consider my experimental design, before turning to the empirical results and their implications for my argument. The experiment ran at the Centre for Experimental Social Sciences (CESS) at Nuffield College, Oxford, between November 2015 and January 2016.

## 5.1 Experimental design

In both the treatment and the control sessions, the experiment consisted of three main stages. In the first, group identities were assigned based on participants' art preferences. Subjects in a control condition completed a similar task, but were not assigned group identities. The second stage comprised a real-effort task involving additions, which determined the distribution of endowments. In the third and final stage of the experiment, subjects played ten rounds of an interactive stealing game, in which one player could attempt steal from the other, who in turn could make strategic investments aimed at producing deterrence.

### 5.1.1 Participants

The experiment involved 128 participants in total, all of which were recruited from the CESS subject pool, using the Online Recruitment System for Economic Experiments (Greiner, 2015). 53% of the participants were female, and 47% were male. The average age was 30. A majority of the subjects were undergraduate students from the University of Oxford, but the sample also included a significant share—38%—of non-students. Whereas much (if not most) pervious

experimental work in both political science and economics has relied on student participants as a convenience sample (in a survey of major economics journals, for example, Danielson and Holm (2007) find that only 7% of papers involving laboratory experiments used non-student subjects), there are growing concerns in the methodological literature about the generalisability of student behaviour to wider and more heterogenous populations. Summarised briefly, a general conclusion from this research suggests that, in economic games measuring other-regarding or social preferences, student participants tend to be atypically selfish (Sears, 1986; Carpenter, Connoly, and Knowles Myers, 2008; Belot, Duch, and Miller, 2015; Peterson, 2001; Falk, Meier, and Zehnder, 2013). On the other hand, the evidence remains more mixed when it comes to designs that do not explicitly involve other-regarding behaviour (Bosch-Domènech et al., 2002; Depositario et al., 2009). Given these concerns, and while a more representative set of subjects would admittedly have been desirable, the sample used here represents an improvement on comparable designs relying on student participants only (for discussions, see Druckman and Kam 2011; Belot, Duch, and Miller 2015).

Several steps were taken to meet the ethical requirements and best-practices of experiments with human participants. All procedures were fully anonymised, and at no point during the experiment were there any traceable links between the personal information stored in the CESS subject database and the data collected in the experiment itself. The experimental data will be used for the purposes of this project only, and will not otherwise be made publicly available in any form. Subjects were not asked to provide any sensitive information, and there was no deception. Participation was strictly voluntary, and subjects were free to withdraw from the experiment at any point, without reason or penalty. All subjects gave their written consent to participate. Accordingly, the experimental design was approved by the CESS Ethics Review Committee, as well as by the Central University Research Ethics Committee of the University of Oxford.

### 5.1.2 Design and procedure

The experiment ran in six sessions, two of which had 24 participants and the remaining four 20 participants. One session was the control condition with no identity treatments. At the beginning of each session, subjects were randomly (and anonymously) seated at computer cubicles in the CESS laboratory, in such

a way that they could not observe each other. They were then asked to read a brief information sheet. The entire experiment took place on computers using z-Tree software (Fischbacher, 2007). The information sheet is included in Appendix C below, together with the full set of instructions (which were displayed on participants' computer screens as well as read aloud by the experimenter before each stage of the experiment) are available on request. Participants were remunerated with a £4 show-up payment, plus additional tokens (labelled ECU, for Experimental Currency Unit) earned in the experiment itself. At the end of the experiment, these were converted to cash at a rate of 60 ECU to £1. Total take-home payments thus varied between subjects based on their performance, but ranged from a minimum of £8.20 to a maximum of £21.50, averaging £14.70.

**Stage 1: Group inducement**

As I have emphasised above, the experiment aims to test the argument that shared or divergent identities between potential criminals and potential victims is an important determinant of preferences over alternative means of deterring crime. Thus, a critical but difficult aspect of the experimental design concerns the question of how to manipulate the level of heterogeneity. Although some previous work has relied on pre-existing or naturally occurring identities and priming techniques to do so (for example, Habyarimana et al. 2007; Bornhorst et al. 2010; Buchan, Johnson, and Croson 2006; Bernhard, Fehr, and Fischbacher 2006; Goette, Huffman, and Meier 2006; Tanaka, Camerer, and Nguyen 2010), I choose to induce exogenous identities in the laboratory using the minimal group paradigm (Tajfel et al., 1971; Tajfel and Turner, 1986; Yamagishi and Kiyonari, 2000; Simpson, 2006; Chen and Li, 2009). Summarised briefly, a core finding form this social psychology literature is that even when group membership is based on some relatively trivial criterion—typically participants' preferences for specific paintings—categorisation itself is generally sufficient to establish robust group identities and effects.

There are several reasons for preferring exogenously induced minimal groups over the use of natural identities together with priming. The perhaps most important advantage of artificial or exogenous identities has to do with control. Naturally occurring identities are often (if not always) multidimensional, and it is difficult to infer empirically *ex ante* which identity traits subjects hold salient,

or whether the degree to which they do so varies from subject to subject in some non-random and systematic way (for a discussion, see Chen and Li, 2009). Consequently, causal identification and hence causal inference in such designs can be challenging. Artificial identities, on the other hand, avoid these problems, and allow for a greater degree of certainty that the observed treatment effects are due to the experimental manipulations. Moreover, large literatures in both social psychology and behavioural economics have established that artificial identities are sufficiently strong to influence behaviour in a wide range of both cooperative and non-cooperative contexts (Hogg 2008; Eckel and Grossman 2005; Charness, Rigotti, and Rustichini 2007; Diehl 1990, amongst many others).

In line with previous studies using the minimal group paradigm (Tajfel et al., 1971; Chen and Li, 2009; Yamagishi and Kiyonari, 2000), this experiment induced identities in three sequential steps. At the start of each session, subjects were shown a sequence of five pairs of paintings by Paul Klee and Wassily Kandinsky, with one painting by each artist in each pair. Without receiving any information about which artist had painted which of the two paintings, they were asked to select the one in each pair that they most preferred. Based on their relative preference for the works by either painter, they were subsequently divided into two equally sized groups, and privately informed of their group membership. Once assigned, these identities remained constant for the remainder of the session. A list of the paintings used, which were the same as those used in the majority of previous minimal group experiments, is included in Appendix C.

To reinforce these induced group identities, subjects subsequently participated in a collective quiz, in which they were asked to identify the artist of additional paintings, also by Klee and Kandinsky. This task worked as follows. The two paintings were initially displayed to the subjects on their computer screens, and they were asked to submit their initial guesses about the identity of the painters privately. Once submitted, these initial answers were made available to all other members of their own identity group (that is, subjects in the Kandinsky-group could see the answers of all other subjects in the Kandinsky-group, but not the answers submitted by those in the Klee-group, and *vice versa*). Using this information, subjects were then allowed to revise their initial answers before submitting them permanently. As is common practice, this part of the group inducement process was incentivised: subjects received a bonus payment of 100 ECU if a majority of their fellow group members identified the artist of both

paintings correctly, and an additional 100 ECU if their group collectively provided at least as many correct answers as the members of other group. However, to avoid the possibility that the outcome of this task affected subsequent group behaviour, the results were not revealed until the end of the experimental session.

Third, and as the final part of the group inducement stage, subjects were told to allocate 100 ECU to two other anonymous participants, one in-group and one out-group, in a series of five binary other-other dictator games. Similarly to what is the case in previous studies using minimal groups, they were not allowed to keep any ECU for themselves, as to avoid considerations of material self-interest. The structure of the five dictator games, together with summary responses for the treatment (with identities) and the control conditions (without identities), are shown in Table 5.1 below. In each game, there was a trade-off between the amount that could be allocated to the in-group and to the out-group subject, and all games were zero-sum. As can be seen from Table 5.1, subjects in the treatment condition were consistently more likely to choose the option that most benefited their fellow group member, when possible, whereas those in the control condition chose egalitarian distributions to a greater extent. The overall difference between these choices is highly statistically significant ($t = 10.65$, $p < 0.001$), suggesting that the group inducement procedure was successful. To avoid design bias, moreover, the order in which the alternative allocations were displayed to the subjects on their computer screens was individually randomised.

**Table 5.1:** Summary of other-other dictator games

| | Option A | | | Option B | | |
|---|---|---|---|---|---|---|
| | In/Out (ECU) | Treat. | Ctrl. | In/Out (ECU) | Treat. | Ctrl. |
| 1 | 80/20 | 80.6% | 65.0% | 20/80 | 19.4% | 35.0% |
| 2 | 60/40 | 79.6% | 45.0% | 40/60 | 20.4% | 55.0% |
| 3 | 60/40 | 56.5% | 25.0% | 50/50 | 43.5% | 75.0% |
| 4 | 50/50 | 89.1% | 90.0% | 40/60 | 10.9% | 10.0% |
| 5 | 100/0 | 87.0% | 60.0% | 0/100 | 13.0% | 40.0% |

Once all five decisions had been made, one choice was randomly selected and implemented. As before, however the payoffs from this part of the experiment were not revealed until the end of the session. Given the structure of the game, each subject thus allocated a total of 100 ECU to two other subjects, and received allocations from two further subjects. Like the collective quiz, this task was was included both to enhance group identities, and because it allows for a test of the strength of those identities (Oakes and Turner, 1980; Chen and Li, 2009).

The control condition, finally, followed the above procedure as closely as possible, but without inducing identities. In the initial step, subjects were shown the same five pairs of paintings and were likewise asked to select the one in each that they most preferred, but were not divided into groups accordingly (rather, they were simply informed of their preference for Klee or Kandinsky). In the second step—the collective quiz—subjects once again first submitted their initial guesses, but these were subsequently made available to all other subjects before they submitted their final answers. In this setup, they were remunerated with 100 ECU for each correct individual answer, rather than on the basis of group performance. Lastly, subjects in the control condition played the same set of five other-other dictator games as in the treatment sessions, but without any reference to the group identities of the other players, who were labelled as 'Player 1' and 'Player 2'.

**Stage 2: Income generating real-effort task**

Once the identity inducement stage was complete, subjects earned income in a simple real-effort task involving mathematical additions of varying difficulty. They had 90 seconds to perform as many correct additions as possible. Similar procedures have been used in a large number of previous studies. When completed, subjects in each of the two identity groups (or all subjects, in the control group) were ranked based on their performance, and accordingly assigned differential endowments from a fixed distribution, ranging from 300 ECU for the worst performing participants in either group, to 1000 ECU for those with the highest scores. Performance in the real-effort task thus determined where in the distribution a particular subject ended up, and within each identity group, endowments ranged from small to large. In the subsequent stealing game, in turn, those in the upper half of the endowment distribution were assigned the role of Player 1 (potential victims of crime), whereas those in the lower half were assigned the

role of Player 2 (potential criminals). This design thus ensured that in each of the five treatment sessions, there were either 10 or 12 Klees and an equal number of Kandinskys, with either five or six 'rich' and 'poor' subjects in either group. Further details about the endowment distributions are included in Appendix C.

Previous research has shown that the mechanism by which endowments are assigned in economic experiments involving interactions between subjects can influence their subsequent behaviour (List and Cherry, 2008; Durante, Putterman, and van der Weele, 2014). In broad terms, a distinction can be made between approaches in which rewards are distributed on the basis of merit or performance, and designs in which they are assigned randomly, or according to some principle not tied to performance or effort. A general conclusion in this literature is that when endowments are assigned arbitrarily, individuals tend to exhibit a greater degree of other-regarding, non-selfish behaviour compared to what is otherwise the case. This finding, in turn, is often interpreted with reference to concerns about fairness: when the assignment of assets or advantaged roles is considered unfair, individuals are more willing to sacrifice some of their own material benefits for the sake of achieving an outcome distribution that is seen as more equitable. Cherry, Frykblom, and Shogren (2002) and Cherry (2001), for example, find that first-movers in a standard dictator game were significantly more likely to follow the standard theoretical prediction of offering zero to the receiver when they were acting over funds that they had earned, compared to what was the case when the same funds had been assigned at random. In the opposite direction, Oxoby and Spraggon (2008) show that the reverse result follows when dictators allocate assets generated by effort exerted by the receiver.

Given that I am interested in eliciting preferences over alternative means of producing deterrence, these finings provide good reasons for preferring a non-random assignment of endowments in the experiment developed here. Importantly, adopting this approach rather than the alternative avoids the possibility that the income-generating mechanism itself induces other-regarding concerns as a motivation in the subsequent stealing game (and, even more problematically, that it leads to differential altruism towards in-group and out-group members), which in turn would pose a challenge to the internal validity of the experimental design. In line with this reasoning, moreover, using simple additions comes with the secondary benefit of posing relatively low demands on subjects in terms of the need for prior knowledge or specialised skills.

**Stage 3: Two-person sequential stealing game**

Lastly, the third and final part of the experiment consisted of ten rounds of a two-person stealing game, designed to elicit preferences over alternative means of deterring crime under varying degrees of heterogeneity. Although some previous experimental work has examined the premise that criminal behaviour responds in predictable ways to changes in economic incentives (Abbink, Irlenbusch, and Renner, 2002; Anderson and Stafford, 2003; DeAngelo and Charness, 2012; Friesen, 2012; Rizzolli and Stanca, 2012; Schildberg-Hörisch and Strassmair, 2012; Harbaugh, Mocan, and Visser, 2013; Khadjavi, 2014), these studies look only at the choices made by potential criminals, and accordingly pay no attention to those made by potential victims. To the best of my knowledge, this experiment is the first to shift focus from the former to the latter, and to examine the determinants of preferences over alternative means of producing deterrence, rather than the effects of such measures on the propensity to engage in crime. Additionally, it is also notable that no previous experimental work has considered the role of identity in the context of crime more generally. This is true not only in terms of its effects on preferences for deterrence, but also in terms of its implications for criminal behaviour. As such, and while my main theoretical interest in this chapter lies with the behaviour of potential victims, the design further allows me to assess whether identity matters for the propensity to engage in crime, or for the relative efficiency of alternative means of generating deterrence. For reference, a preliminary analysis of this side of the results is included in Appendix C.

The stealing game itself proceeded as follows. At the beginning of each round, each 'rich' subject (Player 1) was randomly matched with a 'poor' player (Player 2). As indicated above, the Player 1's were drawn from the upper half of the income distribution, and the Player 2's from the lower half. Given that the design implies that there was always an equal number of Klees and Kandinskys, and that each group had an equal number of potential Player 1's and potential Player 2's, the initial probability of being matched with an in-group or an out-group member was 50%. Moreover, since individual endowments within each income category were unequally distributed, the income difference between subjects also varied continuously between different rounds of the game. Once paired, subjects were told whether the subject they were playing against was a Klee or a Kandinsky, and were informed of his or her endowment. To avoid reciprocity effects, no two

subjects could be paired twice, and this fact was public information. Practically, this meant that in the sessions with 20 participants, all rich subjects were matched once with all poor subjects, and that in the sessions with 24 participants, they were matched once with all but one of the poor subjects. The control condition followed an identical setup, aside from the absence of identity groups.

Player 1 (with the larger endowment) was then informed that in the following step of the game, Player 2 (with the smaller endowment) would be able to steal a predetermined share of his or her endowment. To keep the payoffs balanced, this share was always equal to $(1/5)\alpha$, where $\alpha$ is the probability that such an attempt to steal would be successful. In response, Player 1 was given the opportunity to 'invest' in two alternative means of disincentivising Player 2 from attempting to steal: redistribution and punishment. As I discuss below, both options served to make it less profitable for Player 2 to attempt to steal, but they achieved this effect through different mechanisms. Aside from a limit on the total amount that Player 1 could invest—included to avoid negative payoffs for Player 2—there were no other constraints on this choice: he or she could spend nothing, or any positive amount on either or both of redistribution and punishment.

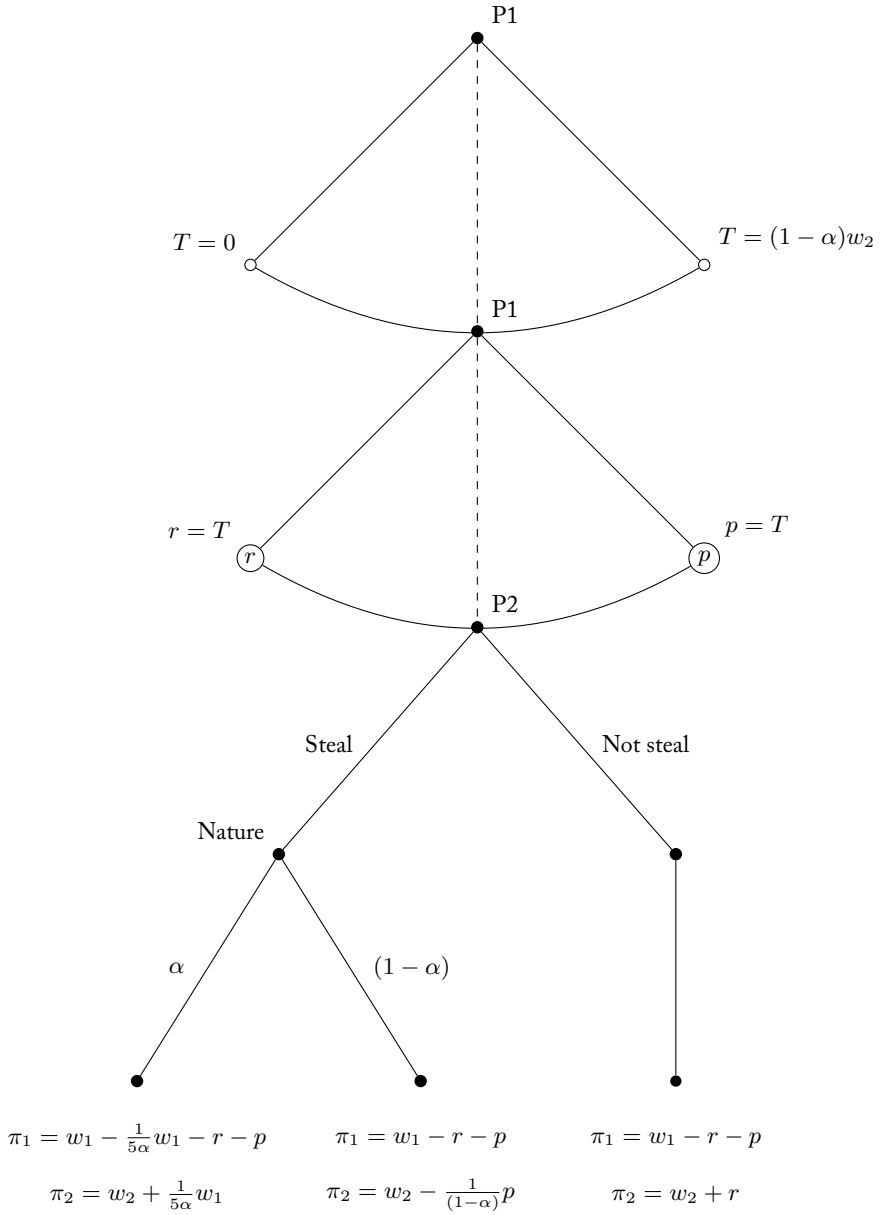
Redistribution functioned as a conditional transfer, which Player 2 received only if he or she did not attempt to steal from Player 1. This means that the more Player 1 decided to spend on redistribution, the greater the opportunity cost of crime became for Player 2. In producing deterrence by increasing the return to legitimate activities rather than by decreasing the payoff tied to crime itself, it thus mirrors, albeit indirectly, the theorised implications of declining inequality on the crime rate. Punishment, in turn, generated a similar effect but through different means: the greater the amount spent on punishment by Player 1, the more costly punishment became to Player 2 in the event of an unsuccessful attempt to steal. In contrast to redistribution, therefore, spending on punishment made crime less attractive by lowering the expected payoff from attempting to steal, but had no effects on its opportunity cost. More specifically, each ECU invested in punishment by Player 1 increased the cost of punishment by a factor of $1/(1-\alpha)$. This meant that independently of $\alpha$, the deterrence effect produced by a given amount invested in either redistribution or punishment was always the same, assuming risk-neutrality: that is, 1 ECU spent on punishment always lowered the expected payoff from attempting steal by the same amount that 1 ECU spent on redistribution increased the payoff from not attempting to steal.

This is an important part of the design, since it implies that given some level of total spending, all combinations of redistribution and punishment had the same effect on the incentives facing Player 2. To make this choice meaningful, moreover, subjects were provided with an interactive interface in which they could test different combinations of spending, and see their implications for both their own and their opponent's payoffs, before they submitted their choices.

Once this choice had been made, Player 2 decided whether or not to attempt to steal from Player 1. If he or she chose not to do so, the payoff for Player 2 was his or her endowment plus the amount, if any, spent on redistribution by Player 1. In this outcome, Player 1 received the endowment net of the total amount invested in the first step of the game, which was always a sunk cost. If Player 2 attempted to steal, by contrast, the attempt was successful with probability $\alpha$ (one-third in one treatment session, one-half in two sessions and for the control group, and two-thirds in two sessions), and unsuccessful with probability $(1-\alpha)$. If the attempt was unsuccessful, Player 2 was forced to pay a fine equal to $p/(1-\alpha)$, where $p$ is the amount spent on punishment by Player 1. Spending on punishment by Player 1 thus increased the expected cost of punishment to Player 2 by the same amount. At this node, the payoff for Player 1 was the same as when Player 2 did not attempt to steal. If the attempt was successful, finally, the payoff for Player 2 was the endowment plus $w_1/(5\alpha)$, where $w_1$ is the endowment of Player 1. Taken together, this means that when Player 1 did not spend anything on either redistribution or punishment, the expected net profit from attempting to steal for Player 2 was always $(1/5)w_1$, independently of the probability that the attempt was successful. The payoff for Player 1, on the other hand, was the endowment less both the amount spent in the previous step and the amount stolen.

For reference, Figure 5.1 on the next page illustrates the basic structure of the stealing game in extensive form. To keep things clear, Player 1's decision in the first part of the game is shown as consisting of two separate steps: he or she first decides on the total amount to spend, denoted $T$, and then choses how to allocate this sum between redistribution and punishment. In the experiment, these two choices were made simultaneously. The three terminal nodes at the bottom of the tree illustrate the three possible outcomes of each round: Player 2 successfully stealing from Player 1, Player 2 unsuccessfully attempting to steal, or Player 2 deciding not to attempt to steal. At each terminal node, $\pi_1$ represents the payoff for Player 1, and $\pi_2$ gives the payoff for Player 2.

**Figure 5.1:** Stealing game in extensive form



After Player 2 had decided to steal or not to steal, the round played out and the game ended. If Player 2 attempted to steal, the attempt was successful with the given probability. As I have said above, all subjects completed a total of 10 rounds. Once they were finished, one of the 10 rounds was randomly chosen by the computer and implemented, the outcome of which then determined the subjects' final payoffs (together with any additional payments they had earned in the first part of the experiment). Total payoffs were displayed to the subjects on their computer screens, broken down into their individual components. This aspect of the design was chosen in order to ensure that the stakes in each round were, at least potentially, sufficiently high to induce meaningful and representative behaviour, as well as to prevent order effects from dominating in the later rounds of the game. At the end of each session, finally, subjects completed a short post-experiment questionnaire, with straightforward questions about demographics (age, gender, marital status), experience with crime, political self-orientation, and prior familiarity with the paintings by Klee or Kandinsky. Descriptive statistics from this part of the experiment are included in Appendix C.

### 5.1.3 Summary of the experimental design

The experimental design is summarised in Table 5.2. Given the argument, my primary substantive interest concerns the relationship between the two identity treatments—the in-group treatment in which both players came from the same identity group, and the out-group treatment in which they belonged to different groups—and the choices made by Player 1 to purchase different combinations of punishment and redistribution. In total, however, the design generates six discrete combination of factor levels (heterogenous or homogenous games, with a low, intermediate, or high probability that an attempt to steal by Player 2 was successful), plus continuous variation in the level of inequality between Player 1 and Player 2. Once again, if my theoretical expectations are met, Player 1 will substitute spending on redistribution for spending on punishment when faced with an out-group opponent, relative to what is the case in the in-group treatment.

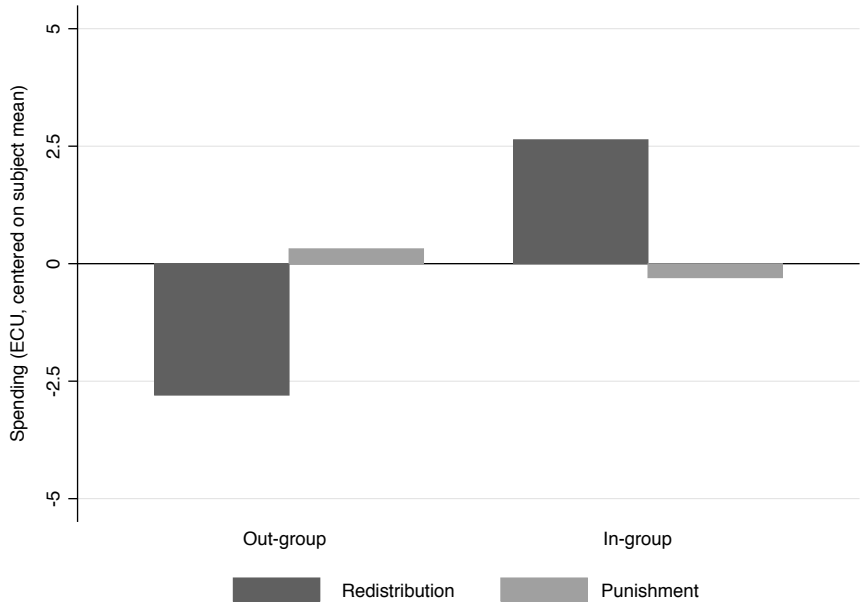
**Table 5.2:** Overview of experimental design

| Stage | Low probability | Intermediate probability | High Probability | Control |
|---|---|---|---|---|
| 1 | Painting choice<br>Identity inducement<br>Collective quiz<br>Dictator games | Painting choice<br>Identity inducement<br>Collective quiz<br>Dictator games | Painting choice<br>Identity inducement<br>Collective quiz<br>Dictator games | Painting choice<br>No identity inducement<br>Individual quiz<br>Dictator games |
| 2 | Real-effort task | Real-effort task | Real-effort task | Real-effort task |
| 3 | Stealing game<br>10 rounds<br>Identities<br>$\alpha = 1/3$ | Stealing game<br>10 rounds<br>Identities<br>$\alpha = 1/2$ | Stealing game<br>10 rounds<br>Identities<br>$\alpha = 2/3$ | Stealing game<br>10 rounds<br>No identities<br>$\alpha = 1/2$ |
| 4 | Survey | Survey | Survey | Survey |

## 5.2 Descriptive patterns

As always, it is useful begin by considering some simple descriptive patterns that emerge from the data. As a start, Figure 5.2 shows the average number of ECU allocated to redistribution and punishment in the in-group and the out-group treatments, based on the pooled sample of observations from all five treatment sessions. To simplify interpretation and to make meaningful comparison more straightforward, the scores underlying these averages are centred on their subject means: as such, negative values imply that subjects spent less than their overall average, on redistribution and punishment respectively, and positive values that they spent more than average. Zero represents the subject mean. They thus carry an interpretation that parallels that of standard fixed-effects regression estimates, giving the effect of the two identity treatments on spending on redistribution and punishment as deviations from each subject mean, thus ignoring all between-subject differences in the overall level of spending. As is clearly visible from Figure 5.2, these initial numbers paint something of mixed picture. Subjects spent less on redistribution when playing against an opponent who did not belong

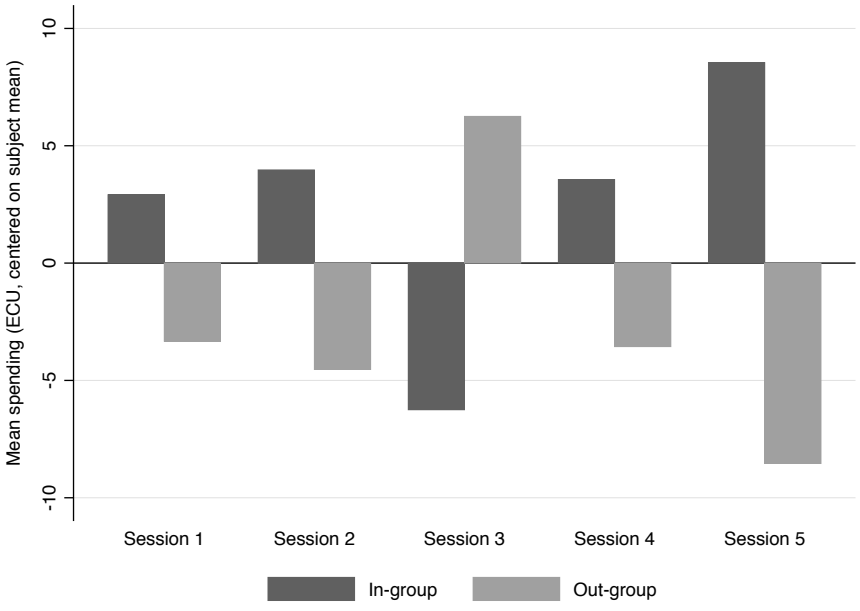
**Figure 5.2:** Mean spending on redistribution and punishment



to the same identity group as themselves, compared to what they did when facing an in-group member. However, this difference is both reasonably small in magnitude and only marginally statistically significant ($t$ = -1.59, $p$ = 0.056). On the punishment side, on the other hand, and while the difference in spending between the two identity treatments is nominally in the right direction, it remains both negligible and well within the margin of error ($t$ = 0.15, $p$ = 0.440).

Nonetheless, this aggregate-level data obscures some more fine-grained but ultimately quite consequential sources of variation. Perhaps most importantly, this includes the observation that the results from one of the treatment sessions are considerably and consistently different from those deriving from the remaining four sessions. This is particularly the case for the redistribution side of the data. Figure 5.3 below illustrates this issue graphically, by plotting the same data shown in Figure 5.2, but disaggregated by session. As Figure 5.3 shows, with respect to the relationship between the group status of Player 2 and spending on redistribution by Player 1, subjects in Session 3 behaved in the opposite way compared to those who participated in the other sessions: on average, they increased spending on redistribution when playing against an out-

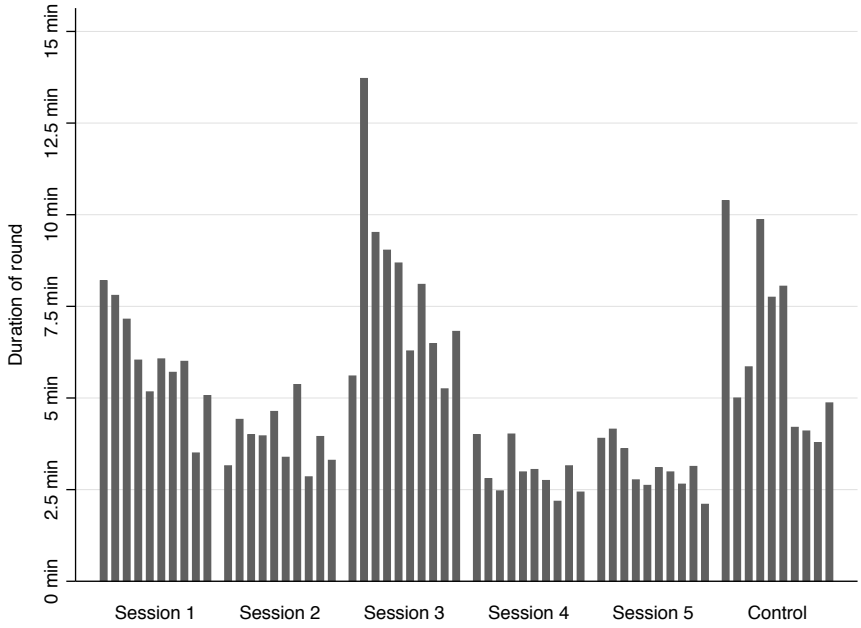
**Figure 5.3:** Mean spending on redistribution by session



group member, and lowered it when playing against someone from their own identity group. These effects are furthermore comparatively large in magnitude. In the other four sessions, the relationship remains the reverse, with decreased spending on redistribution in the out-group relative to the in-group treatment. There are two possible, *ex post facto* explanations for this anomaly.

First, some of the subjects that participated in Session 3 arrived late, allowing the other subjects to interact with and speak to each other before the session could eventually begin. This did not happen before any of the other experimental sessions, but forms one possible explanation of the divergent results and the apparent differential effect of identity on preferences in the stealing game (for a discussion of communication and its consequences in the context of social identity and minimal group experiments, see Chen and Li 2009). Second, and perhaps relatedly, subjects in Session 3 were also notably slower than their counterparts in the other sessions when playing the stealing game. This is illustrated in Figure 5.4, which plots the duration of each round of the game for each of the six sessions, including the control group. In Session 3, it took subjects 80 minutes to complete the 10 rounds of the game, compared to an average of 41 minutes in

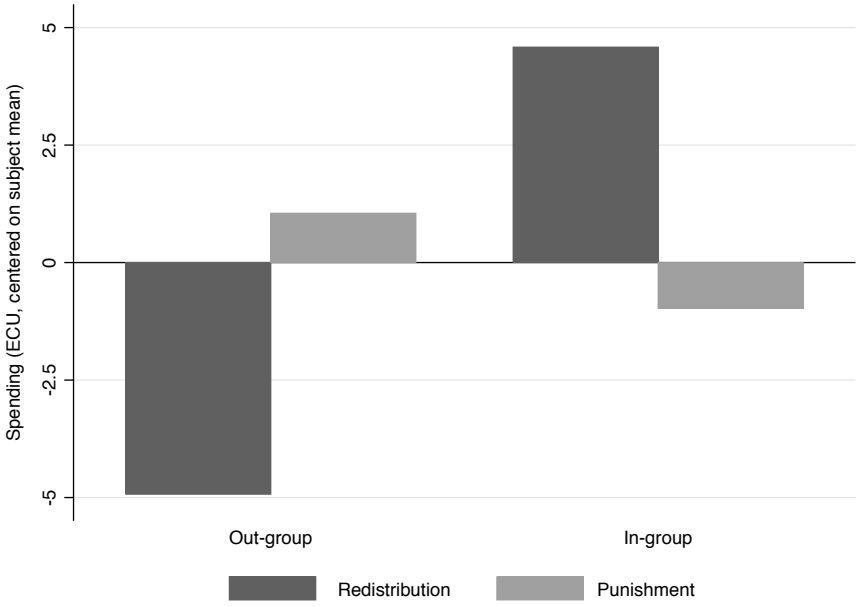
**Figure 5.4:** Duration of the stealing game by session



the other treatment sessions, and 63 minutes for the control group. Aside from the issue noted above, I have no certain explanation for these differences. Moreover, the slow rate at which the game progressed prompted the experimenter to ask the subjects to speed up in order to finish the session on time. As this did not happen in any of the other sessions, it offers another potential explanation for the heterogenous results. For these reasons, I exclude the data from Session 3 from the more formal analysis of the results, as developed in the next section.

Given this concern, Figure 5.5 is analogous to Figure 5.2, but excludes the irregular results from Session 3 from the calculation of the averages. This reduces the total number of observations to 440, clustered within 44 subjects from four treatment sessions. As expected, disregarding the outlying data from Session 3 amplifies the difference noted above between spending on redistribution and spending punishment under the two identity treatments. The direction of these effects remains the same as that noted above: subjects decreased spending on redistribution but increased spending on punishment when playing against an out-group opponent, compared to what was the case when they faced an in-

**Figure 5.5:** Mean spending on redistribution and punishment



group member. As before, however, the magnitude of this effect is considerably larger for the redistribution side than it is for punishment. Moving from the in-group to the out-group treatment, subjects lowered spending on redistribution by approximately 9.52 ECU, from an average of 4.6 ECU above the mean in the in-group treatment, to 4.93 ECU below the mean when their opponent came from the out-group. This difference is highly statistically significant ($t$ =-2.61, $p$ = 0.01) as well as reasonably large in magnitude, especially in relation to the overall average level of spending on redistribution of 41.2 ECU. For punishment, on the other hand, the difference in spending between the two identity treatments is limited to 2.4 ECU (from 0.98 ECU below the mean in the in-group to 1.05 ECU above the mean in the out-group treatment), and is likewise not significantly different from zero at any conventional level ($t = 0.45, p = 0.33$).

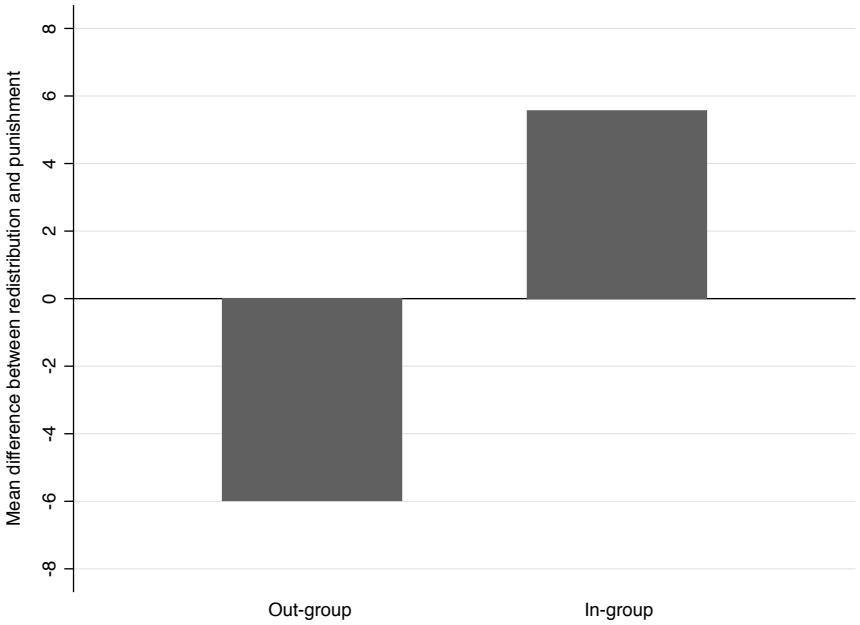
Taken together, these patterns are thus asymmetric: they suggest that although demand for redistribution was higher in the in-group than it was in the out-group treatment, there are no parallel effects going in the opposite direction in terms of demand for punishment. It is important to emphasise, however, that whereas the summary statistics shown above are useful for developing an initial and accurate understanding of the experimental data, they are only indirectly related to my main theoretical concerns. The argument developed in Chapter 2 implies that individuals should alter their demand for redistribution and punishment as alternative means to deter crime in response to the social identity of those most likely to engage in criminal activities, which is quite different—both substantively and statistically—from positing a direct relationship between identity and support for either option when taken in isolation. The simple statistical tests discussed above deal only with the latter set of relationships, but say little about relative spending. They do not distinguish, for example, subjects who increased (or decreased) spending on both redistribution and punishment in either of the two identity treatments, from those who spent more on one of the options but less on the other (or, less strongly, altered the relative composition of their aggregate spending on deterrence in response to the identity of the other player). Given that the latter type of behaviour is in line with my theoretical expectations while the former is not, it follows that simple comparisons such as those considered above are ill suited as tests of my theoretical logic.

An arguably more productive way to approach this data is to analyse the relationship between identity and preferences for redistribution and punishment

when the two are considered jointly, rather than analysed separately. In conceptual terms, this amounts to asking whether subjects purchased different bundles of redistribution and punishment in the two identity treatments, or equally, if they altered spending on redistribution (or punishment) in relation what they spent on punishment (or redistribution) depending on the social identity of their opponent. In contrast to the simple comparisons discussed above, this approach allows me to distinguish between variation having to do with the overall level of spending (which is not of direct relevance to the theory), and variation having to do with the relative composition of spending (which is). Moreover, whereas the statistical tests mentioned over the preceding pages suggest that there was a significant difference in spending on redistribution between the two identity treatments but no similar difference in terms of spending on punishment, they do not show whether the resulting effect of identity on the composition of spending is itself a significant one. Thus, in the more systematic analysis of these results, developed in Section 5.3 below, these are the main questions I deal with.

To provide an initial illustration of this line of reasoning, however, Figure 5.6 plots the average difference between the amount spent on redistribution and

**Figure 5.6:** Relative spending on redistribution and punishment



the amount spent on punishment in the two identity treatments, again centred on the subject mean. As such, negative values mean imply that subjects spent comparatively more on punishment or less on redistribution, and positive values indicate the opposite: that they spent less on punishment or more on redistribution, relative to their individual averages. Once again, this operationalisation allows me to directly assess the effect of identity on relative spending. Compared to the statistics presented above, the data in Figure 5.6 is more unambiguously supportive of my theoretical expectations. When matched with an in-group opponent, the average difference between spending on redistribution and spending on punishment increased by approximately 5.56 ECU, relative to the mean. In the out-group treatment, on the other hand, the difference decreased by 5.98 ECU. Although this is not directly deducible from the figure, the difference between the these two averages is statistically significant at the 95% confidence-level ($t$ = -1.749, $p$ = 0.041). In other words, when subjects played the stealing game against an opponent not belonging to their own identity group, they spent relatively more on punishment and less on redistribution, or equally, they spent more on redistribution and less on punishment when faced with an in-group member. Since in expectation the two treatments were identical in all but the identity of Player 2, moreover, this pattern points to the presence of a causal effect of identity on preferences over alternative means of producing deterrence.

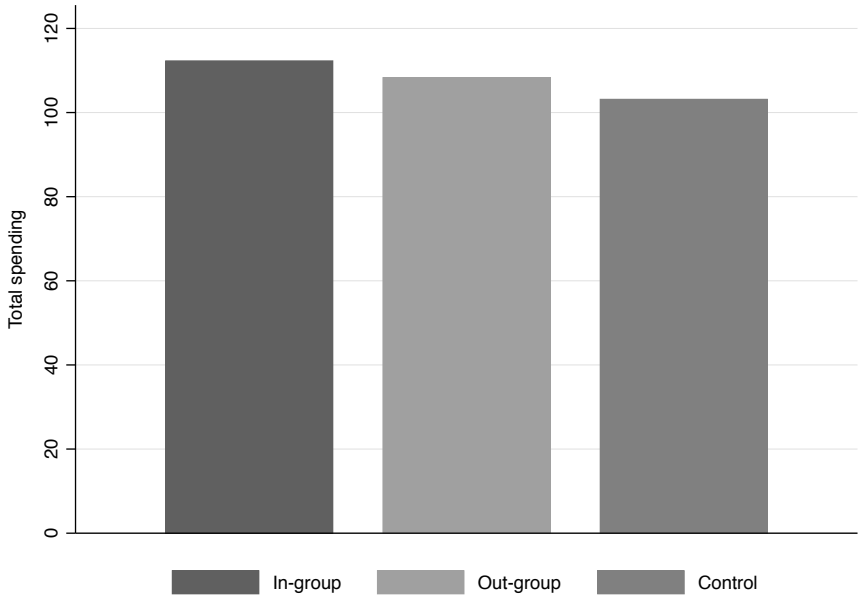
Finally, before moving on to analyse these results more thoroughly, it is important to address a potential complication about the interpretation of these results: the relationship between identity and particularised trust (for discussions, see Rothstein and Uslaner 2005; Evans and Krueger 2009). Admittedly, a possible counterargument to my claim that the observed variation in spending patterns between the two identity treatments has to do with differences in beliefs about the relative efficiency of redistribution and punishment for deterring crime is that they reflect differential levels of trust towards in-group and out-group members. On this logic, subjects may have behaved differently towards in-group members simply because they trusted them more than members of the out-group.

Yet, and without taking this issue lightly, there are several reasons for thinking that trust is not an important part of the explanation for these results. First, although some experimental work has found support for the argument that congruent social identities is conducive to generating trust between individuals, the empirical evidence about this hypothesis is far from unambiguous. Also using

an experimental design with minimal groups, Güth, Levati, and Ploner (2008), for example, find no evidence that identity matters for behaviour in a standard investment game, contrary to expectations if identity itself promotes trust (see Fershtman and Gneezy, 2001; Jin and Yamagishi, 1997; Schopler and Insko, 1992). As a starting point, this means that it is by no means a forgone conclusion that there was a difference in trust between the two identity treatments.

Second, and perhaps more importantly, the implications of the argument that trust was an important driver of subject behaviour in the experiment are not borne out in the data. If higher levels of trust towards in-group members meant that subjects were more likely to expect their opponent not to attempt to steal in the in-group relative to the out-group treatment, it follows that there should be an observable difference between the two in terms of the amount they decided to spend on deterrence. That is, since subjects were always better of spending less rather than more on redistribution and punishment if they expected the other player not to attempt to steal, it is reasonable to expect any difference in trust between the two identity treatments to result in a difference in total spending: if they were more trusting of in-group members, they should have spent less in

**Figure 5.7:** Mean total spending



the in-group treatment. However, no such effect is present. Figure 5.7 illustrates this by plotting average total spending on deterrence in the in-group and the out-group treatment, as well as for the control group without identities. As expected from the discussion above, subjects spent marginally more when playing against an in-group member than they did when their opponent came from the out-group, and in both identity treatments average spending was greater than in the control group. Although neither difference is statistically significant, this pattern is the reverse of that implied by the argument that trust was an important determinant of subject behaviour: had this been the case, subjects should have spent less in the in-group than in the out-group treatment, not the same amount. As such, and while I do recognise potential for trust to be important in theory, there are nevertheless good and persuasive reasons for thinking that such considerations do not play an important part in the explanation for these results.

## 5.3 Multivariate results

Although the simple results discussed above are suggestive, they have limited utility in terms of statistical inference. To that end, this section develops a more formal and systematic analysis of the experimental data. I begin by considering the behaviour of Player 1, emphasising the relationship between identity and relative support for redistribution and punishment given the possibility of victimisation. Using a wide range of statistical approaches, the results from this analysis lend strong support for my main theoretical expectation that identity is a central determinant of preferences over alternative means of deterring crime. Divergent group identities between victims and criminals affected the composition of deterrence spending, away from redistribution and towards punishment. This finding is robust both to different operationalisations of relative spending and to the use of alternative statistical models, and forms the main take-home message of this chapter. In terms of the direct effect of identity on demand for redistribution and punishment, in turn, the results parallel those noted in the informal discussion in Section 5.2: all else equal, demand for redistribution was lower in the out-group than in the in-group treatment, but there are no similar effects in the demand for punishment. In Appendix C, finally, I present an analysis of the behaviour of Player 2, and the determinants of criminal behaviour. The results of this exercise are largely in line with the predictions of the

standard economic model of crime: subjects became more likely to engage in crime as the expected payoff from doing so increased, but less likely to do so as the opportunity cost of crime grew larger. Interestingly, the results also suggests that they were risk-seeking. On the other hand, I find no significant effects of identity on the likelihood that subjects chose to attempt to steal.

### 5.3.1 Identity and relative preferences

As I have argued in Section 5.2 above, the most theoretically relevant way to examine these results is to consider the relationship between identity and relative spending on deterrence, when redistribution and punishment are analysed together. Hence, that is what I do in this section. Given my theoretical argument, I expect that moving from the in-group to the out-group treatment will be associated with a greater reliance on punishment to produce deterrence, either in the form of decreased spending on redistribution or increased spending on punishment, or a combination of the two. In the opposite direction, I expect subjects to have invested comparatively more in redistribution and less in punishment when their opponent was an in-group rather than an out-group member.

More practically, there are a number of alternative ways of operationalising relative spending. An intuitive option is to derive the ratio of spending on punishment to spending on redistribution, or equivalently, the ratio of redistribution to punishment. This approach is inherently problematic, however, since such a ratio cannot be calculated for observations where subjects spent nothing on whichever variable is chosen as the denominator. Consequently, it produces a large number of missing observations, which furthermore are non-random. Given this issue, I instead begin by estimating a series of linear regression model on a dependent variable that measures the difference between the amount spent on redistribution and the amount spent on punishment in each observation (that is, spending on redistribution subtracted by spending on punishment). It thus assumes positive values when spending on redistribution exceeded spending on punishment, and takes on negative values when a subject spent more on punishment than he or she did on redistribution. Zero indicates an equal division of spending between the two options. This operationalisation yields a roughly normally distributed variable, with a mean of $\mu$ = -17.98 and a standard deviation of $\sigma$ = 105.5.

Statistically, I estimate three different regression models. These are reported

in Table 5.3. Model 1 is a standard ordinary least squares (OLS) regression, estimated with cluster corrected standard errors to account for the non-independence of observations from the same subject. These are calculated in a similar way to ordinary Huber-White robust or sandwich variance estimates, but using the number of clusters (in this context, the number of subjects) as the effective number of observations. Since this model corrects for rather than models subject-level variation, it can be seen as a baseline specification. Model 2, in turn, is a mixed-effects random intercept model, which treats all unobserved between-subject heterogeneity as a normally distributed random variable. In practice, this allows each subject to have an individual intercept term (see Rabe-Hesketh and Skrondal, 2012; Snijders and Bosker, 2012). Model 3, finally, is a fixed-effects model, which assumes that the subject-specific effects are a set of fixed (rather than random) parameters. These are then integrated out of the main estimating equation. Thus, and unlike the random-effects specification in Model 2 (which uses both within- and between-subject information for the estimation), Model 3 is a strict within-subject model, and the resulting estimates can be interpreted as measuring the effect of the covariates on deviations from each subject-specific mean (Allison, 2009). To avoid concerns about remaining heteroskedasticity, moreover, both Model 2 and Model 3 are estimated using robust standard errors.

On the right hand side of the equation, Model 1 and Model 2 (as well as most subsequent models discussed in this section) include three independent variables. The first is a dummy representing the identity treatment, coded as one if the two players came from the same identity group, and as zero if they belonged to different groups. In the context of these models, therefore, positive coefficients for this variable indicate a greater relative preference for redistribution over punishment: that is, it shows that the difference between spending on redistribution and spending on punishment was larger, or equally, that is was less negative. The second explanatory variable is the endowment of Player 1, which was proportional to the amount that Payer 2 could steal, in units of ECU. Finally, both models include the probability that an attempt to steal by Player 2 was successful. This is treated as a factor variable with the lowest probability (one-third) as the reference category. Since all time-invariant variation is captured by the subject fixed-effects in Model 3, it includes the in-group identity dummy as the only explanatory predictor.

Turning to the results of this exercise, the estimated effect of identity on relative preferences is consistently in line with my theoretical expectation: across

**Table 5.3:** Determinants of relative spending

| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| In-group | 12.19** | 11.66** | 11.61** |
| | (5.634) | (5.740) | (5.770) |
| Endowment | 0.0707 | 0.0707 | – |
| | (0.186) | (0.186) | |
| Probability of success = 1/2 | -27.40 | -27.41 | – |
| | (31.60) | (31.45) | |
| Probability of success = 2/3 | -26.89 | -26.89 | – |
| | (26.44) | (26.33) | |
| Constant | -74.59 | -74.37 | -23.99*** |
| | (178.9) | (177.9) | (2.990) |
| Subject (s.d.) | – | 74.65*** | – |
| | | (11.48) | |
| Residual (s.d.) | – | 72.74*** | – |
| | | (7.490) | |
| Observations | 440 | 440 | 440 |
| Number of subjects | 44 | 44 | 44 |
| AIC | 12.15 | 11.68 | 11.31 |
| Log likelihood | -2,669 | -2,564 | -2,487 |

DV: Difference between spending on redistribution and punishment.
Model 1 is an OLS model with cluster corrected standard errors.
Model 2 is a linear mixed-effects model with random subject-effects.
Model 3 is a linear mixed-effects model with fixed subject-effects.
Numbers are coefficients. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

all three models, the coefficient for the in-group treatment is both positive and statistically significant at the 95% confidence-level. Substantively, this means that when subjects were matched with an out-group rather than an in-group opponent, they increased spending on punishment relative to what they spent on redistribution. The magnitude of the estimated coefficients is reasonably similar across the models, and range from 12.19 in the baseline specification to 11.61 in Model 3 with subject fixed-effects. Since these are standard linear regression coefficients, they have a straightforward interpretation as marginal effects: thus, they suggest that the difference between spending on redistribution and spending on punishment decreased by approximately 12 ECU when Player 2 was an out-group member, compared to what was the case in the in-group treatment. In the fixed-effects specification in Model 3, the constant term gives the average value of the subject effects, and the coefficient for the in-group treatment can be interpreted as giving the deviation from this mean. Thus, these estimates imply that when all between-subject heterogeneity is disregarded, a typical subject spent approximately 24 ECU more on punishment than he or she did on redistribution in the out-group treatment, compared to a difference of only 12 ECU when Player 2 came from the in-group. Given that overall average level spending in the sample was 59 ECU on punishment and 41 ECU on redistribution, moreover, these effect are arguably not insubstantial.

An alternative method of operationalising relative preferences is to calculate the share of total spending on deterrence that consisted either of redistribution or of punishment in a given observation. This is equivalent to looking at investments in either alternative as a percentage of the total amount a subject decided to spend. Moreover, since the number of observations in which the combined spending on redistribution and punishment was zero is significantly smaller than the number of observations in which spending on either of its constituent parts was zero, this approach avoids some of the problems inherent to using the direct ratio of redistribution to punishment (or punishment to redistribution) as the dependent variable. Still, since subjects in approximately one-quarter of all observations did not purchase a positive amount of either redistribution or punishment, this analysis is inherently limited to the subsample of observations in which this was not the case. A simple z-test suggests, however, that there is no significant difference between the two identity treatments in the likelihood that total spending equalled zero ($z = 1.64$, $p = 0.24$). With this caveat in

mind, therefore, analysing this variable further provides a useful and valuable robustness test of the results derived above, and consequently hedges the analysis against potential idiosyncrasies inherent to either dependent variable.

To that end, I proceed by estimating two further models, explaining the percentage of total spending composed of redistribution (the choice to use redistribution rather than punishment to construct this variable is inconsequential, and the substantive results are the same independently of this decision). These estimates are reported in Table 5.4. Model 1 is a standard OLS regression, again estimated with Huber-White cluster corrected standard errors to account for the non-independence of observations from the same subject. Although this type of regression model is often used to analyse data of this kind—where the dependent variable is a proportion and thus bounded to the unit interval—a potential concern is that it can lead to predictions that are either negative or greater than one. Given this issue, Model 2 is a fractional logit model, or equally, a generalised linear model with a logistic link function and a binomial probability distribution (see Papke and Wooldridge, 1996; Ramalho, Ramalho, and Murteira, 2011; Baum, 2008), estimated by maximum likelihood using robust standard errors. Mathematically very similar to a standard logit regression for binary variables, this model assumes that the dependent variable has a binomial error distribution, and that the conditional mean can be derived as a linear combination of the independent variables through a logistic transformation. In practice, this means that unlike what is the case for OLS specification in Model 1, the predictions from Model 2 have the same theoretical bounds as the dependent variable. Since logistic coefficients are difficult to interpret meaningfully, finally, I report average marginal effects rather than regression coefficients for Model 2.

The results in Table 5.4 lend further support for the expectation that identity is an important determinant of relative demand for redistribution and punishment. Although Model 2 is marginally more efficient, the results from the two models are nearly identical. Most importantly, the effect of the identity treatment is consistently both positive and statistically significant at the 95% confidence-level. This implies that the share of total spending on deterrence that consisted of redistribution declined by about 6 percentage-points when subjects faced an out-group opponent, relative to what was the case when Player 2 was an in-group member. Using the estimates from Model 2 to calculate average predictions, this corresponds to redistribution accounting for approximately 38% of total de-

**Table 5.4:** Determinants of relative spending

| | Model 1 | Model 2 |
|---|---|---|
| In-group | 0.0608** | 0.0608** |
| | (0.0282) | (0.0281) |
| Endowment | 0.000346 | 0.000349 |
| | (0.000683) | (0.000684) |
| Probability of success = 1/2 | -0.137 | -0.137 |
| | (0.120) | (0.119) |
| Probability of success = 2/3 | -0.192 | -0.193* |
| | (0.116) | (0.115) |
| Constant | 0.158 | – |
| | (0.662) | |
| Observations | 284 | 284 |
| Number of subjects | 39 | 39 |
| AIC | 0.768 | 1.138 |
| Log-likelihood | -104.1 | -156.7 |

DV: Redistribution as a fraction of total spending.
Model 1 is an OLS model (numbers are coefficients).
Model 2 is a fractional logit model (numbers are marginal effects).
Cluster corrected standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

terrence spending when Player 2 was an out-group member, compared to 44% when he or belonged to the in-group. This difference is not huge, but it is also not negligible. If subjects believed redistribution to be less efficient at deterring theft by an out-group member, this is precisely the result to be expected. Continuing the pattern seen in the results discussed above, finally, none of the other covariates—neither the probability that an attempt to steal by Player 2 was successful, nor the endowment of Player 1—are statistically significant.

Before moving on, a final way to analyse this data is to look specifically at the extreme bounds of the dependent variable used in these regressions: that is, games in which subjects invested some positive amount in either redistribution or in punishment, but not in both. This is a difficult test for my theoretical argument, since it measures only the strongest possible views that subjects could have about the relative merits of either of the two options for producing deterrence: the belief that the marginal productivity of either option is always strictly greater than that of the other, even around zero. Moreover, since these outcomes represents about 40% of all observations in the dataset, they also merit additional attention in virtue of their frequency. Given my theoretical argument, I expect subjects to have had a higher probability of investing in redistribution only when their opponent was an in-group member, and conversely, a higher probability of investing only in punishment when Player 2 was a member of the out-group.

To that end, I estimate one last set of models, reported in Table 5.5. Model 1 is a standard (binary) logistic regression model, estimated on a dependent variable coded as one if subjects invested in redistribution only, and as zero if they concentrated all of their spending on punishment. Model 2, in turn, is an ordered logistic model, with a dependent variable that includes an intermediate category indicating that subjects spent equal amounts on redistribution and punishment. This outcome represents an additional 20% of all observations with non-zero investment. To simplify interpretation of these estimates, moreover, Table 5.5 reports odds ratios rather than simple regression coefficients. For the binary regression in Model 1, these give the multiplicative effect of a unit change in the independent variables on the odds that a subject invested only in redistribution, rather than only in punishment. The odds ratios for the ordered logit specification in Model 2 carry a similar but somewhat more complicated interpretation: most straightforwardly, they can interpreted as giving the multiplicative effect of a one unit change in the explanatory variables on the odds of being in a higher category,

**Table 5.5:** Determinants of relative spending

| | Model 1 | Model 2 |
|---|---|---|
| In-group | 2.206** | 1.600** |
| | (0.775) | (0.305) |
| Endowment | 1.000 | 1.004 |
| | (0.0102) | (0.00515) |
| Probability of success = 1/2 | 0.451 | 0.622 |
| | (0.520) | (0.484) |
| Probability of success = 2/3 | 0.496 | 0.540 |
| | (0.496) | (0.413) |
| Constant | 0.318 | – |
| | (2.923) | |
| Cut-point 1 | – | 3.233 |
| Cut-point 2 | – | 4.735 |
| Observations | 134 | 193 |
| Number of subjects | 27 | 36 |
| AIC | 1.172 | 2.048 |
| Log-likelihood | -73.54 | -191.64 |

Model 1 DV: 1 = All redistribution, 0 = All punishment.
Model 2 DV: 2 = All redistribution, 1 = Equal division, 0 = All punishment.
Model 1 is a logit model. Model 2 is an ordered logit model.
Numbers are odds ratios.
Cluster corrected standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

that is, on the odds of dividing spending equally rather than allocating it to punishment only, and of concentrating spending on redistribution only rather than dividing it equally. As before, both models use cluster corrected standard errors.

The results shown in Table 5.5 again suggest that identity is an important determinant of relative spending, even when measured in terms of the extreme outcomes where demand for either redistribution or punishment was zero. In both models, the coefficient for the in-group treatment is positive as well as statistically significant at the 95% level, implying that subjects were more likely to invest in punishment only when matched with an out-group opponent. The significant results from Model 2 further indicate that this result holds up when the intermediate category of equal spending is included in the analysis.

To better illustrate the magnitude of these effects, moreover, Table 5.6 shows the average marginal effect of the in-group treatment on the predicted probability of having a specific outcome on the dependent variable. The estimate from Model 1 suggests that when matched with an out-group opponent, subjects were approximately 15 percentage points less likely to invest only in redistribution rather than only in punishment, compared to what was the case in the in-group treatment. This is a reasonably large effect, especially considering

**Table 5.6:** Average marginal effect of in-group status

| | Model 1 | Model 2 |
|---|---|---|
| All punishment | – | -0.114** |
| | | (0.0451) |
| Equal division | – | 0.0467* |
| | | (0.0257) |
| All redistribution | 0.151*** | 0.0669** |
| | (0.0485) | (0.0325) |

Estimates based on the results from Table 5.5.
Two-tailed significance tests:
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

that it corresponds to an average predicted probability of 33% in the in-group treatment, compared to 18% when matched with an out-group member. The marginal effects based on the estimated from Model 2 are more nuanced, but point in the same direction: when Player 2 was an in-group rather than an out-group member, the probability of investing in punishment only declined by about 11.4 percentage points, whereas the probability of dividing spending equally or investing only in redistribution increased by 4.7 and 6.7 percentage points, respectively. Once again, these results have a clear interpretation that is consistent with my argument: shared group identity between the players affected relative preferences, away from punishment and towards redistribution.

### 5.3.2 Identity and redistribution

When taken together, these results strongly suggest that identity has an important effect on relative support for redistribution and punishment as alternative means to deter crime. As subjects moved from the in-group to the out-group treatment, they invested comparatively more in punishment and less in redistribution. The opposite is also true. Moreover, this conclusion is robust to alternative operationalisations of preferences, whether measured in terms of the difference between spending on redistribution and punishment, or in terms the share of total spending on deterrence constituted by either alternative. Once again, the experimental setup further allows for a direct causal interpretation of these effects: since the two identity treatments were identical in all but identity, these results point to the presence of a causal relationship between identity and preferences for deterrence. As such, the experimental results lend persuasive support for the micro-level mechanism at the core of my theoretical logic.

As a supplement to this analysis, however, this section and the next focus on the relationship between identity and preferences for redistribution and punishment when the two are considered separately, rather than jointly. Although this approach is less directly relevant to my theoretical argument than the combined analysis discussed above—since, as I have emphasised in Section 5.2, it involves looking at the relationship between the two identity treatments and demand for either redistribution or punishment, while ignoring simultaneous variation in the other—it nevertheless serves as a useful extension to the former in allowing for a direct assessment of the lower-level variation that is driving these main results.

Whereas the findings from the previous section show that identity had a large and significant effect on the overall composition of deterrence spending, they say less about whether these trends are the result of variation having to do with redistribution or variation having to do with punishment, or both. Given this, I begin by analysing the relationship between identity and demand for redistribution in this section, before turning to consider the effect of identity on demand for punishment. As indicated in Section 5.2, the results suggest that the effects noted in the previous section are driven mainly by spending on redistribution.

To that end, Table 5.7 presents the results from a series of Poisson regressions explaining spending on redistribution, as a function of the same set of independent variables used for the models discussed above: the identity treatment, the endowment of Player 1, and the probability that an attempt to steal by Player 2 was successful. Since the dependent variable in these models measures spending in units of ECU, it is both strictly integer valued and positive. As mentioned before, it also has a reasonably large number of zero-counts, suggesting that several of the assumptions underlying the standard linear regression model are unrealistic. Hence, the choice to use models specifically developed for count data.

Model 1 is a standard Poisson model, estimated by maximum likelihood and using Huber-White cluster corrected standard errors to account within-subject dependence. Like the logistic model considered earlier, the Poisson regression model can be derived as a generalised linear model (Hilbe, 2012), with a Poisson response distribution and a logarithmic link function (that is, it models the conditional mean of the distribution as an exponentiated combination of the independent variables and their coefficients). To simplify interpretation, therefore, Table 5.7 reports exponentiated regression coefficients, or equally, incidence-rate ratios. Conceptually similar to odds ratios, these give the multiplicative effect of a unit change in the independent variables (or, in the case of factor variables, a discrete change from the reference category) on the predicted number of ECU spent on redistribution. Incidence-rate ratios above one thus represent positive relationships, while those below one indicate negative effects.

A number of interesting points emerge from these results. The first two are negative: neither the endowment of Player 1, nor the likelihood of success for Player 2 are estimated as having significant effects on demand for redistribution. The absence of an effect for the endowment of Player 1 is surprising, since in theory, subjects with larger endowments had to spend more on deterrence in order to

**Table 5.7:** Determinants of demand for redistribution

| | Model 1 | Model 2 |
|---|---|---|
| In-group | 1.271** | 1.264*** |
| | (0.139) | (0.0191) |
| Endowment | 1.002 | 1.004 |
| | (0.003) | (0.00925) |
| Probability of success = 1/2 | 1.027 | 2.323 |
| | (0.470) | (3.370) |
| Probability of success = 2/3 | 0.829 | 7.436 |
| | (0.379) | (10.03) |
| Constant | 7.961 | 0.0481 |
| | (24.43) | (0.481) |
| Subject (s.d.) | – | 3.644*** |
| | | (0.514) |
| Observations | 440 | 440 |
| Number of subjects | 44 | 44 |
| AIC | 93.81 | 32.55 |
| Log likelihood | -20,653 | -7,156 |

DV: Spending on redistribution by Player 1 in ECU.
Model 1 is a Poisson model with cluster corrected standard errors.
Model 2 is a mixed-effects Poisson model with random subject-effects.
Numbers are incidence rate ratios. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

disincentivise Player 2 from attempting to steal. On the other hand, the insignificant effect of the probability of success is more easily explicable, and indicates that subjects did not make any systematic assumptions about the risk preferences of their opponents. More importantly, however, the effect of identity is both positive and statistically significant at the 95% confidence-level, suggesting that demand for redistribution declined when Player 2 came from the out-group, compared to what was the case in the in-group treatment. The magnitude of this effect is furthermore quite large, and corresponds to an estimated average marginal effect of 9.8 ECU, against the overall predicted mean spending of 41.2 ECU.

Following the logic of the analysis of relative spending, another approach of dealing with the panel structure of the data is to model the incidence of between-subject heterogeneity more directly. To that end, Model 2 in Table 5.7 reports the results from a mixed-effects Poisson regression that includes subject-level random intercepts. Once again, and unlike the cluster robust specification used for Model 1, this model accounts for the correlation between observations from the same subject by including a subject-specific, normally distributed random parameter in the linear predictor (Hilbe, 2012). It is thus equivalent to Model 3 in Table 5.3. Two specific points, moreover, should be noted about this model. First, since the two coincide by definition, the estimates reported for Model 2 in Table 5.7 have both a subject-specific and a population-averaged interpretation. This means that they are directly comparable to the coefficients from Model 1. Second, and perhaps more importantly, this also means that consistent estimation of the coefficients does not depend on the correct choice of distribution for the random intercepts, in contrast to what is the case in, for example, the random intercept logistic regression model (see Rabe-Hesketh and Skrondal, 2012).

The results from Model 2 to are broadly similar to those from Model 1. The estimated effect of the in-group treatment on demand for redistribution remains both positive and statistically significant, suggesting that subjects spent less on redistribution when faced with an opponent not belonging to their own identity group, compared to what was otherwise the case. The magnitude of this effect is also consistent with that estimated in Model 1, and indicates that spending on redistribution was approximately 26% higher in the in-group relative to the out-group treatment, all else equal. On the other hand, neither of the other covariates reach any conventional level of significance. As expected, finally, the estimate of the subject-level variance is highly significant, indicating that there

was considerable between subject heterogeneity in their propensity to invest in redistribution, even when conditioning on the other covariates of the experiment. A standard likelihood ratio test of the null hypothesis that the subject-level variance is zero gives a test statistic of $\chi^2 = 26.959$ with a p-value of $p < 0.01$.

Moving on, a significant limitation of the Poisson model is that it relies on the distributional assumption, itself inherent to the Poisson distribution, that the conditional mean (or equally, the expected count on the response variable) is equal to its variance. Thus, the greater the predicted value of the dependent variable, the greater the variance around that estimate is assumed to become. When this assumption does not hold and the variance exceeds the conditional mean, the data is said to be overdispersed (although underdispersion is equally problematic, it is rarely encountered in practice). As is well known, not accounting for overdispersion when it is present generally leads to underestimated standard errors, and consequently to incorrect inference tests. Moreover, and although both techniques used above—first, a cluster robust variance estimator, and second, modelling subject-level heterogeneity directly through a random subject-level parameter (which in turn allows for the assumption of a direct equivalence between the conditional mean and the conditional variance to be relaxed)—are both sufficient for dealing with overdispersion caused by the correlation between observations from the same subject, there is still a risk that some additional extra-Poisson variance remains even when these measures are included (for discussions, see Rabe-Hesketh and Skrondal, 2012; Hilbe, 2012).

To address this concern, I proceed to estimate a series of negative binomial (or, more precisely, NB2) regression models by maximum likelihood, reported in Table 5.8. This type of negative binomial model can described as a generalisation of the standard Poisson regression model, in which the conditional variance is free to assume values greater (or smaller) than the conditional mean. This allows for any residual overdispersion to be modelled directly, rather than only corrected for. More formally, the NB2 model is traditionally derived from a Poisson-Gamma mixture distribution, where observations are assumed to differ on some latent variable giving rise to overdispersion, which in turn is taken to follow a Gamma probability density function (Long, 1997; Cameron and Trivedi, 1998). However, and like the Poisson model, it can also (and more intuitively) be characterised as a generalised linear model with a negative binomial response distribution and a logarithmic link function. In this specification, the

NB2 regression model thus has a mean structure very similar to that of a standard Poisson model, but with a variance function that includes an additional and separate overdispersion parameter, which in turn is estimated directly from the data (Hilbe, 2012; Cameron and Trivedi, 1998). Aside from this difference, however, the interpretation of the model results remains the same.

As a test of the robustness of the results derived above, Table 5.8 reports the results from three distinct NB2 specifications. Model 1 is a standard pooled model estimated with cluster corrected standard errors. Model 2, in turn, is a mixed-effects model with normally distributed subject-level random intercepts. As such, these models are direct equivalents of Model 1 and Model 2 in Table 5.7. Model 3, finally, includes (unconditional) subject fixed-effects. As in any fixed-effects model, this specification treats all subject-level effects a set of fixed parameters, which in turn absorb all (observed and unobserved) time-invariant heterogeneity between subjects. This means that the model has the same pure within-subject interpretation as the linear fixed-effects model discussed in Section 5.3.1, again at the cost of not being able to estimate the impact of observed covariates that vary only between subjects. Finally, and following the recommendation from Hilbe (2012), the standard errors in Model 3 are inflated by the Pearson $\chi^2$ statistic divided by the residual degrees of freedom (for a discussion of this approach, the various problems inherent to the alternative unconditional fixed-effects negative binomial model, and the absence of an incidental parameters problem for this specification, see Allison and Waterman 2002; Greene 2001).

Turning to these results, across all three negative binomial models the estimated effect of the in-group treatment remains the same as in the Poisson regressions: when both subjects belonged to the same identity group, Player 1 increased spending on redistribution. In Model 1 (with clustered errors), this effect is significant at the 95% level, and in Model 2 (random effects) and Model 3 (fixed effects) at the 99% level. Interestingly, the estimated magnitude of the effect of in-group status on spending is also significantly larger than in the previous estimates, especially in the random-effects specification in Model 2 and the fixed-effects model in Model 3. In the former, the purported incidence rate ratio is 2.8, compared to 3.2 in the latter. Given that these are multiplicative effects, the impact of identity on preferences appears to be all but negligible: the fixed effects estimate suggests that subjects who spent at least some amount on redistribution in one of the periods increased spending more than threefold when

**Table 5.8:** Determinants of demand for redistribution

| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| In-group | 1.308** | 2.838*** | 3.216*** |
| | (0.152) | (0.828) | (0.933) |
| Endowment | 1.004 | 1.004 | – |
| | (0.003) | (0.007) | |
| Probability of success = 1/2 | 1.006 | 2.311 | – |
| | (0.460) | (2.717) | |
| Probability of success = 2/3 | 0.728 | 7.325* | – |
| | (0.328) | (8.136) | |
| Constant | 5.771 | 0.021 | 27.47*** |
| | (13.17) | (0.136) | (16.68) |
| Subject (s.d.) | – | 3.74*** | – |
| | | (2.122) | |
| Observations | 440 | 440 | 320 |
| Number of subjects | 44 | 44 | 32 |
| AIC | 6.44 | 6.17 | 8.131 |
| Log likelihood | -1,412 | -1,353 | -1,268 |

DV: Spending on redistribution by Player 1 in ECU.
Model 1 is a negative binomial model with cluster corrected standard errors.
Model 2 is a mixed-effects negative binomial model with random subject-effects.
Model 3 is a mixed-effects negative binomial model with fixed subject-effects.
Numbers are incidence rate ratios. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

their opponent belonged to the same group as themselves, compared to when he or she did not. As before, on the other hand, none of the other covariates have significant effects. Notably, these models also appear to fit the data better, as expected given the potential issue of overdispersion in the dependent variable.

### 5.3.3 Identity and punishment

In this section, I develop an analysis similar to that presented above, but focused on the direct relationship between identity and demand for punishment, rather than identity and demand for redistribution. Whereas the results derived above suggest that changes in the level of spending on redistribution forms an important part of the explanation for the difference, as discussed in Section 5.3.1, in the composition of spending on deterrence between the two identity treatments, they say little about whether the same conclusion holds true for punishment. Hence, the objective in this section is to establish whether or not this is indeed the case.

At the outset, it should be acknowledged that there is no evidence for a systematic or robust relationship between identity and spending on punishment: independently of the statistical model used, the effect of identity on the amount invested in punishment remains very small, and is more often than not well within the statistical margin of error. Given the descriptive patterns shown in Section 5.2, this result is hardly surprising. Of course, whilst this does not contradict the argument that identity is an important determinant of preferences for deterrence, it does show that this effect works predominantly through variation in demand for redistribution. More practically, the analysis below follows a similar structure to that used in the preceding discussion of demand for redistribution. The dependent variable is the number of ECU spent on punishment, meriting the use of count models. To that end, I estimate the same set of regression models as in the previous section: first a set of pooled and mixed-effects Poisson models, and second a series of negative binomial models with both random and fixed subject effects. For all models, I again report incidence-rate ratios.

Table 5.9 presents the results from the initial Poisson regressions. As before, Model 1 is a standard pooled model, estimated with cluster corrected standard errors to account for the non-independence of observations from the same subject, and Model 2 is a random-intercept model, in which the same objective is achieved by allowing each subject to have a separate and random intercept

**Table 5.9:** Determinants of demand for punishment

| | Model 1 | Model 2 |
|---|---|---|
| In-group | 0.960 | 0.966*** |
| | (0.0584) | (0.0120) |
| Endowment | 1.000 | 1.009 |
| | (0.00204) | (0.00746) |
| Probability of success = 1/2 | 1.604 | 2.637 |
| | (0.642) | (3.035) |
| Probability of success = 2/3 | 1.413 | 5.007 |
| | (0.507) | (5.389) |
| Constant | 47.66* | 0.00293 |
| | (96.06) | (0.0198) |
| Subject (s.d.) | – | 2.945*** |
| | | (0.393) |
| Observations | 440 | 440 |
| Number of subjects | 44 | 44 |
| AIC | 100.3 | 40.79 |
| Log-likelihood | -22,067 | -8,969 |

DV: Spending on punishment by Player 1 in ECU.
Model 1 is a Poisson model with cluster corrected standard errors.
Model 2 is a mixed-effects Poisson model with random subject-effects.
Numbers are incidence rate ratios. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

term. These models are direct equivalents of Model 1 and Model 2 in Table 5.7 above. In terms of the main effect of interest—the relationship between identity and spending—these results are ambiguous. In Model 1, the estimated coefficient for the in-group treatment is negative, but fails to reach any conventional level of statistical significance. In Model 2, on the other hand, the effect of shared group membership is similar in both magnitude and direction, but is also statistically significant at the 99% confidence-level. This difference parallels that seen for redistribution in Table 5.7, where introducing subject-specific random intercepts likewise led to an increase in efficiency (which is unsurprising given the apparent large degree of between-subject heterogeneity). The same holds true for the model fit, which increases dramatically as between-subject heterogeneity is modelled directly rather than simply adjusted for in the estimation of the standard errors. At face value, the significant effect of the in-group treatment suggests that identity has the predicted implications for preferences over punishment: when matched with an out-group opponent, subjects spent more on punishment than they did in the in-group treatment. Still, it is important to emphasise that the estimated magnitude of this effect remains reasonably small, indicating that subjects lowered spending on punishment by approximately 3.4% when playing against someone from their own group, compared to what they did when their opponent was an out-group member.

These results should be interpreted with some degree of caution, however. As I have discussed in some detail above, the Poisson distribution (and hence, regression model) assumes that the conditional mean and the conditional variance are equal. In the random-intercept specification in Model 2, this assumption is relaxed since the inclusion of random effects with a variance that is significantly different from zero (as it is here) not only permits but also forces the conditional variance to exceed the conditional mean (Rabe-Hesketh and Skrondal 2012; also Hilbe 2012). As such, although the model is inherently overdispersed, this is not necessarily an issue. Still, since the model uses a single parameter to account both for overdispersion at the observational level and for clustering within subjects, it is theoretically possible that additional overdispersion remains even when the random subject effects are included in the model. If this is the case, in turn, there is a risk that the error terms in Model 2 are underestimated.

Given this issue, I therefore once again proceed to estimate a series of negative binomial regression models, allowing me to test the robustness of this result.

**Table 5.10:** Determinants of demand for punishment

| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| In-group | 0.933 | 1.063 | 1.081 |
| | (0.0581) | (0.209) | (0.198) |
| Endowment | 0.999 | 1.009 | – |
| | (0.00256) | (0.00651) | |
| Probability of success = 1/2 | 1.624 | 2.643 | – |
| | (0.625) | (2.659) | |
| Probability of success = 2/3 | 1.412 | 5.056* | – |
| | (0.509) | (4.754) | |
| Constant | 71.65* | 0.00301 | 43.83*** |
| | (177.2) | (0.136) | (21.65) |
| Subject (s.d.) | – | 2.905*** | – |
| | | (1.560) | |
| Observations | 440 | 440 | 360 |
| Number of subjects | 44 | 44 | 36 |
| AIC | 8.23 | 7.95 | 9.38 |
| Log likelihood | -1,804 | -1,742 | -1,651 |

DV: Spending on punishment by Player 1 in ECU.
Model 1 is a negative binomial model with cluster corrected standard errors.
Model 2 is a mixed-effects negative binomial model with random subject-effects.
Model 3 is a mixed-effects negative binomial model with fixed subject-effects.
Numbers are incidence rate ratios. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

These models are reported in Table 5.10. To recapitulate very briefly, the negative binomial regression model can be thought of as a direct extension of the standard Poisson model, and includes a variance function with a shape or overdispersion parameter that allows for the conditional variance to differ from the conditional mean. As in Section 5.3.2, I estimate three different types of model: Model 1 is a straightforward pooled model estimated with cluster robust standard errors, Model 2 is a mixed-effects model with normally distributed random subject-level intercepts, and Model 3 is an unconditional fixed-effects model.

The results in Table 5.10 confirm the scepticism regarding the significant result from the Poisson estimation. In neither of the negative binomial regressions—either estimated on the pooled sample with the cluster robust sandwich estimator, or with random or fixed subject-effects—does the dummy for in-group games reach any common level of statistical significance (with $p$-values for the $z$-statistic of $p = 0.265$, $p = 0.755$, and $p = 0.672$, respectively). The point estimates of the effect also vary widely: in Model 1, the effect is taken to be negative, whereas in Model 2 and Model 3 it is estimated as being positive. Given their extremely wide confidence intervals, however, these differences carry no real analytic value. As such, the conclusion noted above stands: there is no significant relationship between identity and spending on punishment, suggesting that the observed differences in relative spending derives from variation in the demand for redistribution. Similarly to the results for redistribution, finally, the estimated subject-level variance in Model 2 is again significant, indicating that there is substantial between-subject variation in terms of spending on punishment.

## 5.4 Summary

In sum, this the results from the experiment show that in a controlled setting that allows for causal inference, social identity has significant effects on preferences over alternative means of producing deterrence. As the results above have shown, shared group membership between a potential victim and a potential perpetrator of crime induces the former to rely comparatively more on redistribution but less on punishment, compared to what is the case when both the victim and the perpetrator belong to the same identity group. This finding is highly consistent with the results from the empirical analysis based on observational data developed in Chapter 3 and Chapter 4—in both cases, evidencing a significant effect

of population heterogeneity on the relationship between fear of crime and demand for redistribution, on the one hand, and views about law enforcement and punishment, on the other—and thus add meat to the bones of my argument that these macro-level results are fundamentally driven by social identity concerns.

At the same time, it is also clear that these findings are driven largely by changes in spending on redistribution, rather than changes in spending on punishment. It remains an open question what explains this result. One possibility, of course, is that this result is the product of the experimental design. It could be argued, for example, that for potential victims in the stealing game, redistribution was a simpler option than punishment: whereas the former did not require any calculations, the latter necessitated some mental arithmetic. Exploring whether this result holds up in alternative experimental settings that do not suffer from this disadvantage is a natural continuation of the work begin here.

# Chapter 6

# Conclusion

This dissertation began with two distinct empirical puzzles: one about cross-sectional variation in support for redistribution among the affluent, the other about differences in punitive preferences and attitudes towards crime control. Why is it that significant shares of the affluent are in favour of redistribution, against their material-self interest, in some places but not in others? And similarly, what explains the observation that public opinion in some countries—and in some regions within countries—favours 'tougher' and more punitive means of dealing with criminals, but opposes the same set of policies elsewhere?

My argument has suggested that these observations are related, and jointly explained by the importance of fear of crime and population heterogeneity for preferences over alternative means of reducing crime. Heterogeneity and social identity matter because they affect how criminal behaviour is interpreted, with consequences for views about how crime as a social problem can be solved. As I have argued, when the poor share a salient identity trait with the more affluent, the latter are more likely to draw a causal inference from inequality to crime, and hence more likely to demand redistribution as a solution for it. When the poor are more heterogeneous, by contrast, the affluent are more likely to explain crime with reference to dispositional traits unrelated to inequality, and consequently more likely to support punitive solutions to crime, rather than redistribution. Taken as a whole, this model this speaks to both empirical patterns

noted in the previous paragraph. On the one hand, support for redistribution from high-income groups can be explained by reference to fear of crime, but only in geographical contexts that are sufficiently homogenous. And on the other, variation in support for punitive criminal justice policies can similarly be explained by identity: when criminals are perceived as 'others', demand for harsher punishment and increased law enforcement efforts increase. This context dependent relationship between fear of crime and preferences—and the implication that there exists a real trade-off between demand for redistribution and demand for security—is perhaps the main take-home message of this dissertation.

Viewed collectively, the empirical results presented above has produced strong support for these expectations. Using European data, the analysis developed in Chapter 3 showed that fear of crime has widely divergent effects on preferences in different contexts. In counties with small foreign-born population shares, being afraid of crime significantly increases support for redistribution, but has no effect on punitive attitudes. In countries that are more diverse, the opposite holds true: fear of crime has a large, positive effect support for harsher punishment, but does not matter to redistribution preferences. On the local level, in turn, the results from Chapter 4 confirmed that a virtually identical pattern exists in the US. As the black population share grows, fear of crime matters less and less to demand for redistribution from white Americans, but becomes an increasingly important predictor of support for increased spending on law enforcement. In both cases, these results proved highly robust to alternative theoretical explanations and practical modelling choices. In a controlled setting that allows for more robust causal inference, finally, the experimental evidence presented in Chapter 5 confirmed the importance of social identity as a key mediating variable between fear of crime and demand for alternative means of producing deterrence.

The causal relationship between inequality and fear of crime, moreover, gives this conclusion wider importance. As I have discussed at some length in both Chapter 1 and Chapter 2, the debate about how inequality and redistribution are related under democracy features—and has done so for a long time—at the heart of much comparative political economy scholarship. The theory and empirics presented in the preceding chapters suggests that by ignoring externality effects such as fear of crime, and the processes that link such concerns back to preferences, the previous literature has overlooked a key causal mechanism in the inequality-redistribution nexus. Indeed, an important implication of the argument proposed

here is that while inequality, through fear of crime, can increase support for redistribution among the affluent, this effect is mediated by heterogeneity. It is possible, therefore, that the conflicting results of previous studies investigating the relationship between relative income, inequality and demand for redistribution stems, at least in part, from the omission of heterogeneity as a key intermediate variable.

Likewise, the findings of this dissertation also speak to the large, growing literature on how population heterogeneity, immigration and other concerns related to social identity affects redistribution preferences. On one level, they confirm what we perhaps already knew: heterogeneity and support for redistribution are negatively related. On the other hand, they challenge existing accounts of this relationship, in at least two ways. First, in highlighting the importance of heterogeneity for how fear of crime affects redistribution preferences, unlike most previous arguments the model proposed here does not rely on altruism. Rather, heterogeneity matters because it affects beliefs about the causes of crime, which in turn has implications for views about how it can be reduced. Second, in emphasising the connection between heterogeneity and the redistribution preferences of the affluent, the argument and findings of this dissertation joins a growing body of research challenging the conventional wisdom that heterogeneity first-and-foremost affects the preferences of comparatively low-income individuals. As I the results derived throughout this dissertation have shown, while the redistribution preferences of the poor are not unaffected by the level of heterogeneity, this effect is substantively small relative to that observed for higher income individuals.

In a separate direction, the preceding chapters also contribute to the comparative literature on punitive attitudes, and hence help to build an understanding of cross-national differences in criminal justice policy—an arguably understudied topic in political science more generally. As I have discussed in Chapter 2, existing research on the empirical relationship between fear of crime and punitive preferences has produced largely inconsistent results: while some studies find evidence for a positive relationship between fear of crime and support for harsher punishment or increased spending on law enforcement (Langworthy and Whitehead, 1986; Costelloe et al., 2002; Costelloe, Chiricos, and Gertz, 2009; Dowler, 2003, for example), many others have arrived at the opposite conclusion (see Tyler and Boeckmann, 1997; King and Maruna, 2009; Kleck and Jackson, 2016; Barkan and Cohn, 1994). As with the relationship between inequality and redistribution preferences, the argument and findings of this dissertation suggests that these in-

consistencies likely stem from the omission of identity as a mediating factor. Fear of crime can indeed generate support for law enforcement and punishment, but only in contexts that are sufficiently diverse. Although forms of this argument have been made before, such contributions have almost exclusively been limited to the US, both empirically and theoretically (King and Wheelock, 2007). I have shown, however, that there are good reasons for thinking that the US is not unique in this regard: observationally, the same pattern is evident in Europe, and experimentally, it exists in a setting with exogenously induced, artificial social identities.

Less academically, these results further shed light on a number of highly salient contemporary political phenomena. The implication of my argument that high inequality and high heterogeneity represents a potentially virulent combination, weakening demand for redistribution but boosting support for 'tougher' means of dealing with crime, clearly resonates with recent political developments on either side of the Atlantic. In Europe, increasing levels of population heterogeneity—especially over the last decade, following the Syrian refugee crisis—has gone hand-in-hand with a growing political emphasis on traditional law-and-order policies, first on the populist right and more recently among more mainstream parties. From Sweden and Germany to Greece and Italy, the same pattern is evident. Sweden provides a particularly pertinent case in point. Between 2010 and 2018, the foreign-born population share increased dramatically—by about one-third—from 14.1% of the population to more than 17.8%. Over the same period, the share of voters naming crime as the most important problem facing the country nearly doubled (Martinsson and Andersson, 2018), allowing law-and-order issues to emerge as a (if not the) central theme ahead of the 2018 parliamentary elections. In the US, on the other hand, the alleged link between immigration in general, and illegal immigration in particular, and crime has featured prominently in political debates in recent years. Without denying that supply-side factors matter for explaining these outcomes, it is notable that they are highly consistent with the implications of my argument: inequality combined with heterogeneity creates demand for 'tough on crime' policies, which parties and other political actors can tap or exploit to their advantage.

Nonetheless, it is also important to be open and honest about the limitations of the empirical analysis developed above. For one, and as discussed throughout the preceding empirical chapters, not all findings are in line with expectations. In both Chapter 3 and Chapter 4, while the results for the affluent largely conform

to my theoretical predictions, the same cannot be said about the relationship between fear of crime and the preferences of lower income individuals. It is interesting to note, however, that the behaviour of the poor in response to fear of crime is rather different in Europe and the US. In the analysis of European data in Chapter 3, first, I find although fear of crime never makes substantive difference to the redistribution preferences of low-income individuals, it is associated with a both large and highly statistically significant positive effect on support for harsher punishment, provided that the level of heterogeneity is sufficiently low. Using American data in Chapter 4, on the other hand, the opposite holds true: for the poor, fear of crime does not affect law enforcement preferences, but has a significant positive effect on support for redistribution in contexts that are sufficiently heterogenous. The preceding chapters have offered possible explanations for these outcomes—focusing on group-based punishment in response to criminal victimisation and residential sorting based on attitudes towards minorities, respectively—but it remains an open question why these effects appear to be so different on either side of the Atlantic. In preliminary work focusing on sub-national regions rather than countries in Europe, I find that the patterns discussed above—where fear of crime has a positive effect on the punishment preferences of the poor, but only when the level of heterogeneity is low—remains largely unchanged, suggesting that the answer to this question does not lie with the choice of macro-unit. Exploring these patterns further nonetheless forms a natural continuation of the work presented here.

Additionally, it should also be acknowledged that this dissertation's empirical analysis is, in part, severely constrained by the limited range of available data. In particular, a critical reader may point out that while I do demonstrate the effect of social identity on preferences for redistribution and law enforcement as alternative solutions to crime, I do not show that this relationship derives from the distinction between instrumental and dispositional explanations for criminal behaviour, as my theory suggests is the case. This critique—that the analysis developed above 'skips over' one causal mechanism, going directly from identity to preferences, rather than from identity to beliefs about the causes of crime, and subsequently, only from such beliefs to preferences—is admittedly justified. The reason for this shortfall is, above all, practical: neither the ESS nor the GSS includes items that would allow me to analyse this expectation directly, a conclusion that also holds true for other, similarly large-scale com-

parative datasets—which, as I have argued above, also suffer from a number of additional limitations in the context of my argument. In further work, I plan to address this deficiency using more fine-grained subnational data. This would allow me to test this general expectation—that shared group membership increases the reliance of instrumental explanations of criminal behaviour, while divergent identities makes dispositional explanations more important—more directly. One possibility that I am currently in the process of exploring is to use the Swedish National Election Survey combined with municipality-level data on mandatory immigrant and refugee placements, which provides exogenous variation in the local level heterogeneity. This basic design would thus parallel that of Dahlberg, Edmark, and Lundqvist (2012), but with a different substantive focus and using data from a different time-period, allowing me to consider changes in heterogeneity resulting from the large rise in immigration over the last few years, as mentioned above. A survey-experiment would also lend itself well to this task, as would an additional laboratory experiment.

These empirical limitations aside, the recent increase in immigration to Europe, mostly as a result of ongoing conflicts in the Middle-East, could also be used to test the empirical implications of my argument more generally. Not only has the influx of migrants differed hugely between otherwise similar countries—compare Sweden and Denmark, for example, or France and Germany—but perhaps more interestingly, the geographical distribution of immigration between different subnational regions also follows some interesting—and from a statistical perspective, useful—patterns. In both Sweden and Germany, for instance, recent immigrants have been allocated between municipalities and states, respectively, on a fully proportional basis: each subnational unit has been required to accommodate a share of new arrivals based, above all, on its pre-existing population size. This fact, in turn, provides good grounds for assuming that the placement of refugees, and the resulting change in the local level of population heterogeneity, is exogenous both to previous attitudes to immigrants, and with respect to preferences for either redistribution or public security. Thus, exploiting this variation to test the main argument developed above would be a useful next step.

More theoretically, it is also clear that my argument can be extended in a number of directions, beyond what I do here. As discussed in Chapter 2, the perhaps most obvious route for doing so relates to the joint issues of geographic heterogeneity in the provision of redistribution and security and residential sort-

ing. Relaxing these assumptions—first, that redistribution and security are public goods, and second, that there is no endogenous sorting, either on crime or on local policies implemented to reduce crime—opens up a number of possibilities. First, and even ignoring differences in local policy provisions, it is possible to think of residential sorting as an additional strategy—over and above more redistribution, more policing, and more punishment—available as a response to fear of crime. This analytical move takes the argument beyond the framework of the standard economic model of criminal behaviour—which emphasises possible changes to the policy variables that affect individual-level incentives to engage in crime—to account for the fact that another way of 'dealing' with fear of crime is simply to escape it. Developing the argument in this direction would, in turn, shift attention towards the factors that determine the feasibility of residential sorting, on the one hand, and the relationship between residential sorting and patterns of population heterogeneity, on the other.

Moving beyond the pubic goods assumption, in turn, leads to a perhaps even richer set of possibilities. As I have discussed above, it is possible to argue that many common forms of both redistribution and public security are excludable—consider public schools, which cater to residents of a certain geographical area but exclude outsiders, for example, or local police departments. This in turn implies geographical heterogeneity in the provision of these services, with two important consequences. First, it reinforces the incentives for residential sorting, since it allows individuals to choose a place of residence based on the bundle of public services that are provided locally. In other words, it introduces of the option of 'voting with one's feet', which is not available when these goods (or perhaps, services) are provided more centrally. And second, it opens the door for more dynamic arguments about the relationship between the local supply of redistribution and security, residential sorting, property prices and crime levels. What might such an argument look like? One alternative emphasises the risk of stratification. Consider, for example, a scenario in which local jurisdictions differ in the goods they provide for the purpose of reducing crime—whether through policing, for instance, or redistributive programs that make crime less attractive for the least well off—with the result of a corresponding variation in the crime rate. If individuals sort based on these differences, we would expect demand for housing in low crime jurisdictions to increase, resulting in geographical stratification based on income. And given the relationship between income and crime, in turn, high

property prices should result in less crime, because it limits the local population to those who are the least likely to engage in criminal activities. The outcome, therefore, is one of segregation into low-crime, high-income jurisdictions, on the one hand, and high-crime, low-income neighbourhoods, on the other.

To integrate this logic with the main argument of this dissertation, several points would need to be considered. First, it is important to keep in mind that crime is not necessarily confined to a particular place or jurisdiction. On this point, the logic of sorting based on crime and the policies locally implemented to reduce it is quite different from that emphasised, for example, in the large literature on sorting based on school provision: whereas eligibility to a school can (and often is) geographically restricted, individuals can (and again, often do) engage in crime outside of the area in which they reside. This suggests, in turn, that for the affluent, the identity of low income individuals living in areas outside of their own should matter for their preferences over alternative means of reducing fear of crime, along the lines that I have argued throughout the preceding chapters. Following the same logic, moreover, it is also reasonable to expect local clubbing in response to fear of crime to be more common on the security side of my argument—since protection is, by definition, centred on potential victims, and thus something that can be provided locally—than for policies aimed at reducing inequality, which are oriented around potential perpetrators of crime, who may reside elsewhere. If these intuitions are correct, we should thus observe greater local variation in law enforcement efforts in places that are more diverse, but not necessarily more local variation in redistributive policies in places that are more homogenous—an expectation that, on a first glance, does not seem implausible.

Second, greater attention would also have to be paid to cross-sectional differences in the extent to which policies aimed at reducing inequality, and more importantly, the provision of law enforcement and security are centralised or decentralised. Like fiscal institutions more generally, countries differ widely in the extent to which not only law enforcement but criminal justice in general are supplied and financed locally, regionally or at some higher level of government. If the above reasoning holds true, in turn, these differences are consequential, since they shape the strategies available to the affluent in response to fear of crime. And finally, the relationship between public and private security might have to be reconsidered. Indeed, it is interesting to note that if public security is thought of as excludable, the theoretical distinction between these two alternatives becomes

less clear cut—a pertinent point when considering the implications of my argument outside of Western Europe and the US. These are fascinating possibilities, both theoretically and empirically, that I look forward to exploring in future work.

On a more general level, finally, the findings of this dissertation also direct attention in several directions. One again concerns the supply-side of politics. For redistribution preferences, a perhaps unintuitive implication of my argument is that support for redistribution from the affluent will be the highest in the places that are the least diverse but the most unequal. This naturally raises the question of how support for redistribution can be high where inequality is also high—an issue that points to the importance of yet to be delineated supply-side, structural factors in explanations of redistributive outcomes. Indeed—and more generally—while the recent growth in scholarship centred on the demand-side of the politics of inequality and redistribution (that is, arguments aimed at explaining variation in redistribution preferences) is impressive, we know much less about what these micro-level insights mean for macro-level outcomes (such as actual levels of redistribution and patterns of inequality). Bridging this divide should be high up the research agenda in comparative political economy.

The other side of my argument—about punitive attitudes—also opens up space for further work. As I have argued in Chapter 1, the scant interest in crime from a political science perspective is both surprising and unfortunate. There is a persuasive argument to be made for considering both crime and criminal justice as political outcomes, whether indirectly—through inequality, for example—or directly, through actual policy choices in the areas of law enforcement and punishment. Yet, despite this we know comparatively little about what shapes these processes. As Lacey and Soskice (2015, pp. 456) very accurately point out, although much of the existing literature on this topic 'goes forward in terms of discussions of the politics of race and crime, the politics of law and order, or punishment and democracy', rarely does it 'scrutinise the relevant preferences and interests, and the political institutions through which criminal justice preferences and policies are constructed and filtered over time'. To begin at the beginning, therefore, if we want to understand patterns of crime and punishment—both cross-sectionally and over time—then studying the formation of individual-level preferences over these outcomes is a natural starting point. The argument developed above, with its emphasis on inequality and social identity as central determinants of demand for punishment and law enforcement, takes one step in this direction, but much

work admittedly remains to be done. The same holds true on the supply-side, where—as with demand for redistribution—more attention needs to be paid to how criminal justice preferences are aggregated and translate into specific policy outcomes. Given the aforementioned salience of crime as a contentious political issue, further work in this area is a compelling next step.

# Appendices

# Appendix A

# Appendix to Chapter 3

**Full regression results** As mentioned above, for reasons of clarity I did not include the full set of regression estimates in the main text. Instead, they are included below: Table A.1 presents the main results for redistribution preferences (included in abbreviated form as Table 3.4 in the main text), while Table A.1 does the same for the analysis of punishment preferences (included as Table 3.5). As discussed in Chapter 3, both tables reports similar sets of four models. Model 1 and Model 3 are baseline specifications that include only the three substantive variables of interest, their interactions and the macro-level control (social expenditure or the incarceration rate). Model 2 and Model 3 in turn add the full battery of individual-level control variables. In both cases, Model 1 and Model 2 are standard logit models with cluster robust standard errors, while Model 3 and Model 4 are mixed-effects logit models with country random-effects. Because of their number, I do not report full regression results for the series of robustness tests discussed in Section 3.5, in addition to the average marginal effects included in the main text. If required, these are available on request.

**Table A.1:** Determinants of redistribution preferences

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| Relative income | -0.103*** | -0.082** | -0.106*** | -0.088*** |
| | (0.037) | (0.032) | (0.007) | (0.008) |
| Fear of crime | 0.169 | 0.066 | 0.252*** | 0.152*** |
| | (0.128) | (0.135) | (0.048) | (0.050) |
| Heterogeneity | 0.004 | 0.006 | 0.005 | 0.011*** |
| | (0.021) | (0.021) | (0.004) | (0.004) |
| Fear of crime × Relative income | 0.093*** | 0.097*** | 0.092*** | 0.097*** |
| | (0.032) | (0.032) | (0.019) | (0.020) |
| Relative income × Heterogeneity | -0.002 | -0.002 | -0.002*** | -0.002*** |
| | (0.003) | (0.003) | (0.001) | (0.001) |
| Fear of crime × Heterogeneity | 0.001 | 0.001 | -0.004 | -0.004 |
| | (0.009) | (0.010) | (0.004) | (0.004) |
| Fear of crime × Relative income × Heterogeneity | -0.006** | -0.007** | -0.007*** | -0.008*** |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| Social expenditure | -0.147*** | -0.142*** | -0.102*** | -0.102*** |
| | (0.049) | (0.051) | (0.008) | (0.008) |
| Age | – | 0.035*** | – | 0.039*** |
| | | (0.007) | | (0.002) |
| $Age^2$ | – | -0.000*** | – | -0.000*** |
| | | (0.000) | | (0.000) |
| Female | – | 0.252*** | – | 0.260*** |
| | | (0.043) | | (0.012) |
| Religiosity | – | 0.004 | – | -0.068*** |
| | | (0.054) | | (0.021) |
| Education | – | -0.365*** | – | -0.326*** |
| | | (0.044) | | (0.013) |
| Married | – | -0.056* | – | -0.049*** |
| | | (0.034) | | (0.014) |
| Constant | 1.915*** | 0.979*** | 1.671*** | 0.610*** |
| | (0.338) | (0.375) | (0.126) | (0.135) |
| Year fixed-effects | Yes | Yes | Yes | Yes |
| Country random-effects | No | No | Yes | Yes |
| BIC | 178,313 | 168,193 | 173,640 | 163,809 |
| Log-likelihood | -89,067 | -84,007 | -86,725 | -81,774 |
| Observations | 152,440 | 145,196 | 152,440 | 145,196 |
| Countries | 16 | 16 | 16 | 16 |

Numbers are coefficients. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Included as Table 3.4 in Chapter 3.

**Table A.2:** Determinants of punishment preferences

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| Relative income | -0.080*** | -0.060* | -0.082*** | -0.065** |
| | (0.028) | (0.035) | (0.024) | (0.028) |
| Fear of crime | 0.441 | 0.441 | 0.583*** | 0.523*** |
| | (0.311) | (0.376) | (0.134) | (0.147) |
| Heterogeneity | -0.058** | -0.037 | -0.000 | -0.026** |
| | (0.026) | (0.024) | (0.013) | (0.012) |
| Fear of crime × Relative income | -0.171*** | -0.146** | -0.181** | -0.158* |
| | (0.046) | (0.061) | (0.075) | (0.082) |
| Relative income × Heterogeneity | -0.001 | 0.001 | -0.001 | 0.001 |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| Fear of crime × Heterogeneity | 0.012 | 0.005 | 0.001 | -0.000 |
| | (0.027) | (0.032) | (0.010) | (0.011) |
| Fear of crime × Relative income × Heterogeneity | 0.015*** | 0.013*** | 0.016*** | 0.014** |
| | (0.004) | (0.005) | (0.006) | (0.006) |
| Incarceration rate | 0.012*** | 0.013*** | -0.009 | 0.008*** |
| | (0.002) | (0.001) | (0.006) | (0.003) |
| Age | – | -0.011** | – | -0.011** |
| | | (0.004) | | (0.004) |
| $Age^2$ | – | 0.000** | – | 0.000*** |
| | | (0.000) | | (0.000) |
| Female | – | 0.135*** | – | 0.133*** |
| | | (0.044) | | (0.025) |
| Religiosity | – | 0.019 | – | -0.098** |
| | | (0.142) | | (0.044) |
| Education | – | -0.765*** | – | -0.761*** |
| | | (0.063) | | (0.026) |
| Married | – | 0.192*** | – | 0.208*** |
| | | (0.052) | | (0.027) |
| Constant | 0.113 | 0.169 | 1.576*** | 0.559* |
| | (0.393) | (0.398) | (0.561) | (0.321) |
| Year fixed-effects | Yes | Yes | Yes | Yes |
| Country random-effects | No | No | Yes | Yes |
| BIC | 43,984 | 39,904 | 43,598 | 39,715 |
| Log-likelihood | -21,939 | -19,890 | -21,742 | -19,769 |
| Observations | 35,654 | 33,169 | 35,654 | 33,169 |
| Countries | 13 | 13 | 13 | 13 |

Numbers are coefficients. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Included as Table 3.5 in Chapter 3.

**Details of country-years included in the ESS** Table A.3 below gives details of the country-years included in the main analysis. As mentioned above, the analysis of redistribution preferences employs data from all rounds of the ESS, whereas the analysis of support for harsher punishment relies on the surveys from 2008 and 2010 only. Again, the reason for this is that only the 2008 and 2010 surveys include the dependent variable measuring punitive attitudes.

**Table A.3:** Summary of country-years included in the ESS

| Country | Year |
|---|---|
| Austria | 2002, 2004, 2006, 2014 |
| Belgium | 2002, 2004, 2006, 2008, 2010, 2012, 2014 |
| Denmark | 2002, 2004, 2006, 2008, 2010, 2012, 2014 |
| Finland | 2002, 2004, 2006, 2008, 2010, 2012, 2014 |
| France | 2004, 2006, 2008, 2010, 2012, 2014 |
| Germany | 2002, 2004, 2006, 2008, 2010, 2012, 2014 |
| Greece | 2002, 2004, 2008, 2010 |
| Iceland | 2004, 2012 |
| Ireland | 2004, 2006, 2010, 2012, 2014 |
| Italy | 2002, 2004, 2012 |
| Netherlands | 2002, 2004, 2006, 2008, 2010, 2012, 2014 |
| Norway | 2002, 2004, 2006, 2008, 2010, 2012, 2014 |
| Portugal | 2002, 2004, 2006, 2008, 2012, 2014 |
| Spain | 2002, 2004, 2006, 2008, 2010, 2012, 2014 |
| Sweden | 2002, 2004, 2006, 2008, 2010, 2012, 2014 |
| United Kingdom | 2002, 2004, 2006, 2008, 2010, 2012, 2014 |

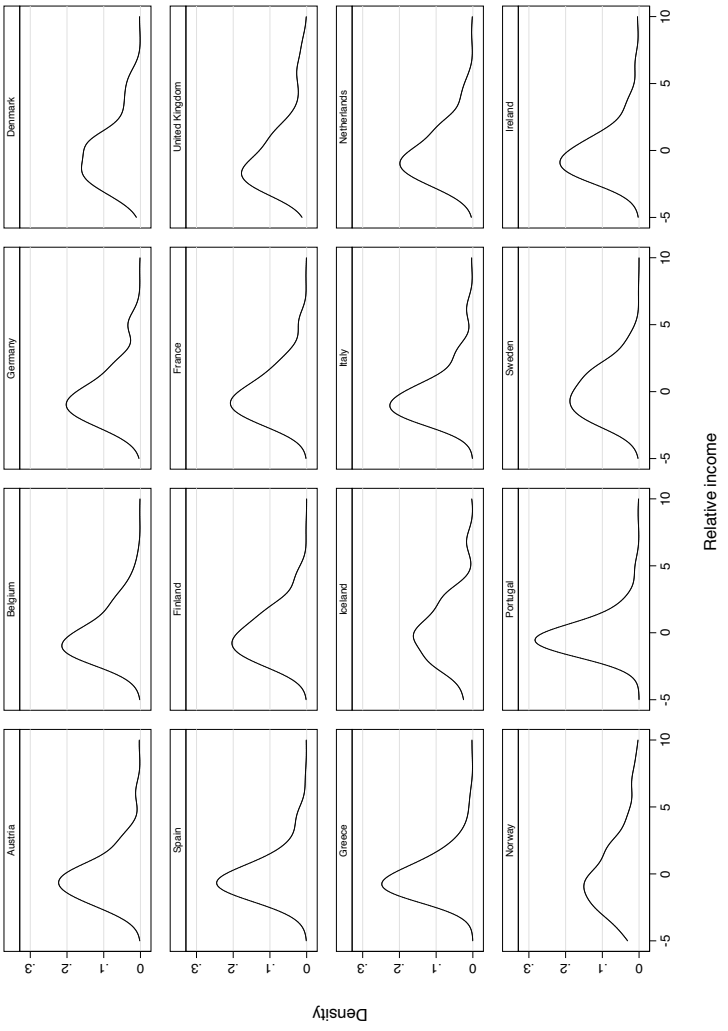
**Descriptive statistics** Table A.4 below presents simple descriptive statistics for the variables included in the analysis. For detailed variable descriptions, see Section 3.1 for the variables included in the main analysis, and Section 3.5 for those included as additional robustness tests.

**Table A.4:** Descriptive statistics

| Substantive variables | Mean | % | Median | Min | Max | SD |
|---|---|---|---|---|---|---|
| Fear of crime | – | 20.9 | – | – | – | – |
| Relative income | 0.00 | – | -0.47 | -6.69 | 26.97 | 2.36 |
| Heterogeneity | 11.6 | – | 11.9 | 1.23 | 23.1 | 4.59 |
| Redistribution preferences | – | 70.3 | – | – | – | – |
| Punishment preferences | – | 68.0 | – | – | – | – |
| **Control variables** | **Mean** | **%** | **Median** | **Min** | **Max** | **SD** |
| Age | 49.4 | – | 49.0 | 18.0 | 123 | 18.0 |
| Female | – | 52.5 | – | – | – | – |
| Higher education | – | 28.0 | – | – | – | – |
| Religiosity | – | 14.3 | – | – | – | – |
| Married | – | 51.9 | – | – | – | – |
| Social expenditure | 8.99 | – | 8.86 | 3.08 | 19.4 | 3.17 |
| Incarceration rate | 96.1 | – | 92.0 | 39.0 | 165 | 27.8 |
| Police rate | 291 | – | 302 | 140 | 558 | 95.3 |
| Heterogeneity (ESS) | 8.90 | – | 9.02 | 1.93 | 16.2 | 2.99 |
| Heterogeneity (OECD) | 11.2 | – | 11.7 | 2.9 | 17.0 | 3.34 |
| Altruism | 2.16 | – | 2.00 | 1.00 | 6.00 | 0.97 |
| Skill-specificity | 4.29 | – | 3.38 | 0.79 | 25.1 | 3.87 |
| Ideology | 5.04 | – | 5.00 | 0.00 | 10.0 | 2.09 |
| Trust | 5.33 | – | 5.00 | 0.00 | 10.0 | 2.36 |
| Economic satisfaction | 4.71 | – | 5.00 | 0.00 | 10.0 | 2.51 |
| Unemployed | – | 4.40 | – | – | – | – |

**Relative income** For reference, Figure A.1 plots Gaussian kernel density estimates for relative income in each country included in the analysis above. As described in Section 3.1.2 above, the relative income variable measures the difference between respondents' income and the mean income in each country-year. As can be seen in the figure, the values chosen to represent the rich ($45,600 above the mean) and the poor ($28,000 below the mean) are not in the extreme tails of these distributions.

**Figure A.1:** The distribution of relative income



# Appendix B

# Appendix to Chapter 4

**Full regression results** The pages below present the full set of regression results from the analysis developed in Chapter 4 of the main text. Table B.1 and Table B.2 report the results for the redistribution side of the analysis, with and without the South respectively. Table B.3 in turn reports the results of the models estimating the determinants of law enforcement preferences. As discussed in the main text, all models are mixed-effects logistic regression with random-effects on the MSA-level. In all three tables, Model 1 is a baseline model that includes relative income, fear of crime and the percentage of black residents in the MSA, while Model 2 adds interactions between these variables. Model 3, in turn, also adds the full set of individual- and macro-level control variables. Once again, for reasons of space I do not include full regression estimates for the robustness tests.

**Table B.1:** Determinants of redistribution preferences (including the South)

| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| Relative income | -0.074*** | -0.088*** | -0.102*** |
| | (0.011) | (0.023) | (0.025) |
| Fear of crime | 0.381*** | 0.552*** | 0.478*** |
| | (0.096) | (0.156) | (0.165) |
| % Black | -0.013** | -0.009 | -0.012* |
| | (0.006) | (0.006) | (0.007) |
| Fear of crime × Relative income | – | 0.032 | 0.046 |
| | | (0.036) | (0.037) |
| Relative income × % Black | – | 0.001 | 0.002 |
| | | (0.002) | (0.002) |
| Fear of crime × % Black | – | -0.013 | -0.011 |
| | | (0.012) | (0.012) |
| Fear of crime × Relative income × % Black | – | -0.003 | -0.004* |
| | | (0.002) | (0.002) |
| Age | – | – | 0.024 |
| | | | (0.021) |
| $Age^2$ | – | – | -0.000 |
| | | | (0.000) |
| Female | – | – | 0.192** |
| | | | (0.097) |
| Married | – | – | -0.068 |
| | | | (0.117) |
| Education | – | – | 0.011 |
| | | | (0.021) |
| Religiosity | – | – | -0.251*** |
| | | | (0.077) |
| Segregation | – | – | 0.008* |
| | | | (0.005) |
| Federal transfers | – | – | -0.000 |
| | | | (0.000) |
| Constant | -0.303** | -0.361*** | -0.571 |
| | (0.127) | (0.124) | (0.706) |
| Observations | 1,702 | 1,702 | 1,628 |
| MSAs | 111 | 111 | 111 |

Numbers are coefficients. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Included as Table 4.2 in Chapter 4.

**Table B.2:** Determinants of redistribution preferences (excluding the South)

| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| Relative income | -0.065*** | -0.131*** | -0.154*** |
| | (0.011) | (0.028) | (0.030) |
| Fear of crime | 0.512*** | 0.629*** | 0.600*** |
| | (0.113) | (0.187) | (0.191) |
| % Black | -0.001 | -0.003 | -0.013 |
| | (0.011) | (0.014) | (0.023) |
| Fear of crime × Relative income | – | 0.097** | 0.137*** |
| | | (0.044) | (0.043) |
| Relative income × % Black | – | 0.006*** | 0.008*** |
| | | (0.002) | (0.002) |
| Fear of crime × % Black | – | -0.007 | -0.006 |
| | | (0.018) | (0.017) |
| Fear of crime × Relative income × % Black | – | -0.009*** | -0.013*** |
| | | (0.003) | (0.003) |
| Age | – | – | 0.037 |
| | | | (0.026) |
| $Age^2$ | – | – | -0.000* |
| | | | (0.000) |
| Female | – | – | 0.198* |
| | | | (0.113) |
| Married | – | – | -0.066 |
| | | | (0.153) |
| Education | – | – | 0.010 |
| | | | (0.024) |
| Religiosity | – | – | -0.323*** |
| | | | (0.095) |
| Segregation | – | – | 0.007 |
| | | | (0.009) |
| Federal transfers | – | – | -0.000 |
| | | | (0.000) |
| Constant | -0.404*** | -0.421*** | -0.435 |
| | (0.138) | (0.152) | (1.059) |
| Observations | 1,171 | 1,171 | 1,113 |
| MSAs | 71 | 71 | 71 |

Numbers are coefficients. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Included as Table 4.3 in Chapter 4.

**Table B.3:** Determinants of law enforcement preferences

| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| Relative income | -0.012 | 0.011 | 0.011 |
| | (0.008) | (0.019) | (0.019) |
| Fear of crime | 0.219** | 0.205 | 0.247 |
| | (0.108) | (0.169) | (0.184) |
| % Black | 0.016*** | 0.018*** | 0.023*** |
| | (0.005) | (0.006) | (0.007) |
| Fear of crime × Relative income | – | 0.009 | 0.003 |
| | | (0.033) | (0.035) |
| Relative income × % Black | – | -0.003*** | -0.003*** |
| | | (0.001) | (0.001) |
| Fear of crime × % Black | – | -0.001 | -0.003 |
| | | (0.010) | (0.010) |
| Fear of crime × Relative income × % Black | – | 0.003 | 0.003 |
| | | (0.002) | (0.002) |
| Age | – | – | 0.059*** |
| | | | (0.017) |
| $Age^2$ | – | – | -0.001*** |
| | | | (0.000) |
| Female | – | – | 0.176 |
| | | | (0.123) |
| Married | – | – | 0.191 |
| | | | (0.130) |
| Education | – | – | -0.057*** |
| | | | (0.020) |
| Religiosity | – | – | 0.165** |
| | | | (0.071) |
| Segregation | – | – | -0.007 |
| | | | (0.005) |
| Law enforcement employment | – | – | -0.083 |
| | | | (0.096) |
| Constant | -0.300** | -0.292* | -0.689 |
| | (0.149) | (0.159) | (0.684) |
| Observations | 1,621 | 1,621 | 1,552 |
| MSAs | 110 | 110 | 110 |

Numbers are coefficients. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Included as Table 4.4 in Chapter 4.

**Descriptive statistics** Table B.4 below provide descriptive statistics for the variables used in the main text. For variable definitions, see Section 4.2 for those used in the main analysis, and Section 4.5 for those added as robustness tests.

**Table B.4:** Descriptive statistics

| Substantive variables | Mean | % | Median | Min | Max | SD |
|---|---|---|---|---|---|---|
| Redistribution preferences | – | 44.3 | – | – | – | – |
| Law enforcement preferences | – | 55.4 | – | – | – | – |
| Relative income | 0.00 | – | -1.47 | -7.06 | 15.9 | 5.23 |
| Fear of crime | – | 41.2 | – | – | – | – |
| % Black | 13.6 | – | 11.2 | 0.30 | 46.4 | 10.1 |
| **Control variables** | **Mean** | **%** | **Median** | **Min** | **Max** | **SD** |
| Age | 45.7 | – | 43.0 | 18.0 | 89.0 | 16.9 |
| Female | – | 55.9 | – | – | – | – |
| Married | – | 47.1 | – | – | – | – |
| Education | 13.6 | – | 13.0 | 0.00 | 20.0 | 3.01 |
| Religiosity: | | | | | | |
| Liberal | – | 30.9 | – | – | – | – |
| Moderate | – | 40.6 | – | – | – | – |
| Fundamentalist | – 28.6 | – | – | – | – | – |
| Residential segregation | 59.9 | – | 61.4 | 20.1 | 86.8 | 13.9 |
| Federal transfers | 2,799 | – | 2,702 | 1,861 | 4,647 | 552 |
| Law enforcement employment | 3.45 | – | 3.36 | 1.76 | 10.1 | 0.83 |
| Ideology | 3.91 | – | 4.00 | 1.00 | 7.00 | 1.42 |
| Denomination: | | | | | | |
| Protestant | – | 50.9 | – | – | – | – |
| Catholic | – | 26.1 | – | – | – | – |
| Jewish | – | 2.38 | – | – | – | – |
| None | – | 14.5 | – | – | – | – |
| Other | – | 5.12 | – | – | – | – |

*Continued on next page...*

*Continued from previous page...*

| Control variables | Mean | % | Median | Min | Max | SD |
|---|---|---|---|---|---|---|
| Social class: | | | | | | |
| Lower class | – | 5.76 | – | – | – | – |
| Working class | – | 43.1 | – | – | – | – |
| Middle class | – | 47.2 | – | – | – | – |
| Upper class | – | 3.93 | – | – | – | – |
| Occupational unemployment | 0.05 | – | 0.04 | 0.00 | 0.50 | 0.04 |
| Skill specificity | 3.97 | – | 3.02 | 0.79 | 25.1 | 3.14 |
| Work status: | | | | | | |
| Full time | – | 63.9 | – | – | – | – |
| Part time | – | 13.9 | – | – | – | – |
| Unemployed | – | 7.04 | – | – | – | – |
| Other | – | 15.2 | – | – | – | – |
| Central county | – | 92.4 | – | – | – | – |
| State income | 25,857 | – | 25,720 | 16,266 | 35,315 | 3,080 |
| % Black (GSS) | 15.1 | – | 10.5 | 0.00 | 89.5 | 14.4 |
| Large city | – | 20.3 | – | – | – | – |
| Social trust | 0.75 | – | 0.00 | 0.00 | 2.00 | 0.94 |
| Financial satisfaction | 1.99 | – | 2.00 | 1.00 | 3.00 | 0.75 |
| Unemployed | – | 3.49 | – | – | – | – |

# Appendix C

# Appendix to Chapter 5

**Rational criminals?** Moving on from the actions taken by potential victims of crime, the experiment also allows me to examine the determinants of criminal behaviour. This is not of direct relevance to my theoretical concerns, but nevertheless remains a substantively interesting topic. Hence, I develop such an analysis below.

From Becker (1968) and onwards, the economic analysis of crime has rested on the straightforward theoretical argument that criminal activities respond to economic incentives: similarly to what is the case for any other form of economic behaviour, crime is the result of an instrumentally rational individual's response to the potential benefit from crime relative to its cost. Within this framework, deterrence refers to any factor that serves to increase the relative cost of crime, most notably the probability and cost of punishment, on the one hand, and the opportunity cost of engaging in criminal activities, on the other. If the economic analysis of crime is correct, any increase in these variables should reduce the propensity of individuals to engage in crime, on the micro-level, as well as lead to a decrease in the crime rate, when individual behaviour is aggregated. Yet, and as I have discussed at some length above, whereas a reasonably large empirical literature has examined the second part of this proposition—linking aggregate level deterrence variables, generally the size of the police force, to the crime rate—very little evidence exist regarding the micro-level mechanism underlying this relationship. Moreover, causal identification in this macro-level literature has

also proved difficult because of the simultaneity between the two: that is, because of the fact that while crime responds to deterrence, efforts to deter crime often also respond to changes in the crime rate. At the bottom line, therefore, less is known than should be about the causal effect of deterrence on crime.

Given this shortfall, the objective of this section is twofold. The first is to test whether the expectation that the likelihood to engage in crime responds to economic incentives holds up in a laboratory setting, where changes in deterrence are strictly exogenous and randomly assigned. The second is more novel, and involves investigating whether identity mattered for the choice to steal or not to steal. Since the results derived above suggest that identity did matter for the choices made by potential victims of crime, it is not an unreasonable possibility that similar concerns also mattered for the decisions taken by prospective criminals. Nevertheless, no study to date has investigated this issue.

To start, Table C.1 reports the results from a series of logistic regression models, explaining the decision to attempt to steal rather than not to do so. Model 1 is a random-intercept model, including the group status of the potential victim and the expected payoff associated with attempting as explanatory variables. The latter is calculated as the expected net gain from attempting to steal, and thus incorporates the effects of the probability of punishment and its severity, and the opportunity cost of attempting to steal. As such, it depends both on the stochastic parameters of the experiment, and negatively on any spending on either redistribution or punishment by Player 1. It thus provides a baseline test of the deterrence hypothesis: if criminal behaviour does respond to incentives, the likelihood that a subject attempted to steal should increase with the expected benefit of doing so. Model 2, moreover, includes the same covariates together with subject fixed effects. In contrast to Model 1, therefore, it relies on within-subject variation only, and the estimated effects should be interpreted as giving the effects of the independent variables controlling for all between-subject time-invariant heterogeneity. As before, I report odds ratios rather than linear coefficients.

The estimated results from Model 1 and 2 lend strong initial support for the predictions of the standard economic model. The effect of the expected payoff from stealing is positive and statistically significant at the 99% level in both models, meaning that subjects were more likely to attempt to steal when the expected relative gain from doing to increased. To provide a sense of the magnitude of this effect, moreover, Figure C.1 below plots the average predicted

**Table C.1:** Determinants of the propensity to steal

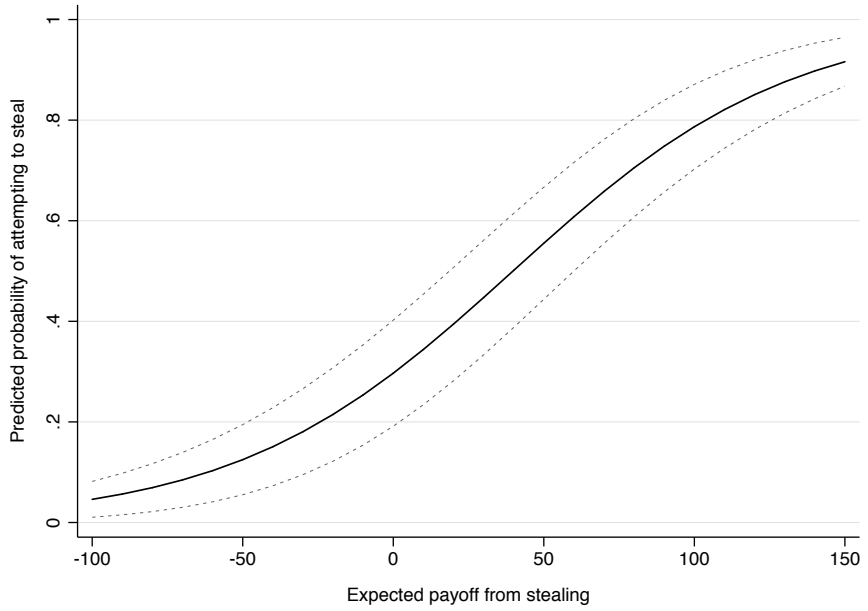
| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| In-group | 0.928 | 1.041 | 0.925 |
| | (0.259) | (0.299) | (0.260) |
| Expected payoff | 1.022*** | 1.022*** | 1.006 |
| | (0.00233) | (0.00240) | (0.00677) |
| Probability of success | – | – | 0.958** |
| | | | (0.0182) |
| Pr. of success ×Expected payoff | – | – | 1.00357** |
| | | | (0.000156) |
| Constant | 0.439*** | – | 3.128 |
| | (0.132) | | (2.827) |
| Subject (s.d.) | 1.204*** | – | 1.182*** |
| | (0.249) | | (0.250) |
| Observations | 440 | 410 | 440 |
| Number of subjects | 44 | 41 | 44 |
| AIC | 387.50 | 209.42 | 383.95 |
| Log likelihood | -189.8 | -102.7 | -186.0 |

DV: 1 = Attempted to steal, 0 = Did not attempt to steal
Model 1 and 3 are mixed-effects logit models with random subject-effects.
Model 2 is a mixed-effects logit model with fixed subject-effects.
Numbers are odds ratios. Standard errors in parentheses.
Two-tailed significance tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

probability of attempting to steal as a function of the expected payoffs, based on the estimates in Model 1. When the expected payoff was -50, implying that in expectation subjects would be made worse off by attempting to steal, the probability of engaging in crime was approximately 12.5%. As the expected payoff associated with crime increased, so did the probability that subjects chose to do so: to 29.7% at when the expected gain was 0, to 55.5% when it was 50, and 78.7% at 100. This is a highly unambiguous result: as crime became more profitable, subjects were more likely to engage in it. As such, the hypothesis that criminal behaviour responds to economic incentives is clearly supported.

Model 3 in Table C.1 has a similar random-intercept specification to Model 1, but includes an interaction term between the expected payoff from attempting to steal and the probability that an attempt to steal was successful. This is useful in order to examine subjects' attitude towards risk: that is, to check whether their behaviour show evidence of risk aversion, risk neutrality, or risk seeking (for a highly useful theoretical discussion of risk preferences in the context of crime, see Mungan and Klick 2015). Since the effect of the probability of success on

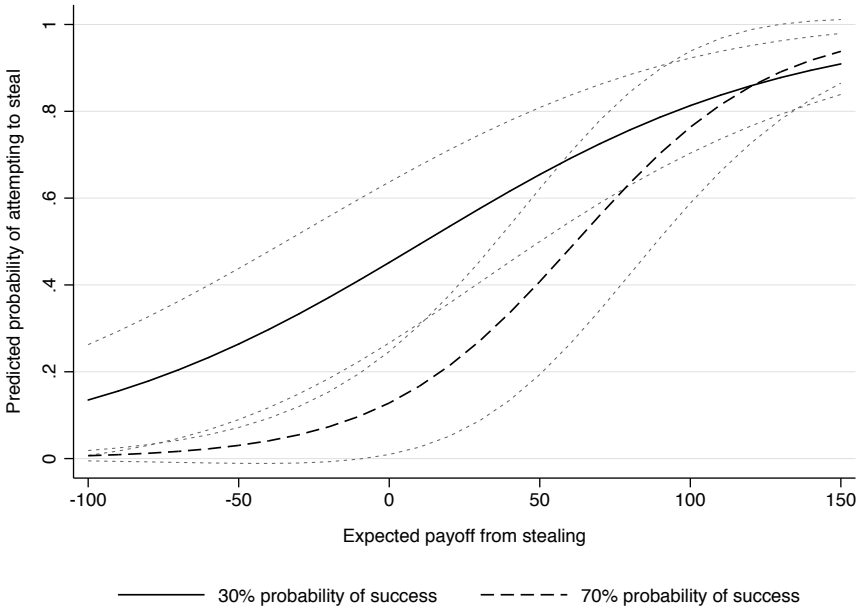
**Figure C.1:** Average predicted probability of attempting to steal



the potential benefit from attempting to steal is captured by the expected payoff variable, this model tests whether the success probability had an independent effect on the propensity to steal, over and above its effect on the expected payoff.

In line with previous observational studies showing that criminals are generally more sensitive to changes in the certainty than the severity of punishment (Grogger 1991; Block and Gerety 1995; for a theoretical discussion, see Becker 1968), the results from Model 3 suggest that subjects playing the game as potential criminals exhibited risk seeking behaviour. To simplify interpretation of this effect, Figure C.2 plots the average predicted probability (with 95% confidence intervals for the predictions) that a subject attempted to steal as a function of the expected payoff, when the probability of such an attempt being successful is set at 30% (solid line) and 70% (dashed line) respectively. As can be seen in the figure, given some expected payoff, subjects were more likely to attempt to steal when the probability of success was lower, and this difference is marginally statistically significant for a range of payoffs at and below zero. In other words, subjects were more prone to steal when the likelihood that an attempt to steal was low, but the

**Figure C.2:** Average predicted probability of attempting to steal



payoff from a successful crime was high, than they were when the probability of success was high but the payoff was lower. These differences are furthermore quite large. At an expected payoff of zero (that is, where attempting to steal and not attempting to steal had the same expected nominal value), the probability that a subject attempted to steal was 12% when faced with a probability of being successful of 70%, compared to 45% when the same probability was 30%.

Finally, in neither of these regressions is the identity of the potential victim estimated to affect the likelihood that a subject attempted to steal. The point estimates for the dummy representing the in-group treatment are negative in the random-effects specifications used in Model 1 and 3, implying that a shared group identity makes crime less likely, but positive in Model 2 when subject fixed-effects are included. Still, none of these estimates are significantly different from zero, or even remotely so. As such, and although identity does have important consequences for the behaviour of potential victims, as the previous analysis has shown, the same conclusion does not hold true for potential criminals.

Moving on, an important policy question concerns the relative deterrence effect of increasing the opportunity cost of crime compared to decreasing the expected payoff from criminal activities. This amounts to asking whether crime is more effectively deterred by increasing the payoff available to potential criminals if they choose not to engage in crime (through redistribution, for example), or by decreasing the reward from crime itself (by increasing the probability or severity of punishment, or a combination of the two). To address this issue, I estimate one last set of models, reported in Table C.2 on the next page. Following the same reasoning as that described above, Model 1 is a random-intercept logistic model, and Model 2 is a fixed-effects model, estimated by standard maximum likelihood. Substantively, the models are complementary and can be seen as a robustness test of each other. The dependent variable in both regressions is the same as for the models reported in Table C.1 (that is, coded as 1 if the subject attempted to steal, and as 0 if he or she did not), but now include both the payoff associated with not attempting to steal and the expected payoff of attempting to steal as separate variables on the right-hand side of the equation.

The estimates shown in Table C.2 again lend support to the standard expectation that potential criminals respond in predictable ways to changes in economic incentives. Subjects become less likely to engage in crime as the payoff associated with not stealing increased, controlling for the expected value of

**Table C.2:** Determinants of the propensity to steal

| | Model 1 | Model 2 |
|---|---|---|
| In-group | 0.935 | 1.048 |
| | (0.262) | (0.303) |
| Payoff (not stealing) | 0.977*** | 0.974*** |
| | (0.00255) | (0.00306) |
| Payoff (stealing) | 1.020*** | 1.021*** |
| | (0.00233) | (0.00252) |
| Constant | 2.557 | – |
| | (2.214) | |
| Subject (s.d.) | 1.186*** | – |
| | (0.252) | |
| Observations | 440 | 410 |
| Number of subjects | 44 | 41 |
| AIC | 384.39 | 207.08 |
| Log-likelihood | -187.4 | -100.5 |

DV: 1 = Attempted to steal, 0 = Did not attempt to steal
Model 1 is a mixed-effects logit model with random subject-effects.
Model 2 is a mixed-effects logit model with fixed subject-effects.
Numbers are odds ratios. Standard errors in parentheses.
Two-tailed significance tests:
$^{***}\, p < 0.01, ^{**}\, p < 0.05, ^{*}\, p < 0.1$

attempting to steal. The reverse relationship is also borne out: holding everything else constant, including the opportunity cost of crime, the propensity to engage in crime increased with its expected payoff. These effects are all highly statistically significant (at the 99% confidence-level) in both the random- and the fixed-effects models. Overall, therefore, the evidence provided here gives convincing support for the argument that criminal behaviour corresponds to the micro-level assumptions inherent to the view that crime is rational, at least in terms of the comparative statics. Paralleling the results from the previous regressions, however, the identity of the potential victim has no effect.

To better compare their relative magnitudes, I calculate the average marginal effects of the two sets of payoffs on the probability that a subject attempted to steal. These are reported in Table C.3 below, for Model 1 and Model 2 respectively. Since both independent variables are continuous, these figures can be interpreted as the instantaneous rate of change in the probability with respect to changes in either payoff. As these numbers show, increasing the opportunity cost of crime had a larger effect on the probability of that a subject attempted to steal than what was the case for a comparable decline in the expected payoff of engaging in crime. The average effect of a marginal increase in the expected value of stealing is approximately 0.24%, compared to between -0.28% and -0.3% for an equivalent change in the payoff available if subject chose not to steal. For both models, moreover, the difference between the two estimated average marginal affects is statistically significant just below the 95% level. As such, these results suggest that measures aimed at increasing the opportunities available to potential criminals of they choose not to engage in crime produce a greater deterrence effect than those directed at lowering the profitability of crime itself.

**Table C.3:** Average marginal effects of payoffs

| | Model 1 | Model 2 |
|---|---|---|
| Payoff (not stealing) | -0.00280*** | -0.00301*** |
| | (0.0000884) | (0.0000821) |
| Payoff (stealing) | 0.00243*** | 0.00236*** |
| | (0.0000977) | (0.0257) |

Estimates based on the results in Table C.2.
Numbers are average marginal effects. Standard errors in parentheses.
Two-tailed significance tests:
$^{***}\, p < 0.01, ^{**}\, p < 0.05, ^{*}\, p < 0.1$

**Information sheet** For reference, the information sheet distributed to all participants before the experiment is shown below. This was given to all participants in hard copy form, before they gave their consent to participate.

**Participant Information Sheet**

**Study title**
Crime and Equality, or Crime and Punishment?

**Background and aims of the study**
The aim of this study is to investigate the way in which people form political preferences in different types of group settings. The research is conducted by Karl Kahn (DPhil student), in collaboration with David Rueda (principal researcher).

**Why have I been invited to take part?**
You have been invited to take part in the study because you have signed up to the recruitment database of the Centre of Experimental Social Sciences at the University of Oxford. Other than that, there are no other specific reasons as to why you have been selected.

**Do I have to take part?**
You can decide for yourself whether or not you want to participate in the study. If you agree, you may withdraw yourself and your data from the study at any time by advising the researchers of your decision. This will not impose any adverse consequences and academic penalties.

**What will happen in the study?**
If you agree to take part in the study, you will be asked to sign a consent form before you can participate. In the experiment itself, you will have to make a series of decisions on a computer screen. The experiment may take up to 60 minutes to complete, and you will be compensated with a £4 show-up fee and extra monetary earnings, based on your performance during the game.

**Are there any potential risks in taking part?**
Taking part in the study will not impose any potential risks.

**What happens to the research data provided?**
The research data is confidentially and anonymously collected by the Centre for Experimental Social Sciences. We will collect data about your decisions and performance in the experimental game, as well as basic demographic information. However, at no point during or after the experiment will there be any link between your personal information and the experimental data.

**Will the research be published?**
The data generated in the experiment will be used as part of a DPhil thesis, and the resulting research may be published as an article in an academic journal. Like all student theses at the University of Oxford, this research will be deposited in print and electronic form to the University archives, where it will be made publically available.

**Who has reviewed this project?**
This project has been reviewed by and received clearance through the Social Science and Humanities Inter-Departmental Research Ethics Committee and the Centre for Experimental Social Sciences Ethics Review Committee.

**Who do I contact if I have a concern about the study or I wish to complain?**
If you have any concerns relating to this study, please contact Karl Kahn or David Rueda. If you wish to make a formal complaint, you should contact the Social Science and Humanities Inter-Departmental Research Ethics Committee directly, who will seek to resolve the problem in an as expeditious manner as possible.

**Contact details**
Karl Kahn: karl.ljungstromkahn@politics.ox.ac.uk
David Rueda: david.rueda@politics.ox.ac.uk
Social Science and IDREC: ethics@socsci.ox.ac.uk

**Paintings** Following Chen and Li (2009), the paintings used in the group inducement stage were: 1A *Gebirgsbildung*, 1924, by Klee; 1B *Subdued Glow*, 1928, by Kandinsky; 2A *Dreamy Improvisation*, 1913, by Kandinsky; 2B *Warning of the Ships*, 1917, by Klee; 3A *Dry-Cool Garden*, 1921, by Klee; 3B *Landscape with Red Spots No. I*, 1913, by Kandinsky; 4A*Gentle Ascent*, 1934, by Kandinsky; 4B *A Hoffmannesque Tale*, 1921, by Klee; 5A *Development in Brown*, 1933, by Kandinsky; and 5B *The Vase*, 1938, by Klee. The two additional paintings used in the collective quiz were: A *Monument in Fertile Country*, 1929, by Klee; and *Start*, 1928, by Kandinsky. Two examples are shown below.



**Figure C.3:** *Landscape with Red Spots No. I*, 1913, by Kandinsky



**Figure C.4:** *Dry-Cool Garden*, 1921, by Klee

**Instructions** The pages below present the full set of on-screen instructions used for the main treatment sessions of the experiment. The control group employed a very similar but marginally modified script, with all references to the identity groups removed. This version is available from the author on request.

**1. Introduction**

Welcome! Thank you for agreeing to participate in this experiment.

Throughout the procedures, you will be able to earn Experimental Currency Units (ECUs). At the end of the session, these are converted to cash at an exchange rate of 60 ECU=£1. The instructions are simple, and if you follow them carefully and make good decisions you will be able to earn a significant amount of money.

Please do not communicate with other participants. If you have a question, raise your hand and one of the experimenters will answer your question privately.

**2. Five pairs of paintings**

In the first part of the experiment, you will be shown five pairs of paintings by two artists, Paul Klee and Wassily Kandinsky. You will be asked to select the paining in each pair that you most prefer.

Based on your relative preference for the paintings by either artist, you will then be classified either as a member of the KLEEs (or as a Klee, for short), or as a member of the KANDINSKYs (or as a KANDINSKY, for short). More specifically, half of the participants in the session will be classified as KLEEs and half as KANDINSKYs, based on the similarity of their art preferences. You will be informed about whether you are a KLEE or a KANDINSKY privately.

Once assigned, your classification as a KLEE or a KANDINSKY remains fixed for the rest of the experiment.

After this classification, you will be asked to identify the artist of two additional paintings, also by Paul Klee and/or Wassily Kandinsky. If you answer

correctly, you can earn additional ECUs. More specifically, you will receive 100 ECUs if the majority of the members of your group, the KLEEs or the KANDINSKYs, identify the artists of both paintings correctly. Additionally, you will receive 100 ECUs if the members of your group collectively provide at least as many correct answers as the members of the other group. As will be explained later, you will be able to receive help from your fellow group members when completing this task.

If everything is clear, please click Begin below to proceed.

**3. Collective quiz**

Based on your choices, you have a relative preference for the paintings by [Wassily Kandinsky / Paul Klee]. You are therefore classified as a [KANDINSKY / KLEE]. In total, there are [12] [KANDINSKYs / KLEEs] in this session.

Your classification as a [KANDINSKY / KLEE] remains fixed for rest of the experiment.

You will now be shown two more paintings, also by Paul Klee and/or Wassily Kandinsky. You will be asked to first submit your initial guesses about the artist of each painting privately. After doing so, you will then be shown the the initial guesses of all of your fellow KLEEs if you are a KLEE, or your fellow KANDINSKYs if you are a KANDINSKY, and have an opportunity to revise your answers before submitting them permanently.

As mentioned previously, you will receive 100 ECUs if the majority of the members of your group, the [KLEEs / KANDINSKYs], identify the artists of both paintings correctly. Additionally, you will receive 100 ECUs if the members of your group collectively provide at least as many correct answers as the members of the other group, the [KANDINSKYs / KLEEs]. The results of this task, and your combined earnings from it, will be revealed to you at the end of the experimental session.

If everything is clear, please click Continue below to proceed.

**4. Dictator games**

In the next part of the experiment, you will be asked to divide 100 ECUs between two other anonymous participants, one KLEE and one KANDINSKY. In total, you will make five such decisions. For each, you will be shown two alternative ways of allocating this sum between the two other participants, and should select the one that you most prefer. Also, not that it is not possible for you to keep any tokens for yourself from this part of the experiment.

At the end of the experiment, one of your five choices will be randomly selected, and the two other participants will receive the ECUs that you have allocated to them in this round. You will also receive additional ECUs, depending on the choices made by two other participants in this task.

If everything is clear, please click Continue below to proceed.

**5. Real-effort task**

Next, you will be asked to do some additions. Once this task starts, two numbers will be displayed on your computer screen, and you should enter the sum of those two numbers into the box provided. You have 90 seconds to complete as many additions as you can. You will receive one point for each correct answer you give, but receive no points for incorrect answers.

Your relative performance in this task determines your endowment of ECUs for the remainder of the experiment. Within your group, the [KANDINSKYs / KLEEs], the participant with the highest number of correct answers will receive the largest endowment, the participant with the second highest number of correct answers will receive the second largest endowment, and so on. The relationship between your performance in this task and your subsequent endowment is displayed in the table below. In the table, rank 1 represents the [KLEE / KANDINSKY] member with the highest number correct answers, and rank [12] represents the member with the lowest number of correct answers.

If everything is clear, please click Continue below to proceed.

**6. Stealing game (all participants)**

Based on your performance, your endowment is [XX] ECUs.

In the next part of the experiment, you will play ten rounds of a game in which some of the participants in the experiment (those assigned the role of Player 2) will be able to attempt to steal [60 / 40 / 30]% of other participants' (those assigned the role of Player 1) endowments. Participants in the upper half of the endowment distribution will have the role of Player 1, and participants in the lower half of the endowment distribution will have the role of Player 2. In each round of the game, Player 1 is first given the option to make alternative investments intended to make it less profitable for Player 2 to attempt to steal, and Player 2 subsequently decides whether or not to attempt to steal from Player 1. If Player 2 attempts to steal from Player 1, the attempt is successful [one-third / half / two-thirds] of the time. If everything is clear, please click Continue below in order to receive further instructions.

**7A. Stealing game (Player 1 only)**

Given your endowment of [XX] ECUs, you will have the role of Player 1.

As mentioned previously, this means that you will play ten rounds of a game in which some of the other participants in the experiment can steal [60 / 40 / 30]% of your endowment, and you can make decisions about different ways of trying to prevent this from occurring. At the start of each round, the computer randomly pairs you with another participant. The person you are playing with knows whether you are a KLEE or a KANDINSKY, but has no additional information about your identity. The same holds true for the experimenters.

The game works as follows. The person you are playing with decides to attempt to steal from you, or not to attempt to steal from you. If the other player attempts to steal, the attempt is successful [one-third / half / two-thirds] of the time, and unsuccessful [two-thirds / half / one-third] of the time. If the attempt is successful, the other player takes [60 / 40 / 30]% of your income, leaving you with [40 / 60 / 70]% of what you currently have.

To make attempting to steal less profitable for the other player, you have two options:

1. You can give some of your income to the other player, conditional on them not attempting to steal. Each token you spend on this option increases the payoff of not attempting to steal for the other player by the same amount: for example, if you spend 50 tokens, the other player receives an additional 50 tokens if they decide not to steal, and if you spend 100 tokens, the other player receives an additional 100 tokens if they decide not to steal. In contrast, the other player does not receive any share of what you spend on this option if they attempt to steal, independently of whether the attempt is successful or unsuccessful. This means that the more you give, the less profitable attempting to steal becomes relative to not attempting to steal for the other player. In the text below, this is referred to as Option A.

2. You can invest some of your income to make an unsuccessful attempt to steal more costly for the other player. For each token you spend on this option, the other player has to pay an additional [1.5 / 2 / 3] tokens if they attempt to steal and the attempt is unsuccessful. In other words, if you invest 50 tokens, the other player has to pay [75 / 100 / 150] tokens if they attempt to steal but the attempt is unsuccessful, and if you invest 100 tokens, they have to pay [150 / 200 / 300] tokens. Again, if the other player attempts to steal, the attempt will be successful [one-third / half / two-thirds] of the time, and unsuccessful [two-thirds / half / one-third] of the time. Similarly to what is the case for Option A, this means that the more you invest, the less profitable attempting to steal becomes relative to not doing so for the other player. In the text below, this is Option B.

In each round of the game, you first decide how much of your endowment to allocate to Option A and Option B, before the other player decides whether or not to attempt to steal. There is a limit on the combined amount that you can spend on Options A and B, which is displayed on your computer screen in each round. As long as the total amount does not exceed this limit,

however, you can choose between the two options in any way that you prefer. For example, if the limit on how much you can spend is 100 tokens, you can spend 50 on Option A and 50 on Option B, 100 on Option A and 0 on Option B, 25 on Option A and 50 on Option B, 0 on Option A and 0 on Option B, and so on, as long as the total amount does not exceed the limit. To help you decide what you want to do, you can see the implications of your choice about Option A and Option B on the payoffs facing the other player when you make your decision. You are also told whether the other player is a KLEE or a KANDINSKY. Once you spend a certain amount on these options, moreover, you cannot get your money back, independently of what the other player does.

After you have made your choice about Option A and Option B, the other player either attempts to steal or does not attempt to steal. If the other player attempts to steal and the attempt is successful, your earnings are [40 / 60 / 70]% of your initial endowment (since the other player steals [60 / 40 / 30]%), minus the amount you spent on Option A and Option B. The other player receives their own endowment plus the amount stolen. If the other player attempts to steal but the attempt is unsuccessful, your payoff is your endowment minus the amount spent on Option A and Option B, and the payoff for the other player is their endowment minus the cost of an unsuccessful attempt to steal, determined by how much you spent on option Option B. Finally, if the other player does not attempt to steal, your earnings are again your endowment minus the amount you spent on Option A and Option B, and the payoff for the other player is their endowment plus the amount you spent on Option A.

Each round of the game is independent, and game resets after each round. You will never play against the same person more than once. At the end of the experiment, one of the rounds is randomly selected by the computer, and this round determines your final earnings. This means that any of the ten rounds could be the one that determines your take-home payment.

The table below summarises the payoff structure for each round of the game. Remember that you make your choice about how much to spend on Option A and Option B first, before the other player decides to attempt to steal

or not to attempt to steal. In the table, 'Endowment' represents your endowment, 'Other player's endowment' the endowment of the other player, 'A' is the number of tokens you spend on Option A (which increases what the other player gets if they decide not to attempt to steal), and 'B' is the number of tokens you spend on Option B (which increases the cost of an unsuccessful attempt to steal for the other player).

**7B. Stealing game (Player 2 only)**

Given your endowment of [XX] ECUs, you will have the role of Player 2.

As mentioned previously, this means that you will play ten rounds of a game in which you can attempt to steal from some of the other participants in the experiment. At the start of each round, the computer randomly pairs you with another participant. The person you are playing with knows whether you are a KLEE or a KANDINSKY, but has no additional information about your identity. The same holds true for the experimenters.

The game works as follows. In each round, you decide either to attempt to steal from the other player, or not to attempt to steal. If you attempt to steal, the attempt is successful [one-third / half / two-thirds] of the time, and unsuccessful [two-thirds / half / one-third] of the time. If you attempt to steal and the attempt is successful, your payoff is your endowment plus [60 / 40 / 30]% of the income of the other player. If you attempt to steal but the attempt is unsuccessful, you receive your endowment minus a fine. If you do not attempt to steal, on the other hand, your payoff is your endowment plus a bonus payment for not attempting to steal. The size of the fine you have to pay if you attempt to steal but the attempt is unsuccessful, and the size of the bonus payment you receive if you do not attempt to steal, both vary from round to round, depending on the choices made by the other player.

To help you decide, in each of the ten rounds you will be shown the amount that you can steal from the other player, the fine you will have to pay if you attempt to steal but is unsuccessful, and the bonus payment you receive if you decide not to attempt to steal. Again, if you decide to attempt to steal, the attempt will be successful [one-third / half / two-thirds] of the time. You will also be told whether the other player is a KLEE or a KANDINSKY.

Each round of the game is independent, and game resets after each round. You will never play against the same person more than once. At the end of the experiment, one of the rounds is randomly selected by the computer, and this round determines your final earnings. This means that any of the rounds you play could determine your take-home payment.

The table below summarises the payoff structure for each round of the game. In the table, 'Endowment' represents your endowment, and 'Other player's endowment' the endowment of the other player. 'Fine' is the fine you have to pay if you attempt to steal but the attempt is unsuccessful, and 'Extra payment' is the bonus payment you receive if you do not attempt to steal. Again, both the fine and the extra payment will vary between the rounds of the game, and will be displayed to you on your screen as you make your choice.

**Descriptive statistics** Table C.4 below provides descriptive statistics for the variables included in the main analysis. For a more detailed discussion of each variable (and their rationale), see Section 5.3 of the main text.

**Table C.4:** Descriptive statistics

| Dependent variables | Mean | % | Median | Min | Max | SD |
|---|---|---|---|---|---|---|
| Redistribution | 41.2 | – | 0.00 | 0.00 | 280 | 64.6 |
| Punishment | 59.8 | – | 30.0 | 0.00 | 310 | 73.6 |
| Difference | -18.6 | – | 0.00 | -310 | 277 | 104 |
| Redistribution share | 0.40 | – | 0.50 | 0.00 | 1.00 | 0.36 |
| Punishment share | 0.60 | – | 0.50 | 0.00 | 1.00 | 0.36 |
| All redistribution | – | 23.7 | – | – | – | – |
| Ordinal measure: | | | | | | |
| All redistribution | – | 16.9 | – | – | – | – |
| Equal division | – | 28.7 | – | – | – | – |
| All punishment | – | 54.4 | – | – | – | – |
| Theft attempt | – | 57.8 | – | – | – | – |
| **Independent variables** | **Mean** | **%** | **Median** | **Min** | **Max** | **SD** |
| In-group | – | 51.5 | – | – | – | – |
| Endowment | 701 | – | 787 | 300 | 1,000 | 226 |
| Probability of success: | | | | | | |
| 1/3 | – | 40.7 | – | – | – | – |
| 1/2 | – | 37.0 | – | – | – | – |
| 2/3 | – | 22.2 | – | – | – | – |
| Age | 29.5 | – | 24.5 | 17.0 | 70.0 | 12.2 |
| Female | – | 52.9 | – | – | – | – |
| Crime victim | – | 37.0 | – | – | – | – |
| Left-right self-placement | 1.75 | – | 2.00 | 1.00 | 3.00 | 0.70 |

**Payoff details** For reference, the pages below provide some additional details about the calculation of the payoffs (and expected payoffs) in the main stealing game, together with the associated constraints on the distribution of initial endowments. In the first step, P1 spends $R$ on the conditional transfer and $P$ on punishment. Once made, these are sunk costs. Endowments are $w_1$ for P1 and $w_2$ for P2 respectively. Since P2 cannot receive a negative payoff, there is a limit on the total amount that P1 can invest in $R$ and $P$ (see below), equal to:

$$R + P \leq (1 - \alpha)w_2 \tag{C.1}$$

In the second step, P2 decides whether or not to attempt to steal from P1. If P2 does not attempt to steal, the payoffs $\pi_1$ and $\pi_2$ are given by:

$$\pi_1 = w_1 - R - P \tag{C.2}$$

$$\pi_2 = w_2 + R \tag{C.3}$$

If P2 attempts to steal, on the other hand, the attempt is successful with probability $\alpha$: in one session $\alpha = \frac{1}{3}$, in two sessions and for the control group $\alpha = \frac{1}{2}$, and in one session $\alpha = \frac{2}{3}$. If the attempt is successful the payoffs are:

$$\pi_1 = w_1 - R - P - \frac{1}{5\alpha}w_1 \tag{C.4}$$

$$\pi_2 = w_2 + \frac{1}{5\alpha}w_1 \tag{C.5}$$

That is, P1 receives the endowment $w_1$ less the amount spent on $R$ and $P$ in the first step, and the share of $w_1$ stolen by P2. The proportion of $w_1$ that P2 can steal is adjusted by $\frac{1}{5\alpha}$ in order to keep the expected discounted benefit from crime constant at $\frac{1}{5}w_1$ independently of $\alpha$. P2 receives the endowment of $w_2$ and the amount stolen from P1. If P2 attempts to steal but the attempt is unsuccessful, the payoffs are:

$$\pi_1 = w_1 - R - P \tag{C.6}$$

$$\pi_2 = w_2 - \frac{1}{(1 - \alpha)}P \tag{C.7}$$

In other words, P1 receives the endowment $w_1$ minus the amount spent on $R$ and $P$. The payoff for P2 is $w_2$ less a fine determined by the choice by P1 of how much to spend on $P$ in the first step. The size of the fine is adjusted by $\frac{1}{(1-\alpha)}$: the lower the probability that P2 is caught, the larger the effect of $P$ on the cost of punishment for P2. This also means that $P$ and $R$ always have the same effect on the incentives for P2 to steal or not to steal, independently of $\alpha$. Taken together, the above implies that the expected payoffs if P2 attempts to steal are always:

$$E(\pi_1) = \frac{4}{5}w_1 - R - P \tag{C.8}$$

$$E(\pi_2) = w_2 + \frac{1}{5}w_1 - P \tag{C.9}$$

The payoffs is P2 decides not to steal are similarly always given by:

$$\pi_1 = w_1 - R - P \tag{C.10}$$

$$\pi_2 = w_2 + R \tag{C.11}$$

If P2 is risk neutral, this means that he or she should steal when:

$$\frac{1}{5}w_1 > R + P \tag{C.12}$$

The endowment distributions must be such that the cap $R + P \leq (1-\alpha)w_1$ on how much P1 can spend on $R$ and $P$ is greater than the amount needed to deter P2 from attempting to steal, which is $\frac{1}{5}w_1$. On the other hand, the limit must also be less than the difference between $w_1$ and $\frac{1}{5\alpha}w_1$, as the amount that P2 can steal, since P1 cannot have a negative payoff. In other words, for all possible matches between P1 and P2 it has to be the case that:

$$\frac{1}{5}w_1 \leq (1-\alpha)w_2 \leq w_1 - \frac{1}{5\alpha}w_1 \tag{C.13}$$

Table C.5 below gives three distributions that satisfy this. In all sessions, subjects 1 to 12 will be P2, and subjects 13 to 24 will be P1.

**Probability of success = 1/3**

Taking all of this together, it means that in the session where $\alpha = \frac{1}{3}$, the payoffs

if P2 attempts to steal and is successful are:

$$\pi_1 = w_1 - R - P - \frac{3}{5}w_1 \tag{C.14}$$

$$\pi_2 = w_2 + \frac{3}{5}w_1 \tag{C.15}$$

If P2 is unsuccessful, in contrast, they are:

$$\pi_1 = w_1 - R - P \tag{C.16}$$

$$\pi_2 = w_2 - \frac{3}{2}P \tag{C.17}$$

The maximum that P1 can spend in the first step is:

$$R + P \leq \frac{2}{3}w_2 \tag{C.18}$$

And for all subjects the income distribution must satisfy:

$$\frac{3}{10}w_1 \leq w_2 \leq \frac{3}{5}w_1 \tag{C.19}$$

**Probability of success = 1/2**

In the three sessions with $\alpha = \frac{1}{2}$, the payoffs if P2 successfully steals are:

$$\pi_1 = w_1 - R - P - \frac{2}{5}w_1 \tag{C.20}$$

$$\pi_2 = w_2 + \frac{2}{5}w_1 \tag{C.21}$$

And if the attempt is unsuccessful:

$$\pi_1 = w_1 - R - P \tag{C.22}$$

$$\pi_2 = w_2 - 2P \tag{C.23}$$

The maximum that P1 can spend in the first step is:

$$R + P \leq \frac{1}{2}w_2 \tag{C.24}$$

It must hold that:

$$\frac{2}{5}w_1 \leq w_2 \leq \frac{6}{5}w_1 \tag{C.25}$$

**Probability of success = 2/3**

In the one session with $\alpha = \frac{2}{3}$, finally, the payoffs if P2 steals are:

$$\pi_1 = w_1 - R - P - \frac{3}{10}w_1 \tag{C.26}$$

$$\pi_1 = w_2 + \frac{3}{10}w_1 \tag{C.27}$$

If P2 attempts to steal but is unsuccessful:

$$\pi_1 = w_1 - R - P \tag{C.28}$$

$$\pi_2 = w_2 - 3P \tag{C.29}$$

The limit on what P1 can spend in the first step is:

$$R + P \leq \frac{1}{3}w_2 \tag{C.30}$$

And it has to be the case that:

$$\frac{3}{5}w_1 \leq w_2 \leq \frac{21}{10}w_1 \tag{C.31}$$

**Endowment distributions** The table below presents details of the endowment distribution, based on the constraints derived above. It gives details for the sessions with 20 and those with 24 participants, respectively.

**Table C.5:** Endowment distributions (in ECU)

| Subject (20) | Subject (24) | Group | $\alpha = \frac{1}{3}$ | $\alpha = \frac{1}{2}$ | $\alpha = \frac{2}{3}$ |
|---|---|---|---|---|---|
| 1 | 1 | Klee | 300 | 400 | 600 |
| 2 | 2 | Kandinsky | 300 | 400 | 600 |
| 3 | 3 | Klee | 350 | 450 | 650 |
| 4 | 4 | Kandinsky | 350 | 450 | 650 |
| 5 | 5 | Klee | 400 | 500 | 700 |
| 6 | 6 | Kandinsky | 400 | 500 | 700 |
| 7 | 7 | Klee | 425 | 550 | 725 |
| 8 | 8 | Kandinsky | 425 | 550 | 725 |
| 9 | 9 | Klee | 450 | 600 | 750 |
| 10 | 10 | Kandinsky | 450 | 600 | 750 |
| – | 11 | Klee | 475 | 650 | 775 |
| – | 12 | Kandinsky | 475 | 650 | 775 |
| – | 13 | Klee | 825 | 750 | 825 |
| – | 14 | Kandinsky | 825 | 750 | 825 |
| 11 | 15 | Klee | 850 | 800 | 850 |
| 12 | 16 | Kandinsky | 850 | 800 | 850 |
| 13 | 17 | Klee | 875 | 850 | 875 |
| 14 | 18 | Kandinsky | 875 | 850 | 875 |
| 15 | 19 | Klee | 900 | 900 | 900 |
| 16 | 20 | Kandinsky | 900 | 900 | 900 |
| 17 | 21 | Klee | 950 | 950 | 950 |
| 18 | 22 | Kandinsky | 950 | 950 | 950 |
| 19 | 23 | Klee | 1000 | 1000 | 1000 |
| 20 | 24 | Kandinsky | 1000 | 1000 | 1000 |

# List of Tables


3.1 Support for redistribution in Western Europe . . . . . 76
3.2 Support for harsher punishment in Western Europe . . . . . 78
3.3 Determinants of fear of crime . . . . . 99
3.4 Determinants of redistribution preferences . . . . . 103
3.5 Determinants of punishment preferences . . . . . 115
3.6 Marginal effect of fear of crime on redistribution preferences . . . . . 124
3.7 Marginal effect of fear of crime on punishment preferences . . . . . 130

4.1 Determinants of fear of crime . . . . . 166
4.2 Determinants of redistribution preferences (including the South) . . . . 170
4.3 Determinants of redistribution preferences (excluding the South) . . . . 178
4.4 Determinants of law enforcement preferences . . . . . 184
4.5 Marginal effect of fear of crime on redistribution preferences . . . . . 190
4.6 Marginal effect of fear of crime on law enforcement preferences . . . . . 195

5.1 Summary of other-other dictator games . . . . . 207
5.2 Overview of experimental design . . . . . 215
5.3 Determinants of relative spending . . . . . 227
5.4 Determinants of relative spending . . . . . 230
5.5 Determinants of relative spending . . . . . 232
5.6 Average marginal effect of in-group status . . . . . 233
5.7 Determinants of demand for redistribution . . . . . 236
5.8 Determinants of demand for redistribution . . . . . 240
5.9 Determinants of demand for punishment . . . . . 242
5.10 Determinants of demand for punishment . . . . . 244

A.1 Determinants of redistribution preferences . . . . . 260
A.2 Determinants of punishment preferences . . . . . 261

A.3 Summary of country-years included in the ESS . . . . . 262
A.4 Descriptive statistics . . . . . 263

B.1 Determinants of redistribution preferences (including the South) . . . . 266
B.2 Determinants of redistribution preferences (excluding the South) . . . . 267
B.3 Determinants of law enforcement preferences . . . . . 268
B.4 Descriptive statistics . . . . . 269

C.1 Determinants of the propensity to steal . . . . . 273
C.2 Determinants of the propensity to steal . . . . . 277
C.3 Average marginal effects of payoffs . . . . . 279
C.4 Descriptive statistics . . . . . 290
C.5 Endowment distributions (in ECU) . . . . . 295

# List of Figures


1.1 Income inequality in the OECD (S80/S20 ratios) . . . . . 7
1.2 Income inequality in the OECD, 1985 and 2014 (Gini coefficients) . . . 8
1.3 Support for redistribution and income in Europe . . . . . 12
1.4 Support for redistribution and income in the US . . . . . 13
1.5 Cross-national differences in crime control preferences . . . . . 15

3.1 Marginal effect of fear of crime on the preferences of the affluent . . . . 71
3.2 Inequality and fear of crime by region . . . . . 91
3.3 Average support for redistribution by country . . . . . 91
3.4 Average support for redistribution by income . . . . . 92
3.5 Marginal effect of income on redistribution preferences . . . . . 94
3.6 Fear of crime and support for redistribution . . . . . 95
3.7 Average support for harsher punishment by country . . . . . 96
3.8 Fear of crime and support for harsher punishment . . . . . 96
3.9 Predicted probability of being afraid of crime by inequality . . . . . 100
3.10 Predicted probability of being afraid of crime by relative income . . . . 101
3.11 Pred. prob. of supporting redistribution . . . . . 105
3.12 Pred. prob. of supporting redistribution (low heterogeneity) . . . . . 108
3.13 Pred. prob. of supporting redistribution (high heterogeneity) . . . . . 108
3.14 Pred. prob. of supporting redistribution (high income) . . . . . 110
3.15 Pred. prob. of supporting redistribution (low income) . . . . . 110
3.16 Marg. eff. of fear of crime on redistribution preferences (by income) . . . 112
3.17 Marg. eff. of fear of crime on redistribution preferences (by heterogeneity) 112
3.18 Pred. prob. of supporting harsher punishment (low heterogeneity) . . . 117
3.19 Pred. prob. of supporting harsher punishment (high heterogeneity) . . . 117
3.20 Pred. prob. of supporting harsher punishment (high income) . . . . . 119
3.21 Pred. prob. of supporting harsher punishment (low income) . . . . . 119

3.22 Marg. eff. of fear of crime on punishment preferences (by income) . . . 121
3.23 Marg. eff. of fear of crime on punishment preferences (by heterogeneity) 121

4.1 Support for redistribution in the US, 1978-2010 . . . . . 147
4.2 Support for law enforcement spending in the US . . . . . 149
4.3 Distribution of relative income by decade . . . . . 152
4.4 Inequality and fear of crime . . . . . 157
4.5 Support for redistribution in the US by income . . . . . 158
4.6 Poor-rich difference in support for redistribution . . . . . 159
4.7 Fear of crime and support for redistribution . . . . . 160
4.8 Support for redistribution and black population . . . . . 160
4.9 Support for law enforcement spending in the US by income . . . . . 163
4.10 Support for law enforcement spending and fear of crime . . . . . 163
4.11 Support for law enforcement spending and black population . . . . . 164
4.12 Predicted probability of being afraid of crime . . . . . 167
4.13 Pred. prob. of supporting redistribution . . . . . 171
4.14 Pred. prob. of supporting redistribution, 1.6% black . . . . . 174
4.15 Pred. prob. of supporting redistribution, 27.4% black . . . . . 174
4.16 Marg. eff. of fear of crime on redistribution preferences, 1.6% black . . . 176
4.17 Marg. eff. of fear of crime on redistribution preferences, 27.4% black . . 176
4.18 Pred. prob. of supporting redistribution, 1.2% black . . . . . 180
4.19 Pred. prob. of supporting redistribution, 18.6% black . . . . . 180
4.20 Marg. eff. of fear of crime on redistribution preferences, 1.2% black . . . 182
4.21 Marg. eff. of fear of crime on redistribution preferences, 18.6% black . . 182
4.22 Pred. prob. of supporting more law enforcement spending, 1.6% black . . 186
4.23 Pred. prob. of supporting more law enforcement spending, 27.4% black . 186
4.24 Marg. eff. of fear of crime on law enforcement preferences, 1.6% black . 188
4.25 Marg. eff. of fear of crime on law enforcement preferences, 27.4% black . 188

5.1 Stealing game in extensive form . . . . . 213
5.2 Mean spending on redistribution and punishment . . . . . 216
5.3 Mean spending on redistribution by session . . . . . 217
5.4 Duration of the stealing game by session . . . . . 218
5.5 Mean spending on redistribution and punishment . . . . . 219
5.6 Relative spending on redistribution and punishment . . . . . 221

5.7 Mean total spending . . . . . 223

A.1 The distribution of relative income . . . . . 264

C.1 Average predicted probability of attempting to steal . . . . . 274
C.2 Average predicted probability of attempting to steal . . . . . 275
C.3 *Landscape with Red Spots No. I*, 1913, by Kandinsky . . . . . 281
C.4 *Dry-Cool Garden*, 1921, by Klee . . . . . 281

# Bibliography

Abbink, Klaus, Bernd Irlenbusch, and Elke Renner. 2002. "An Experimental Bribery Game." *Journal of Law, Economics, and Organization* 18 (2): 428–454.

Acemoglu, Daron, and James A. Robinson. 2005. *Economic Origins of Dictatorship and Democracy*. Cambridge: Cambridge University Press.

ACP. 2019. "American Communities Project: Diversity and Disparities Statistical Database." Brown University. https://s4.ad.brown.edu/projects/diversity/Data/data.htm.

ACS. 2018. "American Community Survey 2017 (1-Year Estimates)." US Census Bureau. https://www.census.gov/programs-surveys/acs/data.html.

Adriaenssen, An, and Ivo Aertsen. 2015. "Punitive Attitudes: Towards an Operationalization to Measure Individual Punitivity in a Multidimensional Way." *European Journal of Criminology* 12 (1): 92–112.

AfD. 2017. "Manifesto for Germany: The Political Programme of the Alternative for Germany." Alternative fur Deutschland/Alternative for Germany. https://www.afd.de/wp-content/uploads/sites/111/2017/04/2017-04-12_afd-grundsatzprogramm-englisch_web.pdf.

Agnew, Robert. 1992. "Foundation for a General Strain Theory of Crime and Delinquency." *Criminology* 30 (1): 47–88.

Åkerlund, David, Bart H. H. Golsteyn, Hans Grönqvist, and Lena Lindahl. 2016. "Time discounting and criminal behavior." *Proceedings of the National Academy of Sciences* 113 (may): 6160–6165.

Albanesi, Stefania. 2007. "Inflation and Inequality." *Journal of Monetary Economics* 54 (4): 1088–1114.

Alesina, Alberto, and Edward L. Glaeser. 2004. *Fighting Poverty in the US and Europe: A World of Difference*. Oxford: Oxford University Press.

Alesina, Alberto, and Eliana La Ferrara. 2005. "Preferences for Redistribution in the Land of Opportunities." *Journal of Public Economics* 89 (5-6): 897–931.

Alesina, Alberto, and George-Marios Angeletos. 2005. "Fairness and Redistribution." *American Economic Review* 95 (4): 960–980.

Alesina, Alberto, and Paola Giuliano. 2011. "Preferences for Redistribution." In *Handbook of Social Economics*, ed. Jess Benhabib, Matthew O. Jacksson, and Alberto Bisin. Vol. 1A. San Diego, CA: North-Holland.

Allen, Jonathan, Suzanne Edmonds, Allison Patterson, and Dominic Smith. 2006. "Policing and the Criminal Justice System - Public Confidence and Perceptions: Findings from the 2004/2005 British Crime Survey." Home Office Online Report. http://www.homeoffice.gov.uk/rds/pdfs06/rdsolr0706.pdf.

Allison, Paul D. 2009. *Fixed Effects Regression Models*. Thousand Oaks, CA: Sage Publications.

Allison, Paul D., and Richard P. Waterman. 2002. "Fixed-Effects Negative Binomial Regression Models." *Sociological Methodology* 32 (1): 247–266.

Alvaredo, Facundo, Anthony B Atkinson, Thomas Piketty, and Emmanuel Saez. 2013. "The Top 1 Percent in International and Historical Perspective." *Journal of Economic Perspectives* 27 (3): 3–20.

Anderson, Elizabeth S. 1999. "What Is the Point of Equality?" *Ethics* 109 (2): 287–337.

Anderson, Lisa R., and Sarah L. Stafford. 2003. "Punishment in a Regulatory Setting: Experimental Evidence from the VCM." *Journal of Regulatory Economics* 24 (1): 91–110.

Ansell, Ben. 2010. *From the Ballot to the Blackboard The Redistributive Political Economy of Education*. Cambridge, MA: Cambridge University Press.

Ansell, Ben, and David Samuels. 2010. "Inequality and Democratization: A Contractarian Approach." *Comparative Political Studies* 43 (12): 1543–1574.

Ansell, Ben, and David Samuels. 2014. *Inequality and Democratization: An Elite-Competition Approach*. New York, NY: Cambridge University Press.

Ansolabehere, Stephen, Jonathan Rodden, and James M. Snyder. 2006. "Purple America." *Journal of Economic Perspectives* 20 (2): 97–118.

Aos, Steve, Marna Miller, and Elizabeth Drake. 2006. "Evidence-Based Public Policy Options to Reduce Future Prison Construction, Criminal Justice Costs, and Crime Rates." *Federal Sentencing Reporter* 19: 275–290.

Applegate, Brandon K., Francis T. Cullen, and Bonnie S. Fisher. 2002. "Public Views Toward Crime and Correctional Policies: Is There a Gender Gap?" *Journal of Criminal Justice* 30 (2): 89–100.

Applegate, Brandon K., Francis T. Cullen, Bonnie S. Fisher, and Thomas Vander Ven. 2000. "Forgiveness and Fundamentalism: Reconsidering the Relationship between Correctional Attitudes and Religion." *Criminology* 38 (3): 719–754.

Applegate, Brandon K., and Joseph B. Sanborn. 2011. "Public Opinion on the Harshness of Local Courts: An Experimental Test of Question Wording Effects." *Criminal Justice Review* 36 (4): 487–497.

Aristotle. 2009. *The Politics*. Oxford: Oxford University Press.

Atkinson, Anthony B. 2015. *Inequality: What Can be Done?* Cambridge, MA: Harvard University Press.

Atkinson, Anthony B., Thomas Piketty, and Emmanuel Saez. 2011. "Top Incomes in the Long Run of History." *Journal of Economic Literature* 49 (1): 3–71.

Autor, David H., Lawrence F. Katz, and Melissa S. Kearney. 2006. "The Polarization of the U.S. Labor Market." *AEA Papers and Proceedings* 96 (2): 189–194.

Ayres, Ian, and John J. Donohue. 2003. "Shooting Down the "More Guns, Less Crime" Hypothesis." *Stanford Law Review* 55 (4): 1193–1312.

Ayres, Ian, and Steven D. Levitt. 1998. "Measuring Positive Externalities from Unobservable Victim Precaution: An Empirical Analysis of Lojack." *Quarterly Journal of Economics* 113 (1): 43–77.

Baldwin, Peter. 2009. *The Narcissism of Minor Differences: How America and Europe are Alike, an Essay in Numbers*. Oxford: Oxford University Press.

Barkan, Steven E., and Steven F. Cohn. 1994. "Racial Prejudice and Support for the Death Penalty by Whites." *Journal of Research in Crime and Delinquency* 31 (2): 202–210.

Barkan, Steven E., and Steven F. Cohn. 2005. "Why Whites Favor Spending More Money to Fight Crime: The Role of Racial Prejudice." *Social Problems* 52 (2): 300–314.

Barnes, Lucy. 2013. "Does Median Voter Income Matter? The Effects of Inequality and Turnout on Government Spending." *Political Studies* 61 (1): 82–100.

Barro, Robert. 2000. "Inequality and Growth in a Panel of Countries." *Journal of Economic Growth* 32 (March): 5–32.

Bartels, Larry M. 2006. "What's the Matter with What's the Matter with Kansas?" *Quarterly Journal of Political Science* 1 (2): 201–227.

Bartels, Larry M. 2008. *Unequal Democracy: The Political Economy of the New Guilded Age*. Princeton, NJ: Princeton University Press.

Bartels, Larry M. 2009. "Economic Inequality and Political Representation." In *The Unsustainable American State*, ed. Lawrence Jacobs and Desmond King. New York, NY: Oxford University Press.

Baum, Christopher F. 2008. "Modelling Proportions." *The Stata Journal* 8 (2): 299–303.

BEA. 2018. "Regional Economic Accounts: Annual Series of State Personal Income and Employment, Table SA35." Bureau of Economic Analysis.

Beccaria, Cesare. 1995. "On Crimes and Punishments." In *On Crimes and Punishments and Other Writings*. Cambridge: Cambridge University Press.

Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy* 76 (2): 169.

Beckett, Katherine, and Bruce Western. 2001. "Governing Social Marginality: Welfare, Incarceration, and the Transformation of State Policy." *Punishment & Society* 3 (1): 43–59.

Bell, Linda A., and Richard B. Freeman. 2001. "The incentive for working hard: Explaining hours worked differences in the US and Germany." *Labour Economics* 8 (2): 181–202.

Belot, Michele, Raymond Duch, and Luis Miller. 2015. "A Comprehensive Comparison of Students and Non-Students in Classic Experimental Games." *Journal of Economic Behavior and Organization* 113: 26–33.

Benabou, Roland. 1996. "Inequality and Growth." In *NBER Macroeconomics Annual*. Vol. 11. Cambridge, MA: MIT Press.

Benabou, Roland, and Efe A. Ok. 2001. "Social Mobility and the Demand for Redistribution : The POUM Hypothesis." *Quarterly Journal of Economics* 116 (2): 447–487.

Bennett, Richard R., and James P. Lynch. 1990. "Does a Difference Make a Difference? Comparing Cross-National Crime Indicators." *Criminology* 28 (1): 153–181.

Benoit, Jean-Pierre, and Martin J. Osborne. 1995. "Crime, Punishment, and Social Expenditure." *Journal of Institutional and Theoretical Economics* 151 (2): 326–347.

Benson, Bruce L., and Brian Meehan. 2017. "Does Private Security Affect the Level of Crime: A Test Using State Regulations as Instruments." *Applied Economics* 49 (48): 4911–4924.

Bentham, Jeremy. 1996. *An Introduction to the Principles of Morals and Legislation*. Oxford: Clarendon Press.

Beramendi, Pablo, and Christopher Anderson. 2008. "Income, Inequality, and Electoral Participation." In *Democracy, Inequality, and Representation: A Com-*

*parative Perspective*, ed. Pablo Beramendi and Christopher Anderson. New York, NY: Russell Sage Foundation.

Beramendi, Pablo, and Philipp Rehm. 2015. "Who Gives, Who Gains? Progressivity and Preferences." *Comparative Political Studies* 49 (4): 529–563.

Beramendi, Pablo, Silja Hausermann, Herbert Kitschelt, and Hanspeter Kriesi. 2015. *The Politics of Advanced Capitalism*. New York, NY: Cambridge University Press.

Bernhard, Helen, Ernst Fehr, and Urs Fischbacher. 2006. "Group Affiliation and Altruistic Norm Enforcement." *American Economic Review* 96 (2): 217–221.

Bertola, Giuseppe, and Andrea Ichino. 1995. "Wage Inequality and Unemployment: The United States versus Europe." In *NBER Macroeconomics Annual*. Vol. 10. Cambridge, MA: MIT Press.

Bickel, Warren K., Amy L. Odum, and Gregory J. Madden. 1999. "Impulsivity and Cigarette Smoking: Delay Discounting in Current, Never, and Ex-Smokers." *Psychopharmacology* 146 (4): 447–454.

BJS. 2019. "BJS Employment and Expenditure Database." Bureau of Justice Statistics. https://www.bjs.gov/index.cfm?ty=tp&tid=5.

Blackstone, Erwin, and Simon Hakim. 2010. "Private Policing: Experiences, Evaluation, and Future Directions." In *Handbook of Economics and Crime*, ed. Bruce L. Benson and Paul R. Zimmerman. Northampton, MA: Edward Elgar.

Blau, Judith R., and Peter M. Blau. 1982. "The Cost of Inequality: Metropolitan Structure and Violent Crime." *American Sociological Review* 47 (1): 114.

Block, Michael K., and John M. Heineke. 1975. "A Labour Theoretic Analysis of the Criminal Choice." *American Economic Review* 65 (3): 314–325.

Block, Michael K., and Vernon E. Gerety. 1995. "Some Experimental Evidence on Differences between Student and Prisoner Reactions to Monetary Penalties and Risk." *Journal of Legal Studies* 24 (1): 123–138.

BLS. 2019. "BLS CPI Research Series Using Current Methods (CPI-U-RS)." Bureau of Labour Statistics. https://www.bls.gov/cpi/research-series/home.htm.

Blumstein, Alfred, and Jacqueline Cohen. 1980. "Sentencing of Convicted Offenders: An Analysis of the Public's View." *Law & Society Review* 14 (2): 223.

Boix, Carles. 2003. *Democracy and Redistribution*. Cambridge: Cambridge University Press.

Borghans, Lex, and Bart H. H. Golsteyn. 2006. "Time Discounting and the Body Mass Index: Evidence from the Netherlands." *Economics and Human Biology* 4 (1): 39–61.

Bornhorst, Fabian, Andrea Ichino, Oliver Kirchkamp, Karl H. Schlag, and Eyal Winter. 2010. "Similarities and Differences when Building Trust: The Role of Cultures." *Experimental Economics* 13 (3): 260–283.

Bosch-Domènech, Antoni, José G. Montalvo, Rosemarie Nagel, and Albert Sartorra. 2002. "One, Two, (Three), Infinity, ... : Newspaper and Lab Beauty-Contest Experiments." *American Economic Review* 92 (5): 1687–1701.

Bourguignon, Francois. 2001. "Crime as a Social Cost of Poverty and Inequality: A Review Focusing on Developing Countries." In *Facets of Globalization: International and Local Dimensions of Development*, ed. Shadid Yusuf, Simon Evenett, and Weiping Wu. Washington, DC: World Bank.

Bourguignon, Francois, Jairo Nunez, and Fabio Sanchez. 2003. "A Structural Model of Crime and Inequality in Colombia." *Journal of the European Economic Association* 1 (2-3): 440–449.

Boustan, Leah. 2011. "Racial Residential Segregation in American Cities." In *The Oxford Handbook of Urban Economics and Planning*, ed. Nancy Brooks, Kieran Donaghy, and Gerrit-Jan Knaap. Oxford: Oxford University Press.

Bowers, David A. 1998. "Giving People What They Want: An Exploratory Analysis of Felony Sentencing in 49 States." *International Journal of Comparative and Applied Criminal Justice* 22 (1): 119–130.

Box, Steven, Chris Hale, and Glen Andrews. 1988. "Explaining Fear of Crime." *British Journal of Criminology* 28 (3): 340–356.

Brooks, Clem, and Jeff Manza. 2006. "Social Policy Responsiveness in Developed Democracies." *American Sociological Review* 71 (3): 474–494.

Brooks, Clem, and Jeff Manza. 2007. *Why Do Welfare States Persist: The Importance of Public Opinion in Democracies*. Chicago, IL: University of Chicago Press.

Brown, Elizabeth K. 2006. "The Dog that did not Bark: Punitive Social Views and the 'Professional Middle Classes'." *Punishment & Society* 8 (3): 287–312.

Brown, Rupert. 2000. "Social Identity Theory: Past Achievements, Current Problems and Future Challenges." *European Journal of Social Psychology* 30: 745–778.

Buchan, Nancy R., Eric J. Johnson, and Rachel T. A. Croson. 2006. "Let's Get Personal: An International Examination of the Influence of Communication, Culture and Social Distance on Other-Regarding Preferences." *Journal of Economic Behavior and Organization* 60 (3): 373–398.

Cameron, A. Colin, and Pravin K. Trivedi. 1998. *Regression Analysis of Count Data*. New York, NY: Cambridge University Press.

Carpenter, Jeffrey, Christina Connoly, and Caitilin Knowles Myers. 2008. "Altruistic Behavior in a Representative Dictator Experiment." *Experimental Economics* 11 (3): 282–298.

Carroll, Leo, and Pamele Irving Jackson. 1983. "Inequality, Opportunity, and Crime Rates in Central Cities." *Criminology* 21 (2): 178–194.

Carvalho, Leandro. 2010. "Poverty and Time Preference." RAND Labor and Population Working Paper, No. WR-759. Rand Corporation.

Cavaille, Charlotte. 2014. "Demand for Redistribution in the Age of Inequality." PhD dissertation. Department of Social Policy, Harvard University.

Charness, Gary, Luca Rigotti, and Aldo Rustichini. 2007. "Individual Behavior and Group Membership." *American Economic Review* 97 (4): 1340–1352.

Chen, Yan, and Sherry Xin Li. 2009. "Group Identity and Social Preferences." *American Economic Review* 99 (1): 431–457.

Cherry, Todd L. 2001. "Mental Accounting and Other-Regarding Behavior: Evidence from the Lab." *Journal of Economic Psychology* 22 (5): 605–615.

Cherry, Todd L., Peter Frykblom, and Jason F. Shogren. 2002. "Hardnose the Dictator." *American Economic Review* 92 (4): 1218–1221.

Chiu, W. Henry, and Paul Madden. 1998. "Burglary and Income Inequality." *Journal of Public Economics* 69 (1): 123–141.

Clotfelter, Charles T. 1977. "Public Services, Private Substitutes, and the Demand for Protection Against Crime." *American Economic Review* 67 (5): 867–877.

Clotfelter, Charles T. 1978. "Private Security and the Public Safety." *Journal of Urban Economics* 15 (3): 388–402.

Cohen, Gerald A. 1989. "On the Currency of Egalitarian Justice." *Ethics* 99 (4): 906–944.

Cohen, Mark A. 2005. *The Costs of Crime and Justice*. Abingdon: Routledge.

Cook, Philip J. 1986. "The Demand and Supply of Criminal Opportunities." *Crime and Justice* 7 (1): 1–27.

Cook, Philip J., and Nataliya Khmilevska. 2005. "Cross-National Patterns in Crime Rates." *Crime and Justice* 33: 331–345.

Corman, Hope, and Naci Mocan. 2000. "A Time-Series Analysis of Crime, Deterrence, and Drug Abuse in New York City." *American Economic Review* 90 (3): 584–604.

Corneo, Giacomo, and Hans P. Grüner. 2002. "Individual Preferences for Political Redistribution." *Journal of Public Economics* 83 (1): 83–107.

Costelloe, Michael T., Ted Chiricos, Jiří Buriánek, Marc Gertz, and Daniel Maier-Katkin. 2002. "The Social Correlates of Punitiveness Toward Criminals: A Comparison of the Czech Republic and Florida." *Justice System Journal* 23 (2): 191–220.

Costelloe, Michael T., Ted Chiricos, and Marc Gertz. 2009. "Punitive Attitudes Toward Criminals: Exploring the Relevance of Crime Salience and Economic Insecurity." *Punishment & Society* 11 (1): 25–49.

Cox, Cody B., and Margaret E. Beier. 2014. "Too Old to Train or Reprimand: The Role of Intergroup Attribution Bias in Evaluating Older Workers." *Journal of Business and Psychology* 29 (mar): 61–70.

CPS. 2018. "CPS Annual Social and Economic (ASEC) Supplement, Table HINC-05." US Census Bureau. https://www.census.gov/data/tables/time-series/demo/income-poverty/cps-hinc/hinc-05.html.

Cullen, Francis T., Gregory A. Clark, John B. Cullen, and Richard A. Mathers. 1985. "Attribution, Salience, and Attitudes toward Criminal Sanctioning." *Criminal Justice and Behavior* 12 (3): 305–331.

Cummings, Ronald G, and Laura O Taylor. 1999. "Unbiased Value Estimates for Environmental Goods: A Cheap Talk Design for the Contingent Valuation Method." *American Economic Review* 89 (jun): 649–665.

Cundiff, Patrick R., and Duane F. Alwin. 2015. "Changing Beliefs about Crime and Punishment in American Society." *Journal of Law and Criminal Justice* 3 (1): 2374–2682.

Cusack, Thomas, Torben Iversen, and Philipp Rehm. 2006. "Risks at Work: The Demand and Supply Sides of Government Redistribution." *Oxford Review of Economic Policy* 22 (3): 365–389.

Cutler, David M., Edward L. Glaeser, and Jacob L. Vigdor. 1999. "The Rise and Decline of the American Ghetto." *Journal of Political Economy* 107 (3): 455–506.

Dahlberg, Matz, Karin Edmark, and Helene Lundqvist. 2012. "Ethnic Diversity and Preferences for Redistribution." *Journal of Political Economy* 120 (1): 41–76.

Dahlberg, Matz, and Magnus Gustavsson. 2008. "Inequality and Crime: Separating the Effects of Permanent and Transitory Income." *Oxford Bulletin of Economics and Statistics* 70 (2): 129–153.

Danielson, Anders J., and Hakan J. Holm. 2007. "Do You Trust Your Brethren? Eliciting Trust Attitudes and Trust Behavior in a Tanzanian Congregation." *Journal of Economic Behavior and Organization* 62 (2): 255–271.

Davis, Michael L. 1988. "Time and Punishment: An Intertemporal Model of Crime." *Journal of Political Economy* 96 (2): 383.

de Wit, Harriet, Janine D. Flory, Ashley Acheson, Michael McCloskey, and Stephen B. Manuck. 2007. "IQ and Nonplanning Impulsivity are Independently Associated with Delay Discounting in Middle-Aged Adults." *Personality and Individual Differences* 42 (1): 111–121.

DeAngelo, Gregory, and Gary Charness. 2012. "Deterrence, Expected Cost, Uncertainty and Voting: Experimental Evidence." *Journal of Risk and Uncertainty* 44 (1): 73–100.

Deaton, Angus. 1989. "Saving in Developing Countries: Theory and Review." *The World Bank Economic Review* 3 (1): 61–96.

Department of Justice. 2018. "Uniform Crime Reporting Program Data: Police Employee (LEOKA) Data Series." Federal Bureau of Investigation, Department of Justice.

Depositario, Dinah Pura T., Rodolfo M. Nayga, Ximing Wu, and Tiffany P. Laude. 2009. "Should Students be Used as Subjects in Experimental Auctions?" *Economic Letters* 102 (2): 122–124.

Desai, Raj M., Anders Olofsgard, and Tarik M. Yousef. 2003. "Democracy, Inequality, and Inflation." *American Political Science Review* 97 (3): 391–406.

Dezhbakhsh, Hashem, Paul Rubin, and Joanna M. Shepherd. 2003. "Does Capital Punishment Have a Deterrent Effect? New Evidence from Postmoratorium Panel Data." *American Law and Economics Review* 5 (2): 344–376.

Di Tella, Rafael, and Ernesto Schargrodsky. 2004. "Do Police Reduce Crime? Estimates using the Allocation of Police Forces After a Terrorist Attack." *American Economic Review* 94 (1): 115–133.

Di Tella, Rafael, Ernesto Schargrodsky, and Sebastian Galiani. 2010. "Crime Distribution and Victim Behavior During a Crime Wave." In *The Economics of Crime: Lessons for and from Latin America*, ed. Rafael Di Tella, Ernesto Schargrodsky, and Sebastian Galiani. Chicago, IL: University of Chicago Press.

Diehl, Michael. 1990. "The Minimal Group Paradigm: Theoretical Explanations and Empirical Findings." *European Review of Social Psychology* 1 (1): 263–292.

Dimick, Matthew. 2015. "Labor Market Institutions and Redistributive Voting." Buffalo Legal Studies Research Paper Series, No. 2015-028. SUNY Buffalo Law School.

Dimick, Matthew, David Rueda, and Daniel Stegmueller. 2016. "The Altruistic Rich? Inequality and Other-Regarding Preferences for Redistribution." *Quarterly Journal of Political Science* 11 (4): 385–439.

Dimick, Matthew, David Rueda, and Daniel Stegmueller. 2018. "Models of Other-Regarding Preferences and Redistribution." *Annual Review of Political Science* 21: 441–460.

Donohue, John J., and Justin Wolfers. 2006. "Uses and Abuses of Empirical Evidence in the Death Penalty Debate." *The Economists' Voice* 3 (5): 791–846.

Dowler, Kenneth. 2003. "Media Consumption and Public Attitudes Toward Crime and Justice: The Relationship between Fear of Crime, Punitive Attitudes, and Perceived Police Effectiveness." *Journal of Criminal Justice and Popular Culture* 10 (2): 109–126.

Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York, NY: Harper & Row.

Druckman, James N., and Cindy Kam. 2011. "Students as Experimental Participants: A Defence of the 'Narrow Data Base'." In *The Cambridge Handbook of Experimental Political Science*, ed. James Druckman, Donald Green, and Arthur Lupia. Cambridge: Cambridge University Press.

Duffield, Mark. 1998. "Post-Modern Conflict: Warlords, Post-Adjustment States and Private Protection." *Civil Wars* 1 (1): 65–102.

Duggan, Mark. 2001. "More Guns, More Crime." *Journal of Political Economy* 109 (5): 1086–1114.

Durante, Ruben, Louis Putterman, and Joel van der Weele. 2014. "Preferences for Redistribution and Perceptions of Fairness: An Experimental Study." *Journal of the European Economic Association* 12 (4): 1059–1086.

Durlauf, Steven N. 1996. "A Theory of Persistent Income Inequality." *Journal of Economic Growth* 1 (1): 75–93.

Dworkin, Ronald. 1981a. "What is Equality? Part 1: Equality of Welfare." *Philosophy & Public Affairs* 10 (3): 185–246.

Dworkin, Ronald. 1981b. "What is Equality? Part 2: Equality of Resources." *Philosophy & Public Affairs* 10 (4): 283–345.

Dynan, Karen E., Jonathan Skinner, and Stephen P. Zeldes. 2004. "Do the Rich Save More?" *Journal of Political Economy* 112 (2): 397–444.

Eckel, Catherine C., and Philip J. Grossman. 2005. "Managing Diversity by Creating Team Identity." *Journal of Economic Behavior and Organization* 58 (3): 371–392.

Ehrlich, Isaac. 1973. "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation." *Journal of Political Economy* 81 (3): 521.

Ehrlich, Isaac. 1975. "The Deterrent Effect of Capital Punishment: A Question of Life and Death." *American Economic Review* 65 (3): 397–417.

Ehrlich, Isaac. 1977a. "Capital Punishment and Deterrence: Some Further Thoughts and Additional Evidence." *Journal of Political Economy* 85 (4): 741–788.

Ehrlich, Isaac. 1977b. "The Deterrent Effect of Capital Punishment: Reply." *American Economic Review* 67 (3): 452–458.

Ehrlich, Isaac. 1996. "Crime, Punishment, and the Market for Offenses." *Journal of Economic Perspectives* 10 (1): 43 – 67.

Enns, Peter K. 2014. "The Public's Increasing Punitiveness and Its Influence on Mass Incarceration in the United States." *American Journal of Political Science* 58 (4): 857–872.

Estevez-Abe, Margarita, Torben Iversen, and David Soskice. 2001. "Social Protection and the Formation of Skills: A Reinterpretation of the Welfare State." In *Varieties of Capitalism: The Institutional Foundations of Comparative Advantage*, ed. Peter A. Hall and David Soskice. New York, NY: Oxford University Press.

Eurostat. 2019. "Eurostat Regional Database."

Evans, Anthony M., and Joachim I. Krueger. 2009. "The Psychology (and Economics) of Trust." *Social and Personality Psychology Compass* 3 (6): 1003–1017.

Evans, Geoffrey, and James Tilley. 2012. "The Depoliticization of Inequality and Redistribution: Explaining the Decline of Class Voting." *Journal of Politics* 74 (4): 963–976.

Evans, William N., and Emily G. Owens. 2007. "COPS and Crime." *Journal of Public Economics* 91 (1-2): 181–201.

Fajnzylber, Pablo, Daniel Lederman, and Norman Loayza. 2002a. "Inequality and Violent Crime." *Journal of Law and Economics* 45 (1): 1–39.

Fajnzylber, Pablo, Daniel Lederman, and Norman Loayza. 2002b. "What Causes Violent Crime?" *European Economic Review* 46 (7): 1323–1357.

Falk, Armin, Stephan Meier, and Christian Zehnder. 2013. "Do Lab Experiments Misrepresent Social Preferences? The Case of Self-Selected Student Samples." *Journal of the European Economic Association* 11 (4): 839–852.

Fearon, James D. 2003. "Ethnic and Cultural Diversity by Country." *Journal of Economic Growth* 8 (2): 195–222.

Fehr, Ernst, and Klaus M. Schmidt. 2006. "The Economics of Fairness, Reciprocity and Altruism: Experimental Evidence and New Theories." In *The Handbook of the Economics of Giving, Altruism, and Reciprocity*, ed. Serge-Christophe Kolm and Jean Mercier Ythier. Vol. 1. London: Elsevier.

Ferraro, Kenneth F. 1995. *Fear of Crime: Interpreting Victimization Risk*. Albany, NY: State University of New York Press.

Ferraro, Kenneth F., and Randy LaGrange. 1987. "The Measurement of Fear of Crime." *Sociological Inquiry* 57 (1): 70–97.

Fershtman, Chaim, and Uri Gneezy. 2001. "Discrimination in a Segmented Society: An Experimental Approach." *Quarterly Journal of Economics* 116 (1): 351–377.

Finseraas, Henning. 2009. "Income Inequality and Demand for Redistribution: A Multilevel Analysis of European Public Opinion." *Scandinavian Political Studies* 32 (1): 94–119.

Fischbacher, Urs. 2007. "z-Tree: Zurich Toolbox for Ready-Made Economic Experiments." *Experimental Economics* 10 (2): 171–178.

Fischer, Claude S., Gretchen Stockmayer, Jon Stiles, and Michael Hout. 2004. "Distinguishing the Geographic Levels and Social Dimensions of U.S. Metropolitan Segregation, 1960-2000." *Demography* 41 (1): 37–59.

Fisher, Irving. 1930. *The Theory of Interest : As Determined by Impatience to Spend Income and Opportunity to Invest it*. New York, NY: Macmillan.

Fowles, Richard, and Mary Merva. 1996. "Wage Inequality and Criminal Activity: An Extreme Bounds Analysis for the United States, 1975–1990." *Criminology* 34 (2): 163–182.

Frank, Thomas. 2004. *What's the Matter with Kansas? How Conservatives Won the Heart of America*. New York, NY: Metropolitan Books.

Franklin, Travis W., Cortney A. Franklin, and Noelle E. Fearn. 2008. "A Multilevel Analysis of the Vulnerability, Disorder, and Social Integration Models of Fear of Crime." *Social Justice Research* 21 (2): 204–227.

Freeman, Richard B. 1996. "Why Do So Many Young American Men Commit Crimes and What Might We Do About It?" *Journal of Economic Perspectives* 10 (1): 25–42.

Freeman, Richard B. 1999. "The Economics of Crime." In *Handbook of Labor Economics*, ed. Orley Ashenfelter and David E. Card. Vol. 3C. New York, NY: North-Holland.

Freud, Sigmund. 1990. "Instincts and Their Vicissitudes." In *The Major Works of Sigmund Freud*. London: Encyclopaedia Britannica.

Friesen, Lana. 2012. "Certainty of Punishment versus Severity of Punishment: An Experimental Investigation." *Southern Economic Journal* 78 (2): 399–421.

Gambetta, Diego. 1993. *The Sicilian Mafia: The Business of Private Protection*. Cambridge, MA: Harvard University Press.

Garland, David. 1990. *Punishment and Modern Society: A Study in Social Theory*. Chicago, IL: University of Chicago Press.

Garland, David. 2001. *The Culture of Control: Crime and Social Order in Contemporary Society*. Chicago, IL: University of Chicago Press.

Garofalo, James. 1979. "Victimization and Fear of Crime." *Journal of Research in Crime and Delinquency* 16 (1): 80–97.

Garofalo, James, and John Laub. 1978. "The Fear of Crime: Broadening Our Perspective." *Victimology* 3 (3-4): 242–253.

Gaubatz, Kathlyn T. 1995. *Crime in the Public Mind*. Ann Arbor, MI: University of Michigan Press.

Gault, Barbara A., and John Sabini. 2000. "The Roles of Empathy, Anger, and Gender in Predicting Attitudes toward Punitive, Reparative, and Preventative Public Policies." *Cognition & Emotion* 14 (4): 495–520.

Gelman, Andrew. 2008. *Red State, Blue State, Rich State, Poor State: Why Americans Vote the Way They Do*. Princeton, NJ: Princeton University Press.

Gelman, Andrew. 2010. *Red State, Blue State, Rich State, Poor State: Why Americans Vote the Way They Do, Expanded Edition*. Princeton, NJ: Princeton University Press.

Gilens, Martin. 1996. "Race Coding and White Opposition to Welfare." *American Political Science Review* 90 (3): 593–604.

Gilens, Martin. 2005. "Inequality and Democratic Responsiveness." *Public Opinion Quarterly* 69 (5): 778–796.

Gilens, Martin. 2012. *Affluence and Influence: Economic Inequality and Political Power in America*. Princeton, NJ: Princeton University Press.

Gilliam, Franklin D., and Shanto Iyengar. 2000. "Prime Suspects: The Influence of Local Television News on the Viewing Public." *American Journal of Political Science* 44 (3): 560.

Glaeser, Edward L. 1999. "An Overview of Crime and Punishment." Working Paper. Harvard University.

Goette, Lorenz, David Huffman, and Stephan Meier. 2006. "The Impact of Group Membership on Cooperation and Norm Enforcement." *American Economic Review* 96 (2): 212–216.

Gonzalez-Navarro, Marco. 2013. "Deterrence and Geographical Externalities in Auto Theft." *American Economic Journal: Applied Economics* 5 (4): 92–110.

Green, Leonard, Joel Myerson, David Lichtman, Suzanne Rosen, and Astrid Fry. 1996. "Temporal Discounting in Choice Between Delayed Rewards: The Role of Age and Income." *Psychology and Aging* 11 (1): 79–84.

Greene, William. 2001. "Estimating Econometric Models with Fixed Effects." Working Paper. New York University.

Gregorio, Jose De, and Jong-Wha Lee. 2002. "Education and Income Inequality: New Evidence From Cross-Country Data." *Review of Income and Wealth* 48 (3): 395–416.

Greiner, Ben. 2015. "Subject Pool Recruitment Procedures: Organizing Experiments with ORSEE." *Journal of the Economic Science Association* 1 (1): 114–125.

Grogger, Jeffrey. 1991. "Certainty vs. Severity of Punishment." *Economic Inquiry* 29 (2): 297–309.

Guerin, Bernard. 1999. "Children's Intergroup Attribution Bias for Liked and Disliked Peers." *Journal of Social Psychology* 139 (5): 583–589.

Güth, Werner, M. Vittoria Levati, and Matteo Ploner. 2008. "Social Identity and Trust: An Experimental Investigation." *Journal of Socio-Economics* 37 (4): 1293–1308.

Habyarimana, James, Macartan Humphreys, Daniel M. Posner, and Jeremy M. Weinstein. 2007. "Why does Ethnic Diversity Undermine Public Good Provision?" *American Political Science Review* 101 (4): 709–725.

Hagan, John, and Ruth D. Peterson. 1995. "Crime and Inequality in America." In *Crime and Inequality*, ed. John Hagan and Ruth D. Peterson. Stanford, CA: Stanford University Press.

Hakim, Simon, and Yochanan Shachmurove. 1996. "Spatial and Temporal Patterns of Commercial Burglaries." *American Journal of Economics and Sociology* 55 (4): 443–456.

Hale, Chris. 1996. "Fear of Crime: A Review of the Literature." *International Review of Victimology* 4 (2): 79–150.

Hall, Peter A., and David Soskice. 2001. *Varieties of Capitalism: The Institutional Foundations of Comparative Advantage*. Oxford: Oxford University Press.

Harbaugh, William T., Naci Mocan, and Michael S. Visser. 2013. "Theft and Deterrence." *Journal of Labor Research* 34 (4): 389–407.

Hardin, James W., and Joseph M. Hilbe. 2012. *Generalized Linear Models and Extensions*. 3rd ed. College Station, TX: Stata Press.

Harrison, Glenn W., Morten I. Lau, and Melonie B. Williams. 2002. "Estimating Individual Discount Rates in Denmark: A Field Experiment." *American Economic Review* 92 (5): 1606–1617.

Häusermann, Silja, Thomas Kurer, and Hanna Schwander. 2015. "High-Skilled Outsiders? Labor Market Vulnerability, Education and Welfare State Preferences." *Socio-Economic Review* 13 (2): 235–258.

Haushofer, Johannes, and Ernst Fehr. 2014. "On the Psychology of Poverty." *Science* 344 (6186): 862–867.

Hausman, Jerry A. 1979. "Individual Discount Rates and the Purchase and Utilization of Energy-Using Durables." *Bell Journal of Economics* 10 (1): 33–54.

Hay, Jonathan R., and Andrei Shleifer. 1998. "Private Enforcement of Public Laws: A Theory of Legal Reform." *American Economic Review* 88 (2): 398–403.

Heider, Fritz. 1958. *The Psychology of Interpersonal Relations*. New York, NY: John Wiley.

Helsley, Robert W., and William C. Strange. 2005. "Mixed Markets and Crime." *Journal of Public Economics* 89 (7): 1251–1275.

Hersh, Eitan D., and Clayton Nall. 2016. "The Primacy of Race in the Geography of Income-Based Voting: New Evidence from Public Voting Records." *American Journal of Political Science* 60 (2): 289–303.

Hewstone, Miles. 1990. "The 'Ultimate Attribution Error'? A Review of the Literature on Intergroup Causal Attribution." *European Journal of Social Psychology* 20 (4): 311–335.

Hewstone, Miles, and Colleen Ward. 1985. "Ethnocentrism and Causal Attribution in Southeast Asia." *Journal of Personality and Social Psychology* 48 (3): 614–623.

Hilbe, Joseph M. 2012. *Negative Binomial Regression*. Cambridge, MA: Cambridge University Press.

Hirtenlehner, Helmut. 2011. "The Origins of Punitive Mentalities in Late Modern Societies: Testing an Expressive Explanatory Model." In *Punitivity: International Developments*, ed. Helmut Kury and Evelyn Shea. Bochum: Brockmeyer.

Hogan, Michael J., Ted Chiricos, and Marc Gertz. 2005. "Economic Insecurity, Blame, and Punitive Attitudes." *Justice Quarterly* 22 (3): 392–412.

Hogg, Michael A. 2008. "Social Categorization, Depersonalization, and Group Behavior." In *Blackwell Handbook of Social Psychology*, ed. Michael A Hogg and Scott Tindale. Oxford: Blackwell Publishing.

Holloway, Wendy, and Tony Jefferson. 2000. "The Role of Anxiety in Fear of Crime." In *Crime, Risk, and Insecurity: Law and Order in Everyday Life and Political Discourse*, ed. Richard Sparks and Tim. Hope. Abingdon: Routledge.

Hornsey, Matthew J. 2008. "Social Identity Theory and Self-Categorization Theory: A Historical Review." *Social and Personality Psychology Compass* 2: 204–222.

Hough, Michael, Ben Bradford, Jonathan Jackson, and Julian V. Roberts. 2013. *Attitudes to Sentencing and Trust in Justice: Exploring Trends from the Crime Survey for England and Wales*. London: Ministry of Justice Analytical Services.

Hout, Michael. 2004. "Getting the Most Out of the GSS Income Measures." GSS Methodoligical Report, No 101.

Huber, John D., and Piero Stanig. 2009. "Individual Income and Voting for Redistribution Across Democracies." Working Paper. Columbia University.

Huddy, Leonie, and Stanley Feldman. 2009. "On Assessing the Political Effects of Racial Prejudice." *Annual Review of Political Science* 12 (1): 423–447.

Hui-Wen, Koo, and Ivan P. Png. 1994. "Private Security: Deterrent or Diversion." *International Review of Law and Economics* 14 (1): 87–101.

Hummelsheim, Dina, Helmut Hirtenlehner, Jonathan Jackson, and Dietrich Oberwitter. 2010. "Social Insecurities and Fear of Crime: A Cross-National Study on the Impact of Welfare State Policies on Crime-Related Anxieties." *European Sociological Review* 27 (3): 327–345.

Hurwitz, Jon, and Mark Peffley. 1997. "Public Perceptions of Race and Crime: The Role of Racial Stereotypes." *American Journal of Political Science* 41 (2): 375–401.

Hurwitz, Jon, and Mark Peffley. 2005. "Playing the Race Card in the Post-Willie Horton Era: The Impact of Racialized Code Words on Support for Punitive Crime Policy." *Public Opinion Quarterly* 69 (1): 99–112.

Hurwitz, Jon, and Shannon Smithey. 1998. "Gender Differences on Crime and Punishment." *Political Research Quarterly* 51 (1): 89–115.

Imrohoroglu, Ayse, Antonio Merlo, and Peter Rupert. 2000. "On the Political Economy of Income Redistribution and Crime." *International Economic Review* 41 (1): 1–25.

Imrohoroglu, Ayse, Antonio Merlo, and Peter Rupert. 2004. "What Accounts for the Decline in Crime?" *International Economic Review* 45 (3): 707–729.

Inglehart, Ronald F. 2008. "Changing Values among Western Publics from 1970 to 2006." *West European Politics* 31 (1-2): 130–146.

Islam, Mir R., and Miles Hewstone. 1993. "Intergroup Attributions and Affective Consequences in Majority and Minority Groups." *Journal of Personality and Social Psychology* 64 (6): 936–950.

Iversen, Torben, and David Soskice. 2001. "An Asset Theory of Social Policy Preferences." *American Political Science Review* 95 (4): 875–893.

Iversen, Torben, and David Soskice. 2006. "Electoral Institutions and the Politics of Coalitions: Why Some Democracies Redistribute More Than Others." *American Political Science Review* 100 (2): 165–181.

Iversen, Torben, and David Soskice. 2009. "Distribution and Redistribution: The Shadow of the Nineteenth Century." *World Politics* 61 (3): 438–486.

Iversen, Torben, and Frances M. Rosenbluth. 2006. "The Political Economy of Gender: Explaining Cross-National Variation in Household Bargaining, Divorce and the Gender Voting Gap." *American Journal of Political Science* 50 (1): 1–19.

Jaeger, Mads M. 2006. "Welfare Regimes and Attitudes Towards Redistribution: The Regime Hypothesis Revisited." *European Sociological Review* 22 (2): 157–170.

Jin, Nobuhito, and Toshio Yamagishi. 1997. "Group Heuristics in Social Dilemma." *Japanese Journal of Social Psychology* 12 (3): 190–198.

Joh, Elizabeth E. 2004. "The Paradox of Private Policing." *Journal of Criminal Law and Criminology* 95 (1): 49–132.

Johnson, Devon. 2001. "Punitive Attitudes on Crime: Economic Insecurity, Racial Prejudice, or Both?" *Sociological Focus* 34 (1): 33–54.

Kahn, Karl, and David Rueda. 2019. "Insurance Effects on Support for Redistribution: An Experimental Approach." Working Paper. University of Oxford.

Kahn, Robert S., Paul H. Wise, Bruce P. Kennedy, and Ichiro Kawachi. 2000. "State Income Inequality, Household Income, and Maternal Mental and Physical Health: Cross Sectional National Survey." *British Medical Journal* 321: 1311–1315.

Kang, Shin-Goo, and G. Bingham Powell. 2010. "Representation and Policy Responsiveness: The Median Voter, Election Rules, and Redistributive Welfare Spending." *Journal of Politics* 72 (4): 1014–1028.

Karabarbounis, Loukas. 2011. "One Dollar, One Vote." *Economic Journal* 121 (553): 621–651.

Karl, Terry Lynn. 2000. "Economic Inequality and Democratic Instability." *Journal of Democracy* 11 (1): 149–156.

Kawachi, Ichiro, Bruce P. Kennedy, Kimberly Lochner, and Deborah Prothrow-Stith. 1997. "Social Capital, Income Inequality, and Mortality." *American Journal of Public Health* 87 (9): 1491–1498.

Kelly, Morgan. 2000. "Inequality and Crime." *Review of Economics and Statistics* 82 (4): 530–539.

Kenworthy, Lane, and Jonas Pontusson. 2005. "Rising Inequality and the Politics of Redistribution in Affluent Countries." *Perspectives on Politics* 3 (3): 449–471.

Kenworthy, Lane, and Leslie McCall. 2008. "Inequality, Public Opinion and Redistribution." *Socio-Economic Review* 6 (1): 35–68.

Khadjavi, Menush. 2014. "On the Interaction of Deterrence and Emotions." *Journal of Law, Economics, and Organization* 31 (2): 287–319.

Khwaja, Ahmed, Dan Silverman, and Frank Sloan. 2007. "Time Preference, Time Discounting, and Smoking Decisions." *Journal of Health Economics* 26 (5): 927–949.

King, Anna, and Shadd Maruna. 2009. "Is a Conservative just a Liberal who has been Mugged? Exploring the Origins of Punitive Views." *Punishment & Society* 11 (2): 147–169.

King, Gary, Christopher Murray, Joshua Salomon, and Ajay Tandon. 2004. "Enchancing the Validty and Cross-Cultural Comparability of Measurement in Survey Research." *American Political Science Review* 98 (1): 191–207.

King, Ryan D., and Darren Wheelock. 2007. "Group Threat and Social Control: Race, Perceptions of Minorities and the Desire to Punish." *Social Forces* 85 (3): 1255–1280.

Kleck, Gary, and Dylan B. Jackson. 2016. "Does Crime Cause Punitiveness?" *Crime & Delinquency*: 1–28.

Klein, Melanie. 1984. *Love, Guilt, and Reparation, and Other Works, 1921-1945*. New York, NY: Free Press.

Klick, Jonathan, and Alexander Tabarrok. 2005. "Using Terror Alert Levels to Estimate the Effect of Police on Crime." *Journal of Law and Economics* 48 (1): 267–279.

Kury, Helmut, and Theodore Ferdinand. 1999. "Public Opinion and Punitivity." *International Journal of Law and Psychiatry* 22 (3-4): 373–392.

Kuznets, Simon. 1955. "Economic Growth and Income Inequality." *American Economic Review* 45 (1): 1–28.

Kyckelhahn, Tracey. 2011. "Justice Expenditures and Employment, FY 1982-2007 - Statistical Tables." Bureau of Justice Statistics. https://www.bjs.gov/content/pub/pdf/jee8207st.pdf.

Laband, David N., and John P. Sophocleus. 1992. "An Estimate of Resource Expenditures on Transfer Activity in the United States." *Quarterly Journal of Economics* 107 (3): 959–983.

Lacey, Nicola. 2008. *The Prisoners' Dilemma: Political Economy and Punishment in Contemporary Democracies*. Cambridge: Cambridge University Press.

Lacey, Nicola. 2010. "American Imprisonment in Comparative Perspective." *Daedalus* 139 (3): 102–114.

Lacey, Nicola, and David Soskice. 2015. "Crime, Punishment and Segregation in the United States: The Paradox of Local Democracy." *Punishment & Society* 17 (4): 454–481.

Langworthy, Robert H., and John T. Whitehead. 1986. "Liberalism and Fear as Explanations for Punitiveness." *Criminology* 24 (3): 575–591.

Lau, Richard R., and Dan Russell. 1980. "Attributions in the Sports Pages." *Journal of Personality and Social Psychology* 39 (1): 29–38.

Lawrance, Emily C. 1991. "Poverty and the Rate of Time Preference: Evidence from Panel Data." *Journal of Political Economy* 99 (1): 54–77.

Lemanski, Charlotte. 2004. "A New Apartheid? The Spatial Implications of Fear of Crime in Cape Town, South Africa." *Environment and Urbanization* 16 (2): 101–112.

Levitt, Steven D. 1996. "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation." *Quarterly Journal of Economics* 111 (2): 319–351.

Levitt, Steven D. 1997. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime." *American Economic Review* 87 (3): 270–290.

Levitt, Steven D. 2002. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: Reply." *American Economic Review* 92 (4): 1244–1250.

Levitt, Steven D., and Thomas J. Miles. 2007. "Empirical Study of Criminal Punishment." In *Handbook of Law and Economics*, ed. A. Mitchell Polinsky and Steven Shavell. Vol. 1. London: Elsevier.

Lindert, Peter. 2004. *Growing Public: Social Spending and Economic Growth Since the Eighteenth Century*. Cambridge: Cambridge University Press.

Lipset, Seymour Martin. 1996. *American Exceptionalism: A Double-Edged Sword*. New York, NY: W. W. Norton.

List, John A. 2001. "Do Explicit Warnings Eliminate the Hypothetical Bias in Elicitation Procedures? Evidence from Field Auctions for Sportscards." *American Economic Review* 91 (dec): 1498–1507.

List, John A., and Todd L. Cherry. 2008. "Examining the Role of Fairness in High Stakes Allocation Decisions." *Journal of Economic Behavior and Organization* 65: 1–8.

Listokin, Yair. 2007. "Crime and (with a Lag) Punishment: The Implications of Discounting for Equitable Sentencing." *American Criminal Law Review* 44 (115): 115–140.

Long, Scott. 1997. *Regression Models for Categorical and Limited Dependent Variables*. Thousand Oaks, CA: Sage Publications.

Lott, John R. 2010. *More Guns, Less Crime: Understanding Crime and Gun-Control Laws*. Chicago, IL: University of Chicago Press.

Lott, John R., and David B. Mustard. 1997. "Crime, Deterrence, and Right-to-Carry Concealed Handguns." *Journal of Legal Studies* 26 (1): 1–68.

Lupu, Noam, and Jonas Pontusson. 2011. "The Structure of Inequality and the Politics of Redistribution." *American Political Science Review* 105 (2): 316–336.

Luttmer, Erzo F. P. 2001. "Group Loyalty and the Taste for Redistribution." *Journal of Political Economy* 109 (3): 500–528.

Luttmer, Erzo F. P., and Monica Singhal. 2011. "Culture, Context, and the Taste for Redistribution." *American Economic Journal: Economic Policy* 3 (1): 157–179.

Mahler, Vincent A. 2008. "Electoral Turnout and Income Redistribution by the State: A Cross-National Analysis of the Developed Democracies." *European Journal of Political Research* 47 (2): 161–183.

Mandel, Michael J., Paul Magnusson, James E. Ellis, Gail DeGeorge, and Keith L. Alexander. 1993. "The Economics of Crime." *Business Week* 13: 72–81.

Mani, Anandi, Sendhil Mullainathan, Eldar Shafir, and Jiaying Zhao. 2013. "Poverty Impedes Cognitive Function." *Science* 341 (6149): 976–980.

Martinsson, Johan, and Ulrika Andersson. 2018. "Svenska Trender, 1986-2017." SOM-Institutet, University of Gothenburg.

Maruna, Shadd, Amanda Matravers, and Anna King. 2004. "Disowning Our Shadow: A Psychoanalytic Approach to Understanding Punitive Public Attitudes." *Deviant Behavior* 25 (3): 277–299.

Marvell, Thomas B., and Carlisle E. Moody. 1994. "Prison Population Growth and Crime Reduction." *Journal of Quantitative Criminology* 10 (2): 109–140.

Marvell, Thomas B., and Carlisle E. Moody. 1996. "Specification Problems, Police Levels, and Crime Rates." *Criminology* 34 (4): 609–646.

Marx, Karl. 1934. *The Class Struggles in France, 1848-1850*. London: Martin Lawrence.

Mayhew, Patricia, and Jan J. M. van Dijk. 1997. "Criminal Victimisation in Eleven Industrialised Countries: Key findings from the 1996 International Crime Victims Survey." WODC/Home Office Report.

Mayhew, Patricia, and John van Kesteren. 2002. "Cross-National Attitudes to Punishment." In *Changing Attitudes to Punishment: Public Opinion, Crime and Justice*, ed. Julian V. Roberts and Mike Hough. Cullompton: Willian Publishing.

McAdams, Richard H. 2010. "Economic Costs of Inequality." *University of Chicago Legal Forum* 23 (1): 23–41.

McCarty, Nolan, Keith T. Poole, and Howard Rosenthal. 2006. *Polarized America: The Dance of Ideology and Unequal Riches*. Cambridge, MA: MIT Press.

McCorkle, Richard C. 1993. "Research Note: Punish and Rehabilitate? Public Attitudes Toward Six Common Crimes." *Crime & Delinquency* 39 (2): 240–252.

McCoy, Terrence. 2012. "How Joseph Stalin Invented 'American Exceptionalism'." *The Atlantic* (jan).

McCrary, Justin. 2002. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: Comment." *American Economic Review* 92 (4): 1236–1243.

Meguid, Bonnie M. 2005. "Competition Between Unequals: The Role of Mainstream Party Strategy in Niche Party Success." *American Political Science Review* 99 (aug): 347–359.

Meltzer, Allan H., and Scott F. Richard. 1981. "A Rational Theory of Government." *Journal of Political Economy* 89 (5): 914–927.

Merton, Robert K. 1938. "Social Structure and Anomie." *American Sociological Review* 3 (5): 672–682.

Milanovic, Branko. 2000. "The Median-Voter Hypothesis, Income Inequality, and Income Redistribution: An Empirical Test with the Required Data." *European Journal of Political Economy* 16 (3): 367–410.

Milhaupt, Curtis J., and Mark D. West. 2000. "The Dark Side of Private Ordering: An Institutional and Empirical Analysis of Organized Crime." *University of Chicago Law Review* 67 (1): 41–98.

Moene, Karl Ove, and Michael Wallerstein. 2001. "Inequality, Social Insurance, and Redistribution." *American Political Science Review* 95 (4): 859–874.

Moene, Karl Ove, and Michael Wallerstein. 2003. "Earnings Inequality and Welfare Spending: A Disaggregated Analysis." *World Politics* 55 (4): 485–516.

Morgan, Ted. 1999. *A Covert Life: Jay Lovestone, Communist, Anti-Communist, and Spymaster*. New York, NY: Random House.

Morton, Rebecca, and Kenneth Williams. 2010. *Experimental Political Science and the Study of Causality: From Nature to the Lab*. Oxford: Oxford University Press.

Muller, Edward N. 1988. "Democracy, Economic Development, and Income Inequality." *American Sociological Review* 53 (1): 50.

Muller, Edward N. 1995. "Economic Determinants of Democracy." *American Sociological Review* 60 (6): 966.

Mungan, Murat C., and Jonathan Klick. 2015. "Identifying Criminals' Risk Preferences." Working Paper. University of Pennsylvania.

Neilson, William S., and Harold Winter. 1997. "On Criminals' Risk Attitudes." *Economic Letters* 55 (1): 97–102.

Nelson, Richard, and Edmund Phelps. 1996. "Investment in Humans, Technological Diffusion, and Economic Growth." *American Economic Review* 56 (2): 69–75.

Nozick, Robert. 1974. *Anarchy, State, and Utopia*. New York, NY: Basic Books.

Oakes, P. J., and J. C. Turner. 1980. "Social Categorization and Intergroup Behaviour: Does Minimal Intergroup Discrimination make Social Identity more Positive?" *European Journal of Social Psychology* 10 (3): 295–301.

OECD. 2015. *In It Together: Why Less Inequality Benefits All*. Paris: OECD Publishing.

OECD. 2019a. "Income Distribution Database." OECD (Organization for Economic Co-Operation and Development).

OECD. 2019b. "Statistical Library." OECD (Organization for Economic Co-Operation and Development).

Ortega, Suzanne T., and Jessie L. Myles. 1987. "Race and Gender Effects on Fear of Crime: An Interactive Model with Age." *Criminology* 25 (1): 133–152.

Oxoby, Robert J., and John Spraggon. 2008. "Mine and Yours: Property Rights in Dictator Games." *Journal of Economic Behavior and Organization* 65 (3-4): 703–713.

Page, Benjamin I., and Lawrence R. Jacobs. 2009. *Class War? What Americans Really Think about Economic Inequality*. Chicago, IL: University of Chicago Press.

Papke, Leslie E., and Jeffrey M. Wooldridge. 1996. "Econometric Methods for Fractional Response Variables with an Application to 401(k) Plan Participation Rates." *Journal of Applied Econometrics* 11 (6): 619–632.

Pastore, Ann L., and Kathleen Maguire. 2005. *Sourcebook of Criminal Justice Statistics*. Department of Justice, Bureau of Justice Statistics. Washington, DC: USGPO.

Patterson, Britt E. 1991. "Poverty, Income Inequality, and Community Crime Rates." *Criminology* 29 (4): 755–776.

Payne, Brian K., Randy R. Gainey, Ruth A. Triplett, and Mona J. E. Danner. 2004. "What Drives Punitive Beliefs? Demographic Characteristics and Justifications for Sentencing." *Journal of Criminal Justice* 32 (3): 195–206.

Peffley, Mark, and Jon Hurwitz. 2002. "The Racial Components of "Race-Neutral" Crime Policy Attitudes." *Political Psychology* 23 (1): 59–75.

Peffley, Mark, Jon Hurwitz, and Paul M. Sniderman. 1997. "Racial Stereotypes and Whites' Political Views of Blacks in the Context of Welfare and Crime." *American Journal of Political Science* 41 (2): 30–60.

Penn, Mark, and Andrew Stein. 2017. "Back to the Center, Democrats." *New York Times*.

Perotti, Roberto. 1996. "Growth, Income Distribution, and Democracy: What the Data Say." *Journal of Economic Growth* 1: 149–187.

Persson, Torsten, and Guido Tabellini. 1994. "Is Inequality Harmful for Growth?" *American Economic Review* 84 (3): 600–621.

Persson, Torsten, and Guido Tabellini. 2000. *Political Economics: Explaining Economic Policy*. Cambridge, MA: MIT Press.

Peterson, Robert A. 2001. "On the Use of College Students in Social Science Research: Insights from a Second-Order Meta-Analysis." *Journal of Consumer Research* 28 (3): 450–461.

Pettigrew, Thomas F. 1979. "The Ultimate Attribution Error: Extending Allport's Cognitive Analysis of Prejudice." *Personality and Social Psychology Bulletin* 5 (4): 461–476.

Pettit, Becky, and Bruce Western. 2004. "Mass Imprisonment and the Life Course: Race and Class Inequality in U.S. Incarceration." *American Sociological Review* 69 (2): 151–169.

Philipson, Tomas J., and Richard A. Posner. 1996. "The Economic Epidemiology of Crime." *Journal of Law and Economics* 39 (2): 405–433.

Piketty, Thomas. 2014. *Capital in the Twenty-First Century*. Cambridge, MA: Harvard University Press.

Piketty, Thomas, and Emmanuel Saez. 2007. "How Progressive is the U.S. Federal Tax System? A Historical and International Perspective." *Journal of Economic Perspectives* 21 (1): 3–24.

Polinsky, A. Mitchell, and Steven Shavell. 1979. "The Optimal Tradeoff Between the Probability and Magnitude of Fines." *American Economic Review* 69 (5): 880–891.

Polinsky, A. Mitchell, and Steven Shavell. 2000. "The Economic Theory of Public Enforcement of Law." *Journal of Economic Literature* 38 (1): 45–76.

Posner, Richard A. 1985. "An Economic Theory of the Criminal Law." *Columbia Law Review* 85 (6): 1193–1231.

Przeworski, Adam. 1986. *Capitalism and Social Democracy*. Cambridge: Cambridge University Press.

Przeworski, Adam. 2009. "Democracy, Equality, and Redistribution." In *Political Judgement: Essays for John Dunn*, ed. Richard Bourke and Raymond Geuss. Cambridge: Cambridge University Press.

Przeworski, Adam. 2010. *Democracy and the Limits of Self-Government*. New York, NY: Cambridge University Press.

Rabe-Hesketh, Sophia, and Anders Skrondal. 2012. *Multilevel and Longitudinal Modeling Using Stata*. 3 ed. College Station, TX: Stata Press.

Ramalho, Esmeralda A., Joaquim J. S. Ramalho, and Jose M. R. Murteira. 2011. "Alternative Estimating and Testing Empirical Strategies for Fractional Regression Models." *Journal of Economic Surveys* 25 (1): 19–68.

Ramirez, Mark D. 2013. "Americans' Changing Views on Crime and Punishment." *Public Opinion Quarterly* 77 (4): 1006–1031.

Rawls, John. 1971. *A Theory of Justice*. Cambridge, MA: Harvard University Press.

Rehm, Philipp. 2009. "Risks and Redistribution: An Individual-Level Analysis." *Comparative Political Studies* 42 (7): 855–879.

Reimers, Stian, Elizabeth A. Maylor, Neil Stewart, and Nick Chater. 2009. "Associations Between a One-Shot Delay Discounting Measure and Age, Income, Education and Real-World Impulsive Behavior." *Personality and Individual Differences* 47 (8): 973–978.

Riviera, Mauricio, and Barbara Zarate-Tenorio. 2016. "Beyond sticks and stones: Human capital enhancement efforts in response to violent crime in Latin America." *European Journal of Political Research* 55 (3): 531–548.

Rizzolli, Matteo, and Luca Stanca. 2012. "Judicial Errors and Crime Deterrence: Theory and Experimental Evidence." *Journal of Law and Economics* 55 (2): 311–338.

Roberts, Julian V., Loretta J. Stalans, David Indermaur, and Michael Hough. 2003. *Penal Populism and Public Opinion: Lessons from Five Countries*. New York, NY: Oxford University Press.

Roberts, Lynne D., and David Indermaur. 2007. "Predicting Punitive Attitudes in Australia." *Psychiatry, Psychology and Law* 14 (1): 56–65.

Rodriguez, Francisco. 1999. "Does Distributional Skewness Lead to Redistribution? Evidence from the United States." *Economics and Politics* 11 (2): 171–199.

Roemer, John E. 1996. *Theories of Distributive Justice*. Cambridge, MA: Harvard University Press.

Roman, John K., and Christy Visher. 2009. "Prisoner Reentry Programming." In *Investing in the Disadvantaged: Assessing the Benefits and Costs of Social Policies*, ed. David L. Weimer and Aidan R. Vining. Washington, DC: Georgetown University Press.

Romer, Paul. 1990. "Endogenous Technological Change." *Journal of Political Economy* 98 (5): 71–102.

Romer, Thomas. 1975. "Individual Welfare, Majority Voting, and the Properties of a Linear Income Tax." *Journal of Public Economics* 4 (2): 163–185.

Ross, Lee. 1977. "The Intuitive Psychologist And His Shortcomings: Distortions in the Attribution Process." *Advances in Experimental Social Psychology* 10: 173–220.

Rossi, Peter H., and Richard A. Berk. 1997. *Just Punishments: Federal Guidelines and Public Views Compared*. Hawthorne, NY: Aldine de Gruter.

Rothstein, Bo, and Eric M. Uslaner. 2005. "All for All: Equality, Corruption, and Social Trust." *World Politics* 58 (1): 41–72.

Rueda, David. 2005. "Insider-Outsider Politics in Industrialized Democracies: The Challenge to Social Democratic Parties." *American Political Science Review* 99 (1): 61–74.

Rueda, David. 2007. *Social Democracy Inside Out: Partisanship and Labor Market Policy in Industrialized Democracies*. Oxford: Oxford University Press.

Rueda, David. 2018. "Food Comes First, then Morals: Redistribution Preferences, Altruism and Group Heterogeneity in Western Europe." *Journal of Politics* 80 (1): 225–239.

Rueda, David, and Daniel Stegmueller. 2016. "The Externalities of Inequality: Fear of Crime and Preferences for Redistribution in Western Europe." *American Journal of Political Science* 60 (2): 472–489.

Rueda, David, and Daniel Stegmueller. 2019. "Preferences that Matter: Inequality, Redistribution and Voting." Working Paper. University of Oxford.

Saint-Paul, Gilles. 2001. "The Dynamics of Exclusion and Fiscal Conservatism." *Review of Economic Dynamics* 4 (2): 275–302.

Saint-Paul, Gilles, and Thierry Verdier. 1996. "Inequality, Redistribution and Growth: A Challenge to the Conventional Political Economy Approach." *European Economic Review* 3 (40): 719–728.

Sakala, Leah. 2014. "Breaking Down Mass Incarceration in the 2010 Census: State-by-State Incarceration Rates by Race/Ethnicity." Prison Policy Initiative. https://www.prisonpolicy.org/reports/rates.html.

Scheve, Kenneth, and David Stasavage. 2006. "Religion and Preferences for Social Insurance." *Quarterly Journal of Political Science* 1 (3): 255–286.

Schildberg-Hörisch, Hannah, and Christina Strassmair. 2012. "An Experimental Test of the Deterrence Hypothesis." *Journal of Law, Economics, and Organization* 28 (3): 447–459.

Schopler, John, and Chester A. Insko. 1992. "The Discontinuity Effect in Interpersonal and Intergroup Relations: Generality and Mediation." *European Review of Social Psychology* 3 (1): 121–151.

Schumpeter, Joseph. 1944. *Capitalism, Socialism, and Democracy*. London: Allen and Unwin.

Sears, David O. 1986. "College Sophomores in the Laboratory: Influences of a Narrow Data Base on Social Psychology's View of Human Nature." *Journal of Personality and Social Psychology* 51 (3): 515–530.

Senik, Claudia, Holger Stichnoth, and Karine Straeten. 2009. "Immigration and Natives' Attitudes Towards the Welfare State: Evidence from the European Social Survey." *Social Indicators Research* 91 (3): 345–370.

Shapiro, Ian, Peter A. Swenson, and Daniela Donno. 2008. "Introduction." In *Divide and Deal: The Politics of Distribution in Democracies*, ed. Ian Shapiro, Peter A. Swenson, and Daniela Donno. New York, NY: New York University Press.

Shavell, Steven. 1991. "Individual Precautions to Prevent theft: Private Versus Socially Optimal Behavior." *International Review of Law and Economics* 11 (2): 123–132.

Shaver, Kelly G. 1975. *An Introduction to Attribution Processes*. Cambridge, MA: Winthrop Publishers.

Shaw, Clifford R., and Henry D. McKay. 1942. *Juvenile Delinquency in Urban Areas*. Chicago, IL: University of Chicago Press.

Shayo, Moses. 2009. "A Model of Social Identity with an Application to Political Economy: Nation, Class, and Redistribution." *American Political Science Review* 103 (2): 147–174.

Shearing, Clifford D. 1992. "The Relation Between Public and Private Policing." *Crime and Justice* 15 (1): 399–434.

Shelton, Cameron A. 2007. "The Size and Composition of Government Expenditure." *Journal of Public Economics* 91 (11-12): 2230–2260.

Shepherd, Joanna M. 2002. "Police, Prosecutors, Criminals, and Determinate Sentencing: The Truth about Truth-in-Sentencing Laws." *Journal of Law and Economics* 45 (2): 509–533.

Shinada, Mizuho, Toshio Yamagishi, and Yo Ohmura. 2004. "False friends are worse than bitter enemies: "Altruistic" punishment of in-group members." *Evolution and Human Behavior* 6 (25): 379–393.

Siebert, Horst. 1998. "Commentary: Economic Consequences of Income Inequality." In *Symposium of the Federal Reserve Bank of Kansas City on Income Inequality: Issues and Policy Options*. Kansas City, KA: Federal Reserve Bank of Kansas City.

Simpson, B. 2006. "Social Identity and Cooperation in Social Dilemmas." *Rationality and Society* 18 (4): 443–470.

Snijders, Tom A. B., and Roel J. Bosker. 2012. *Multilevel Analysis: An Introduction to Basic and Advanved Multilevel Modeling*. Thousand Oaks, CA: Sage Publications.

Soares, Rodrigo R. 2004. "Development, Crime and Punishment: Accounting for International Differences in Crime Rates." *Journal of Development Economics* 73 (1): 155–184.

Solt, Frederick. 2008. "Economic Inequality and Democratic Political Engagement." *American Journal of Political Science* 52 (1): 48–60.

Spears, Dean. 2011. "Economic Decision-Making in Poverty Depletes Behavioral Control." *The B.E. Journal of Economic Analysis and Policy* 11 (1): 1–42.

Stack, Steven. 1984. "Income Inequality and Property Crime: A Cross National Analysis of Relative Deprivation Theory." *Criminology* 22 (2): 229–256.

Stafford, Mark C., and Omer R. Galle. 1984. "Victimization Rates, Exposure to Risk, and Fear of Crime." *Criminology* 22 (2): 173–185.

Stegmueller, Daniel. 2013. "Religion and Redistributive Voting in Western Europe." *Journal of Politics* 75 (4): 1064–1076.

Stegmueller, Daniel, Peer Scheepers, Sigrid Rossteutscher, and Eelke De Jong. 2012. "Support for Redistribution in Western Europe: Assessing the Role of Religion." *European Sociological Review* 28 (4): 482–497.

Stephan, Walter G. 1977. "Stereotyping: The Role of Ingroup-Outgroup Differences in Causal Attribution for Behavior." *Journal of Social Psychology* 101 (2): 255–266.

Stigler, George J. 1970. "The Optimum Enforcement of Laws." *Journal of Political Economy* 78 (3): 526–536.

Stiglitz, Joseph E. 2013. *The Price of Inequality*. London: Penguin.

Stiglitz, Joseph E. 2016. "Inequality and Economic Growth." In *Rethinking Capitalism: Economics and Policy for Sustainable and Inclusive Growth*, ed. Michael Jacobs and Mariana Mazzucato. Chichester: Wiley-Blackwell.

Stinchcombe, Arthur L., Rebecca Adams, Carol A. Heimer, Kim Lane Scheppele, Tom W. Smith, and D. Garth Taylor. 1980. *Crime and Punishment: Changing Attitudes in America*. San Francisco, CA: Josey Bass.

Stonecash, Jeffrey. 2000. *Class and Party in American Politics*. New York, NY: Routledge.

Svallfors, Stefan. 1997. "Worlds of Welfare and Attitudes to Redistribution: A Comparison of Eight Western Nations." *European Sociological Review* 13 (3): 283–304.

Svallfors, Stefan. 2004. "Class, Attitudes and the Welfare State: Sweden in Comparative Perspective." *Social Policy and Administration* 38 (2): 119–138.

Tajfel, Henri, and J. C. Turner. 1986. "The Social Identity Theory of Intergroup Behavior." In *The Psychology of Intergroup Relations*, ed. Stephen Worchel and William Austin. Chicago, IL: Nelson-Hall.

Tajfel, Henri, and John Turner. 1979. "An Integrative Theory of Intergroup Conflict." In *The Social Psychology of Intergroup Relations*, ed. William G. Austin and Stephen Worchel. Monterey: Brooks/Cole.

Tajfel, Henri, M. G. Billig, R. P. Bundy, and Claude Flament. 1971. "Social Categorization and Intergroup Behaviour." *European Journal of Social Psychology* 1 (2): 149–178.

Tanaka, Tomomi, Colin F. Camerer, and Quang Nguyen. 2010. "Risk and Time Preferences: Linking Experimental and Household Survey Data from Vietnam." *American Economic Review* 100 (1): 557–571.

Tarrant, Mark, and Adrian C. North. 2004. "Explanations for Positive and Negative Behavior: the Intergroup Attribution Bias in Achieved Groups." *Current Psychology* 23 (2): 161–172.

Taylor, D. Garth, Kim Lane Scheppele, and Arthur L. Stinchcombe. 1979. "Salience of Crime and Support for Harsher Criminal Sanctions." *Social Problems* 26 (4): 413–424.

Taylor, Donald M., and Vaishna Jaggi. 1974. "Ethnocentrism and Causal Attribution in a South Indian Context." *Journal of Cross-Cultural Psychology* 5 (2): 162–171.

Thewissen, Stefan, and David Rueda. 2019. "Automation and the Welfare State: Technological Change as a Determinant of Redistribution Preferences." *Comparative Political Studies* 52 (2): 171–208.

Tonry, Michael. 2000. *The Handbook of Crime and Punishment*. New York, NY: Oxford University Press.

Tonry, Michael, and David P. Farrington. 2005. "Punishment and Crime Across Space and Time." *Crime and Justice* 33: 1–39.

Tseloni, Andromachi, and Christina Zarafonitou. 2008. "Fear of Crime and Victimization: A Multivariate Multilevel Analysis of Competing Measurements." *European Journal of Criminology* 5 (4): 387–409.

Tyler, Tom R., and Robert J. Boeckmann. 1997. "Three Strikes and You Are Out, but Why? The Psychology of Public Support for Punishing Rule Breakers." *Law & Society Review* 31 (2): 237–266.

UNDOC. 2019. "UNDOC (United Nations Office on Drugs and Crime) Data Portal."

Unnever, James D., and Francis T. Cullen. 2010. "The Social Sources of Americans' Punitiveness: A Test of Three Competing Models." *Criminology* 48 (1): 99–130.

Unnever, James D., Francis T. Cullen, and Bonnie S. Fisher. 2007. "A Liberal Is Someone Who Has Not Been Mugged: Criminal Victimization and Political Beliefs." *Justice Quarterly* 24 (2): 309–334.

Unnever, James D., Francis T. Cullen, and Brandon K. Applegate. 2005. "Turning the Other Cheek: Reassessing the Impact of Religion on Punitive Ideology." *Justice Quarterly* 22 (3): 304–339.

Unnever, James D., Francis T. Cullen, and James D. Jones. 2008. "Public Support for Attacking the "Root Causes" of Crime: The Impact of Egalitarian and Racial Beliefs." *Sociological Focus* 41 (1): 1–33.

van Dijk, Jan J. M., Patricia Mayhew, and Martin Killias. 1990. *Experiences of Crime Across the World: Key Findings from the 1989 International Crime Survey*. Deventer: Kluwer Law and Taxation Publishers.

Varese, Federico. 2001. *The Russian Mafia: Private Protection in a New Market Economy*. Oxford: Oxford University Press.

Vauclair, Christin-Melanie, and Boyka Bratanova. 2017. "Income Inequality and Fear of Crime Across the European Region." *European Journal of Criminology* 14 (2): 221–241.

Vieno, Alessio, Michele Roccato, and Silvia Russo. 2013. "Is Fear of Crime Mainly Social and Economic Insecurity in Disguise? A Multilevel Multinational Analysis." *Journal of Community and Applied Social Research* 23 (4): 519–535.

Vining, Aidan, and David L Weimer. 2010. "An Assessment of Important Issues Concerning the Application of Benefit-Cost Analysis to Social Policy." *Journal of Benefit-Cost Analysis* 1 (1): 1–40.

Vold, George B. 1958. *Theoretical Criminology*. New York, NY: Oxford University Press.

Volkov, Vadim. 2010. "Violent Entrepreneurship in Post-Communist Russia." *Europe-Asia Studies* 51 (4): 741–754.

Walmsley, Roy. 2018. "World Prison Population List, Twelfth Edition." World Prison Brief.

Warr, Mark. 1995. "Poll Trends: Public Opinion on Crime and Punishment." *Public Opinion Quarterly* 59 (2): 296–310.

Warr, Mark. 2000. "Fear of Crime in the United States : Avenues for Research and Policy." *Measurement and Analysis of Crime and Justice* 4: 451–489.

Western, Bruce. 2002. "The Impact of Incarceration on Wage Mobility and Inequality." *American Sociological Review* 67 (4): 526.

Whitworth, Adam. 2012. "Inequality and Crime across England: A Multilevel Modelling Approach." *Social Policy and Society* 11 (1): 27–40.

WID. 2019. "Top Incomes Time-Series." World Inequality Database.

Wilkinson, Richard G., and Kate E. Pickett. 2006. "Income Inequality and Population Health: A Review and Explanation of the Evidence." *Social Science and Medicine* 62 (7): 1768–1784.

Wilkinson, Richard G., and Kate E. Pickett. 2010. *The Spirit Level: Why Equality is Better for Everyone*. London: Penguin.

Wilson, George, and Roger Dunham. 2001. "Race, Class, and Attitudes Toward Crime Control: The Views of the African American Middle Class." *Criminal Justice and Behavior* 28 (3): 259–278.

Yamagishi, Toshio, and Toko Kiyonari. 2000. "The Group as the Container of Generalized Reciprocity." *Social Psychology Quarterly* 63 (2): 116.

Zimmerman, Paul R. 2014. "The Deterrence of Crime Through Private Security Efforts: Theory and Evidence." *International Review of Law and Economics* 37: 66–75.